THE ORIGINS OF STATICS

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PIERRE DUHEM

# THE ORIGINS OF STATICS

## *The Sources of Physical Theory*

*Translated from the French*

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Pierre Duhem at twelve years of age in his first year at Collège Stanislas from:
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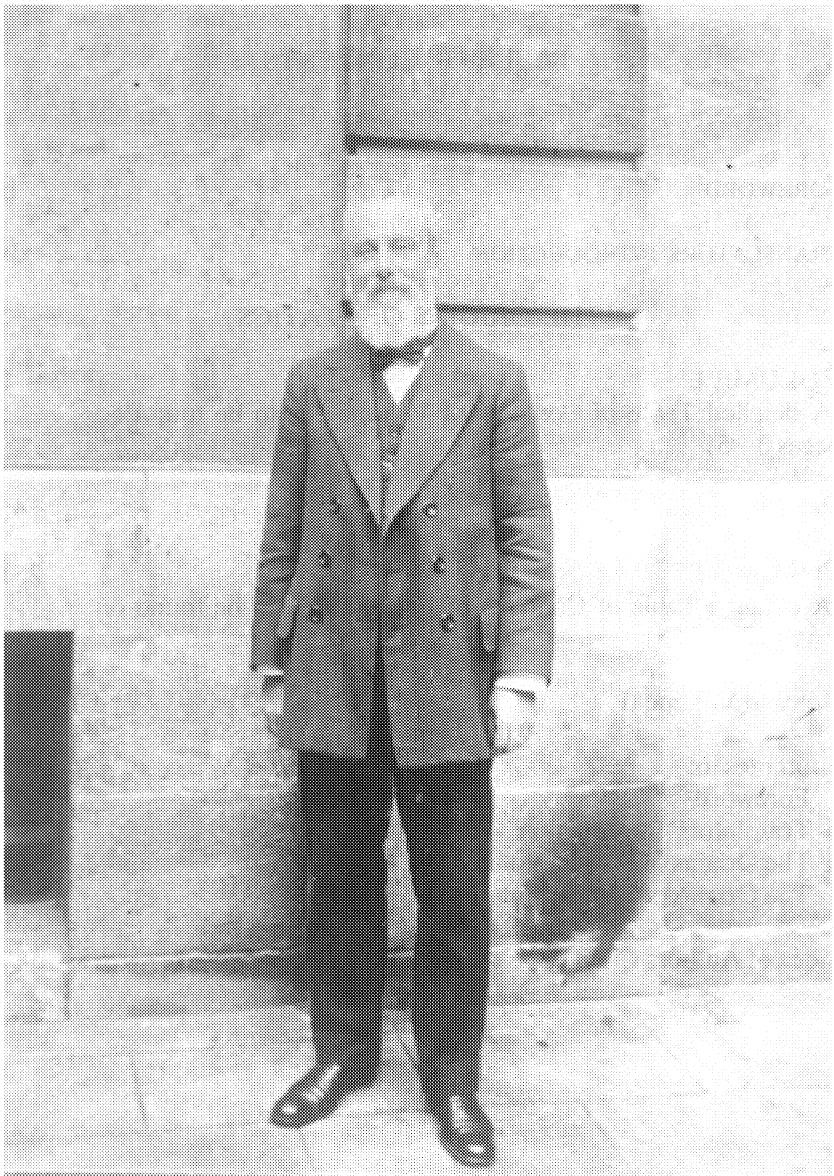


Pierre Duhem (c. 1914; standing in front of the University of Bordeaux)
Courtesy of Marie-Madeleine Gallet.

# FOREWORD

If ever a major study of the history of science should have acted like a sudden revolution it is this book, published in two volumes in 1905 and 1906 under the title, *Les origines de la statique*. Paris, the place of publication, and the Librairie scientifique A. Hermann that brought it out, could seem to be enough of a guarantee to prevent a very different outcome. Without prompting anyone, for some years yet, to follow up the revolutionary vistas which it opened up, *Les origines de la statique* certainly revolutionized Duhem's remaining ten or so years. He became the single-handed discoverer of a vast new land of Western intellectual history. Half a century later it could still be stated about the suddenly proliferating studies in medieval science that they were so many commentaries on Duhem's countless findings and observations.

Of course, in 1906, Paris and the intellectual world in general were mesmerized by Bergson's *Evolution créatrice*, freshly off the press. It was meant to bring about a revolution. Bergson challenged head-on the leading dogma of the times, the idea of mechanistic evolution. He did so by noting, among other things, that to speak of vitalism was at least a roundabout recognition of scientific ignorance about a large number of facts concerning life-processes. He held high the idea of a "*vital impetus* passing through matter," and indeed through all matter or the universe, an *impetus* that could be detected only through intuitive knowledge.

Bergson was fully conscious of the challenge he posed to the rationalist heritage of the French Revolution as hatched by the Enlightenment. He was strangely unaware of a far more reliable and truly epoch-making meaning of the word impetus which was being brought back to light just at the time when the *Evolution créatrice* saw print. Bergson served thereby a proof of being a true child of the Enlightenment. Its champions were chiefly responsible for banishing from intellectual sight the light that invested, half a millennium earlier, the word *impetus* with a meaning that signalled a new epoch in science, in fact, its first genuine epoch.

A pivotal claim of the Enlightenment was dressed in glittering garb when, in the *Evolution créatrice*, Bergson spoke of Galileo's inclined plane as the very instrument on which science descended from heaven to earth. Rarely was a secularist enshrinement more ill-timed. Worse, Bergson might have suspected the irony. He lived in that City of Light where literary and cultural news spread, then as now, with almost the speed of light. Bergson could hardly have failed to learn about the publication of Duhem's *Les origines de la statique* in which the first appearance of a viable science on earth was tied to a means very different from Galileo's inclined plane, whatever its great importance in the rise of modern physics.

Duhem's name was not at all an unknown quantity to Bergson. Duhem is one of the very few modern authors quoted in Chapter 3 of the *Evolution créatrice*. Even more importantly, Bergson, in order to strengthen a principal strategy of his, referred there to Duhem's *Evolution de la mécanique*, published in 1905. The strategy aimed at discrediting the mechanistic world view in which the world of matter, the only world, is a strictly determined machine with a fixed amount of energy. That Duhem had insisted on the various meanings of the word energy as used in physics was seized upon by Bergson as a scientific evidence that "the universe is not made, but is being made continually. It is growing, perhaps indefinitely, by the addition of new worlds." Logic exacted its due when Bergson concluded in 1932 his other widely read work, *Les deux sources de la morale et de la religion*, with the definition of the universe as a "machine for the making of gods."

Duhem could have hardly seen any profit in protesting Bergson's use of his analysis of the notion of energy. There was little if any common ground between Duhem and Bergson, apart from their respective dislike, very different in nature and motivation, of mechanism as an all-purpose explanation. For Bergson the ultimate ground was an evolving universe as a supreme being. The author of *Evolution créatrice* denounced the idea of nothing and therefore had no use for the tenet, so dear to Duhem, of a creation out of nothing and in time. For Bergson, who took conscious time for the primary datum of knowledge, consciousness had to be eternal in a form however impersonal.

By 1906 Duhem had made a very different profession of faith in a classic essay of his, "Physique de croyant."[1] Had he known something of Thomist realism, Duhem would not have misunderstood Abel Rey's charge that his philosophy of physics — being neither mechanistic, nor conceptualist — had to be the physics of a believer. For Rey, a positivist, it was inconceivable that direct knowledge of the reality of plain objects was plain knowledge, fully diffused with reason. Rey called that knowledge an act of faith.

Duhem, partly because of some fideistic touch in the French Catholic thought of his time, failed to see the danger latent in Rey's use of the words, faith and believer, to his own theory of knowledge. Moreover, Duhem had interest in epistemology only inasmuch as he needed it for his theory of physics. Duhem took plain knowledge of plain facts for the very starting point of physics as well as of metaphysics already in 1894 when he began to reflect on the aim and structure of physical theory. But, so Duhem argued, since both physics and metaphysics made different uses of that very same basic knowledge, there could arise no real opposition between the two, let alone between physics and the tenets of Christian faith about supernatural realities and destiny. Such was the gist of Duhem's essay, "Physique de croyant," which, apart from containing Duhem's ringing profession of his Catholic faith, would by its general content have made useless any debate with the author of the *Evolution créatrice*. Much less would have it allowed for any meaningful debate with die-hard mechanists, positivists, and rationalists who set the intellectual tone in Duhem's France.

At any rate, in 1906, Duhem had already been for two years in the grip of unsuspected cultural vistas of which he had caught the first glimpses in the Fall of 1903. Duhem himself tells the story, very briefly, in the introduction he wrote in 1905 to the first volume of this work. His original plan was to write in regular installments the history of statics for the *Revue des questions scientifiques*, the quarterly journal of Catholic scientists with headquarters in Brussels. They counted among their numbers dozens of members of the Académie des Sciences of Paris who found it most difficult to oppose the juggernaut of secularism in their own land.

Duhem conceived the plan of that history as a true child of the second part of the 19th century. There no one would have dreamt that there could be any science to look for between Archimedes and the immediate predecessors of Galileo. But unlike professional historians of the science of mechanics among his contemporaries, Duhem read with the truly meticulous eyes of a scientist the writings of those predecessors of Galileo as he jumped, in telling his story, from Archimedes to the second part of the 16th-century with some references to Leonardo at its beginning. Duhem was in for the greatest surprise of his intellectual life which resulted in a delay of his sending the third installment of his essays.

It was a sign of awareness of Duhem's thorough scholarship that the delay was taken in the editorial offices of the *Revue des questions scientifiques* for an indication that Duhem might be on the track of some important finding. In fact, something of the nature of that finding was correctly guessed by the Père Bosmans, who happened to visit the Père Thirion, editor of the *Revue*, and wanted to see Duhem's latest contribution. "I do not have it," the Père Thirion replied, "Duhem has not finished it yet. He still has lots of reading to do. He promised me further chapters at the rate at which he writes them." "In that case," the Père Bosmans replied, "I would not be surprised if his new readings would not convince Duhem to add complementary chapters to the period," whose history, the Père Bosmans remarked (so it was reported by the Père Thirion a few years after Duhem's death in 1916), related to his own extensive studies of Stevin. The Père Bosmans found that Stevin attributed great importance to Archimedes and Cardan, but ignored Leonardo in whom Duhem saw an important link in the story he was studying. But the Père Bosmans also took the view that "if Stevin underwent Leonardo's influence, he did so in any case only very indirectly. On the other hand I know of two small treatises 'de ponderibus', both attributed to Jordanus de Nemore. Duhem will end by finding them and I would be surprised if he were not to attribute some importance to them."

Jordanus de Nemore was indeed quickly found by Duhem, though not because during the preceding decade or so several historians of mathematics had noticed those treatises. Duhem's lead to Jordanus de Nemore was his critical sense which prevented him from dismissing the charge of Ferrari, a contemporary of Tartaglia, that the latter was a plagiarizer in proposing the law of virtual velocities. Duhem then resolutely followed up some innocuous looking leads which, as he learned to read medieval Latin manuscripts, let him catch a glimpse not only of Jordanus but also of other members of the 14th-century Sorbonne, even more important for the history of science.

Fully aware of the fundamental importance of the law of virtual work in mechanics, Duhem quickly placed the remote origin of Newtonian physics in the Middle Ages. One could therefore only wish that Duhem had taken up Bergson's use of his analysis of the notion of energy. In that case, he could have given a most revolutionary twist to Bergson's reference to Galileo's inclined plane as if that latter had been a secularist Jacob's ladder from the scientific heaven and back. For by 1906, the year which also saw the publication of the first volume of Duhem's Leonardo studies (*Etudes sur Léonard de Vinci: Ceux qu'il a lus et ceux qui l'ont lu*) Duhem had even discovered the *impetus* theory of Buridan. That theory, a substantial anticipation of Newton's first law of inertial motion, was conceived by Buridan in an orthodox theological matrix. Buridan saw the instantaneous beginning of any inertial motion against the broader background of the *impetus* which the celestial bodies had received from the Creator "in the beginning" and is kept by them undiminished because they move in a frictionless space. In a sense, which Bergson and countless contemporaries of his would not have suspected, the science of Copernicus and later of Galileo and Newton was the fruit of contact, through Buridan's vision, with the Heaven of biblical revelation.

In the history of the historiography of science — a history full of amateurism during the 19th century and professionally not robust until the mid-20th century — no discovery has more right to be called revolutionary than the one made and fully elaborated by Duhem. His discovery represented the strongest conceivable challenge to the broader ideology of the French Revolution. Its champions — purely intellectual as well as violently activist — never failed to claim that the Age of Reason, which they fondly equated with the age of Newtonian physics, could not have arisen had the Christian past of Europe, as epitomized in the Middle Ages, not first been thoroughly discredited.

Duhem was fully aware of the impact which that claim exercized on the higher instructional levels. It is still a basic claim of all those, and their number is legion in the academia, who resolutely try to shore up their scepticism, agnosticism, or plain materialism, with untiring references to exact science and its history. By taking a recourse to science, they run counter to the incisive analysis Duhem made of the aim and structure of physical theory. As to the facts of scientific history, it is no less true about them what a Baconian, if not Bacon himself, stated about empirical facts in general: Facts will ultimately prevail and we must be careful not to be found in opposition to them.

Those facts will not go away by the erection of specious stage screens. And most fashionable among them has been the idea of intellectual mutations. Originally a device proposed by Gaston Bachelard, it was made much of by Alexandre Koyré. The essence of that device as articulated by him is not so much to deny the facts discovered by Duhem (although their slighting may be useful), but to claim that they were suddenly seen differently from the early 17th century on. Then the astronomical revolution running from Copernicus to Galileo would be realigned once more with the broader ideology of the French Revolution. This would also secure the role of science as the supreme safeguard of modern scientistic rationalism.

Had Koyré not been blinded by his Boehmean and Spinozean pantheism, he might have perceived that if there was an intellectual mutation in the history of science, it took place at its very medieval birth. Undoubtedly the shift is great from circular inertial motion (in which Galileo still fully believed) to a linear one as proposed by Descartes and later by Newton. Incomparably more fundamental and radical was the shift that involved as its starting and terminal points the following two ideas of motion: One was the idea of non-inertial motion (invariably held in all ancient cultures, including classical and Hellenistic Greece) in which there had to be continuous contact between the mover and the moved. The other rested on the concept of an impetus given in a single instantaneous act with no need for further contact between the mover and the moved. This latter kind of motion, fully inertial (be it still circular), originated, so the great medieval physico-theological breakthrough stated, in an initial impetus tied to the creation of all out of nothing and in time. About this point, amply documented by Duhem, Koyré and his many admirers, pantheists or not, tried to be as taciturn as possible and in the name of scholarship.

In late October 1905, when Duhem wrote the preface to the second volume of this work, he had already on hand substantial evidence about the Christian theological matrix of the birth of modern science. Only with this in mind will one understand the unabashed homage he paid, as a historian of scientific ideas, to the guidance which a truly divine Providence exercises even in scientific history.

These are facts and indeed most crucial facts of intellectual history, represented by Duhem's findings and by their reception, a rather reluctant one, to put it mildly. Those who want, in the name of "objective scholarship" to stay with the less crucial facts of that history, simply honor it in the breach. To recount them here would be superfluous. They can be found in my account[2] of the background, the origin, and reception of this book of Duhem in particular and of his work as a historian of science in general.

Here let emphasis be put once more on the revolutionary character of this work. It is revolutionary not in the trite sense given to that word, a sense unerringly perceived in the French phrase, *plus ça change, plus ça reste la même chose*. Today when an era of drastic political revolutions reveals the triviality of once great revolutionary slogans, that French phrase should show an eery relevance. Academic circles still have to perceive the contradictory character of intricate discourses about scientific revolutions with an overarching structure to them. This they cannot possess if they are truly incommensurable.

With an eye on scientific history, Duhem held high in this book and elsewhere, the idea of a slow and continuous development. He did so in a manner very nuanced from the logical viewpoint and also very graphic at times. The opening paragraphs of the conclusion of the second volume of this book are more than a literary masterpiece. They also witness Duhem's keen observation of nature. The scene, the apparent discontinuity of a river in the Larzac, which he painted with a marvelous choice of words, he also drew as a landscape painter.[3] At any rate, Duhem as a logician had as little use for scientific revolutions as he, as a French patriot, had for the French Revolution. Not that he had not spoken of revolutions in science. But he never meant by them the kind of radical discontinuity which they are meant to convey for most historians and philosophers of science today, invariably forgetful of the duty to give precise definitions of the basic terms they use.

Duhem, who did not consider himself a historian of science, was one of the greatest of them ever. He studied the history of physics only because he wanted to achieve a better grasp of the conceptual foundations of theoretical mechanics. "I am a theoretical physicist and I will return to Paris only as such," he told his daughter around 1903 when some friends of his in Paris tried to obtain for him the chair of the history of science in the Collège de France. By then Duhem had been in exile from Paris for more than a decade and was to stay in exile, that is, in a provincial university (Bordeaux), until his untimely death in 1916 at the age of fifty-six.

The immediate cause of his death was the pain that seized him on hearing a defeatist note as France had just made her heroic stand at Verdun. His heart had for long been taxed by great personal and professional setbacks. His much beloved wife died in the summer of 1892 in trying to give birth to a son who had to be buried with her. During his first ten years in Bordeaux, where he arrived in 1894, after teaching for six years in Lille and one year in Rennes, he had been most unfairly treated by local agents of the Ministry of Public Instruction in Paris that faithfully observed the ukaze Marcelin Berthelot had issued in 1885: "This young man shall never teach in Paris." It was at Berthelot's instigation that Duhem's brilliant doctoral thesis, today a classic, on potential thermodynamics (which refuted a favorite brainchild of Berthelot) was rejected by the Sorbonne on rather flimsy grounds.

By September 14, 1916, when he collapsed, Duhem's energies had been strained to the utmost by what may easily qualify as the greatest individual scholarly effort of modern times. In March 1913 he signed an extraordinary contract with Hermann in Paris. The contract called for the delivery by Duhem during each of the next ten years of about 800 handwritten pages, full of important historic texts to see print for the first time. By early September 1916 Duhem was proofreading the fourth volume of his famous *Système du monde*. The material of the fifth volume had already been sent to the publisher.

Duhem left behind, in fully publishable form, the material for another five volumes. They did not see print until the 1950s. In fact they almost failed to see print at all.[4] That this translation appears in English almost a full century after its publication is part of much the same discouraging story, a story that should make not a few heads hang in shame. Why, one may ask, have professional historians of science not seized long ago the opportunity of making this great book available to the English speaking academic world? Has not that world been all too eager to have many and far less important foreign books available in English?

Tellingly, the translators of this book are not professional historians of science. Indeed, it reflects the universality of Duhem's mind that the three translators were attracted to him from diverse fields of speciality, one a classicist, one a scientist, one a Gallicist. Historians of science stand to them in great debt and should appreciate their thorough competence both in respect to the subject and the fine quality of Duhem's style.

The appearance of this book in English translation almost a hundred years after the publication of the original is a witness to a more encouraging story as well. It is the story of the lasting value of genuine scholarly research and of historical truth. No one had a greater confidence in that story than Duhem did. To his researches in the history of physics one can apply with equal justification his motto, "being eternal, logic can be patient," expressive of his trust in the ultimate victory of a strictly logical physical theory. He used that motto with an eye on the momentary success of partly illogical physical theories. The fashionability of contempt for logic in trend-setting interpretations of the history of science would leave him undisturbed. The wealth of evidence set forth in this book is another proof of a point repeatedly noted about the rise of the idea of progress, be it scientific, and its prospects. If it is to be diffused with genuine confidence, it demands eyes focused on eternity.

STANLEY L. JAKI

# TRANSLATORS' INTRODUCTION

> We might venture the remark that the history of science is science itself. We cannot really know what we possess until we have learned what others have possessed before us.
>
> Goethe, *On the Theory of Color* (1810)

## GENERAL COMMENTS

The completion of this critically annotated translation of Pierre Duhem's now classic depiction of the origins of statics is long overdue in the English speaking world. This delay is largely due to the formidable obstacles presented to any prospective translator by this monumental work. Duhem was himself an exemplar of a now endangered or perhaps already extinct species produced occasionally by the 19th century European educational system and even more rarely by our own, i.e., an extraordinary polymath and polyglot who knew thoroughly the major Western European languages from Classical antiquity to his own time and assumed the same thorough knowledge by his readers. He had a complete grasp not only of the inner logic of the scientific arguments of Western tradition, but also of the grammars those arguments were expressed in. He quotes his sources in the original, be it Classical Greek; Classical, Ecclesiastical, or Renaissance Latin; Early Renaissance French; Italian; German; or even the Flemish of Simon Stevin. In our own age of ever increasing specialization, such universal scholarship is scarcely possible. Thus a triumvirate of collaborators was necessary to produce the present critical translation.

All three collaborators on this translation hold the Ph.D. and teach at the university level. Guy Wagener is a native speaker of French and teaches that language and literature at the University of Nevada, Reno. Grant Leneaux is fluent in French and teaches Classical languages as well as German at the University of Nevada, Reno. Victor Vagliente teaches applied mechanics at San Jose State University and is well-versed in the history of science. By combining the linguistic talents of Wagener and Leneaux with the technical and scientific expertise of

Vagliente, we believe that we have produced a reliable and readable translation of a work of singular importance.

Although we have retained almost all of Duhem's foreign language quotes and footnotes in the original languages, we have translated them all either in the main body of the text or in our own *Translators' Notes.* These *Translators' Notes* also provide valuable critical and interpretive comments and clarifications.

It is our conviction that engineers and technical specialists in our era need a much broader education, including training in the history of their particular discipline and in the humanities. This conviction stems from two very important facts. First of all, no longer can major engineering projects be carried out without considering their economic, social and environmental impact. Many such projects are initiated to solve a particular technical problem but they often create additional non-technical problems. This appears to be due in part to the narrow perspective presented in the typical engineering curriculum by engineering educators who are themselves usually very specialized in training and able to teach only the technical aspects of a subject. In addition, the lack of any historical perspective in many engineering programs contributes further to a very narrow view of an engineering problem. Concepts which took centuries to develop are often presented as barren mathematical formulas. The full understanding of these formulas is impossible without some knowledge of their historical context. For to understand the history of any intellectual field is to see it develop logically and with this understanding the weakness in the theory and the areas where further development is required become obvious. Finally, training in the humanities would produce an integrated engineering effort which would address all aspects of the project.

By translating Pierre Duhem's *Origins of Statics* we support our belief that his contribution to statics has been more that the discovery of a filiation between the various principles of statics reaching back to antiquity. In our opinion, he has presented the origins of statics as an integrated whole and given it a humanistic coloring which makes it an excellent reference for the teaching of statics. His work demonstrates that the major contributors to the theory of statics were not geniuses working alone but researchers working within a tradition. The point has been made many times in the past but it is still relevant: in order to understand any intellectual endeavor in depth, the origins and development of the discipline must be understood.

Since Duhem's effort many research papers have been published on various aspects of the origins of statics. These papers have amplified or in some cases modified in some fashion, the results of Duhem's original research. But for a critical account of the filiation of the fundamental principles from their origins in antiquity to their modern formulations in a language comprehensible to engineers, physicists and historians of science, Duhem's *Origins of Statics* remains the best source available. It must be the starting point for research into the origins of mechanics. Duhem began his research into the origins of statics with the intention of developing a better understanding of physics by writing a history of this subject. He planned on doing the same thing for dynamics, hydrostatics, hydrodynamics, etc. This was a grandiose but not unrealistic plan for he had the language skills and mental acumen to read and understand the ancient manuscripts deposited in National Libraries throughout Europe. Unfortunately, he did not live long enough to carry out his plan. In his *Etudes sur Léonard de Vinci*, Duhem attemped to do for kinematics and dynamics in general what he had done for statics. But he was less successful. In fact, most of the criticism of Duhem's historical enterprise has to do with his presentation of the development of the principles of dynamics in this latter work.

Progress in the sciences takes place gradually and does not come in an instant. For example, the concepts used in the 16th and 17th centuries, namely by Galileo, Descartes, and even Newton, had to be conditioned by the knowledge which existed prior to that time. Duhem firmly believed in this assertion even before he began his research into the origins of statics. As his research progressed, he demonstrated this continuity and filiation which is evident after a chronological perusal of the available sources beginning with the Ancient Greeks through Medieval and Renaissance treatises. This slow evolution came about, he shows, through criticism of older works which made way for the new. In Volume I of the *Origins of Statics*, he shows the relation between ancient and modern statics. It turns out to be far greater than most investigators thought. The details and knowledge are wonderfully condensed in this volume. We would otherwise have to search through many books and journals to obtain as much as we have here. Volume II chronicles the development of the basic principles. It describes the difficulty which most investigators had in accepting the Principle of Virtual Work.

The order of presentation in the *Origins of Statics* can at times cause confusion for the reader. Duhem himself admitted that he wasn't pleased with this aspect of the treatise. But this state developed from the order in which he made his discoveries. Duhem was learning and changing his views as his research progressed. In fact, evidence of this assertion is obvious from Duhem's comment at the beginning of Chapter II, where he says

> The commentaries of the Scholastics dealing with the *Mechanical Problems* of Aristotle added nothing essential to the ideas of the Stagirite.

This statement came before he learned of the contributions of Medieval mechanicians. He will later amend this comment when he discovers the works of Jordanus and Albert of Saxony.

Duhem began the *Origins of Statics* as a series of articles on the origins of statics for the periodical *Revue des questions scientifiques*. The editor, Father Julien Thirion, himself a notable historian of mathematics and professor of physics at the University in Louvain, granted him the freedom he felt he needed to develop his ideas.

He tells us that at the outset he began writing with the accepted view in mind which held that modern science began with Galileo. This is understandable. Duhem relied on the authoritative texts of his time which were Libri, Montucla and Mach. The knowledge accumulated up to this time indicated that the origin of mechanics began with Aristotle and Archimedes, and that Greek knowledge was transmitted by Arab intermediaries to Leonardo and, finally, to Galileo, from whom the modern period is reckoned. But as he continued his research, he found that many of the concepts of mechanics which were transmitted to the Renaissance were too developed in the form in which they were transmitted. He looked for antecedents and so began a study of the sources. This led him to entirely rethink the entire history of mechanics.

The study of history in Duhem's day was much as it is today. The historian paid utmost respect to the facts and viewed the succession of facts as presenting an organic succession. There is nothing new in this. But a vast erudition and mental capacity is required of the investigator who is to fit the known facts into a whole. This is basically Duhem's accomplishment.

The first installment to the periodical covered Aristotle and Archimedes, and Leonardo da Vinci. In this installment, Duhem shows that a link exists between Leonardo and his followers, Cardan and Benedetti, which was not known. There was no indication in the notebooks of Leonardo as edited by Ravaisson-Mollien that such a link existed.

Duhem emphasized the importance of the manuscripts of Leonardo da Vinci because he saw in these manuscripts the convergence of the science of Antiquity and its dissemination to the 16th century. He presents convincing evidence in support of his view. For example, the analysis of various problems by Cardan are almost identical to the analyses of the same problems by Leonardo. Historians have maintained that researchers working independently starting from the same foundation could arrive at the same result. This may be true but the presentation of their results would not be identical. Duhem has shown that in many cases the presentation is identical. This fact makes it difficult to reject Duhem's claim that Leonardo's manuscripts were studied and used during the 16th century.

It is after the first installment had been submitted to the *Revue des questions scientifique* that Duhem "chanced upon a text by Tartaglia." This text proved to be the link between Leonardo and Alexandrian and Greek mechanics.

Tartaglia had a copy of the *Liber Jordani de ratione ponderis* in his possession. This work is modelled on the *Elementa* of Jordanus de Nemore but deletes the three references to Jordanus. It was generally attributed to Jordanus nonetheless. He published this work twice. The first publication was of the theorems without the demonstrations in his *Quesiti*. Later, he bequeathed a copy of this work to Curtius Trojanus, a Venetian publisher, with the charge to publish it. Trojanus did so in 1565, under the title *Jordani Opusculum de ponderositate*. It is this latter publication which prompts Duhem to rethink all that he has done.

Many scholars knew of these works attributed to Jordanus but did not recognize their significance. It is Duhem who recognizes and understands that they represent a link between Greek and Alexandrian sources and the beginnings of modern sicence. Hence, one could assert that modern science has its beginnings in the Middle Ages.

This discovery was truly fortunate. There was an ongoing debate in France during the 19th century over the assertion that Scholasticism smothered empirical research in the Middle Ages. While the discovery of the *Liber Jordani de ratione ponderis* had little to do with empiricism, it demonstrated a very vigorous intellectual activity during the Middle Ages in mechanics. Although Duhem was careful not to allow his personal sentiments to influence his thinking, this discovery was especially satisfying to him.

No one has disagreed with Duhem over the importance of the works attributed to Jordanus, but Duhem's presentation of the chronology of the works has been disputed. Recent scholarship shows in opposition to Duhem that the chronology is as follows.

In 1533, Peter Apian published a text in Nuremberg entitled *Liber Jordani de ponderibus* [1]. This text applies Aristotelian concepts of mechanics to demonstrate the set of theorems which Jordanus also tries to prove in the *Elementa super demonstrationem ponderum*. Duhem characterized this text as a "Peripatetic Commentary" on the *Elementa* of Jordanus and, consequently, as useless in the attempt to understand Jordanus. However, a critical examination of this text indicates that this characterization by Duhem is untenable. In fact, it appears that the *Liber Jordani de ponderibus* preceded the work of Jordanus and that Jordanus was offering a new set of demonstrations for its theorems.

The basis of this claim is as follows: The *Liber Jordani de ponderibus* consists of a Prologue, 7 postulates and 13 theorems. The last 4 of the 13 theorems are from the *De canonio* but their demonstrations are not from this text. Nowhere in the *Liber Jordani* is there any trace of Jordanus. But more important, in the Prologue there is a definition of positional gravity which does not occur in the work of Jordanus. The referenced quote follows [2]:

> Since it is apparent that in the descent [along the arc] there is more impediment acquired, it is clear that gravity is diminished on this account. But because this comes about by reason of the position of heavy bodies, let it be called a positional gravity [i.e. *gravitas secundum situm*] in what follows.

If Jordanus had invented the concept of positional gravity, it is very likely that the definition of the concept would be found in his work. Thus it is very likely that Jordanus in his *Elementa* was commenting on this work.

In addition, Apian gave additional demonstrations for the theorems in the *Liber Jordani*. The first is the actual text and the second he introduced as "another commentary". Duhem thought that the latter demonstrations were an elaboration of Apian on a text known as the *Liber Euclidis de ponderibus*, but it is not from this text at all. In fact, it is a separate commentary written much later in the 14th or 15th century. Its importance lies in the fact that this commentator recognized that the Law of the Lever should rest on the Principle of Virtual Work.

There is a third interpretation by Duhem of the works of this period which has often been criticized. Duhem attributed the authorship of the most important of the medieval manuscripts — the *Liber Jordani de* *ratione ponderis* — to a gifted but unknown disciple of Jordanus whom he called the Precursor of Leonardo da Vinci. His attribution goes against the testimony of the manuscripts which declare *Jordanus* to be the author. However, the *de ratione ponderis* is so superior to the *Elementa* that it is difficult to believe that both treatises issued from a single mind. Not only were the two erroneous theorems on the bent lever replaced with a single correct theorem but the problem of the inclined plane was also solved correctly in the *de ratione ponderis.* Due to the vast difference between the two works attributed to Jordanus, it seems reasonable to accept Duhem's claim that the latter was not by Jordanus but by a gifted disciple whom Duhem called the Precursor of Leonardo da Vinci. Later, Duhem changed his mind somewhat about this claim and coined the appellation of the Precursor to Simon Stevin but retained his assertion that the same author could not have authored the various texts attributed to Jordanus.

## TECHNICAL COMMENTS

There are some technical comments which we would like to make at this point which perhaps might make Duhem's treatise easier to follow. Some are necessary due to omissions by Duhem and others are the result of recent scholarship.

Throughout the *Origins of Statics*, Duhem emphasizes the development of the Principle of Virtual Work and rightly so. This principle encompasses the summation of forces and moments as the criteria for equilibrium and has proved to be very useful to research in all fields of physical science. If for an arbitrary set of infinitesimal displacements from the equilibrium configuration, a compatible set of displacements are found and the virtual work calculated the displacements will cancel out and the remaining equations will be the equilibrium equations, i.e. the forces and moments summing to zero. This happens since force and moment are proportional to work, i.e. the force times the collinear displacement or moment times angular rotation represent work. The advantage of working with the quantity or concept of work is that now we are working with a scalar quantity and do not have to concern ourselves with direction as well as magnitude as when working with vector quantities such as force and moment.

In order to apply the concept of work to problems of equilibrium, it must be treated differently than if force and moment equilibrium were calculated directly. To use work we must depart from the equilibrium state by imagining a displacement. It does not have to be the actual displacement but it must be compatible with the constraints. With force and moment equilibrium, we use the actual state of equilibrium to write our equations.

The utility of this fact cannot be overestimated. If we are dealing with scalar quantities, ordinary algebra permits us to formulate the equations of equilibrium. Although Duhem considered in his investigation only rigid bodies, the Principle of Virtual Work has further applications when dealing with solid bodies and considering their equilibrum and deformation. In this latter case, the equilibrium equations would include internal as well as external forces and moments.

In Chapter I of the *Origins of Statics*, we see the contributions of Aristotle and Archimedes. Every history of mechanics starts at this point. Although there were undoubtedly precursors to these two investigators we have no record of their contributions. The two main streams of statics issue from Aristotle and Archimedes — the former a dynamic approach and the latter a static one.

We need to say more about Aristotle's approach. Although it is not explicitly stated in the *Mechanical Problems*, the general and usual interpretation of this approach by commentators was that a *potential* velocity had to be imagined imposed on the system from the equilibrium position. Since machines are considered here, the *potential* velocity is compatible with the constraints imposed on the system.

It is this imagined or virtual velocity from the equilibrium state which will cause a great deal of difficulty later. The objection will be that there is motion where there should be no motion. That is, in a statics problem, nothing should be moving.

The origin of this concept was not considered by Duhem, but since it helps understand the objection made by many investigators later on, we should mention it. The concept has a metaphysical origin and can be found in the philosophy of Aristotle.

Aristotle saw motion everywhere. Animate and inanimate bodies possessed motion and all motion was to be treated by mechanics. For example, the acorn is potentially on oak. The acorn growing to become an oak represents motion because motion fulfills what exists potentially. Likewise, birth, life and death represent motion. Thus Aristotle viewed motion as a general concept, even change of quality or of the size of an object comprised motion. He went on to say that some bodies move by an internal cause such as animals or human beings while others are moved by external forces.

If we consider an inanimate body in equilibrium it can be considered to have various "potential" states which it could realize by motion. This notion appears to be the root of the notion of a "virtual" displacement or velocity. The Greek word for *potential* is translated as *virtus* in Latin. From the Latin word, it became *virtuelle* in French and finally, *virtual* in English [3]. The scientific meaning attributed to the term *virtual displacement* today is a displacement in essence but not in actual fact.

In the *Mechanical Problems*, it is never stated that the application of the given principle is to be made in conjunction with a virtual displacement or velocity from the equilibrium configuration. However, it is clearly implied in the development and later commentators understood it this way.

To the ancient Greeks, circular motion is the primary type. It is from the example of the planets in the heavens that it appears to be continuous and eternal. Thus in the *Mechanical Problems* the circular arcs made by the lever while rotating about the fulcrum are viewed as significant and with the predilection of the Greeks to geometrize they become part of the explanation. In modern terms, we can see that the problem can be understood as follows:

For Aristotle, force is proportional to velocity, i.e. $F = k \cdot v$ where $F$ is the force, $k$ is the constant of proportionality and $v$ the velocity.

And as Duhem points out in Note A of Volume II, for Aristotle there is a conservation of the quantity

$$\frac{\textit{Force} \times \textit{Distance}}{\textit{Time}} \quad \text{or} \quad \frac{\textit{Weight} \times \textit{Distance}}{\textit{Time}}$$

Thus if we examine the equilibrium of the straight lever, we have the following: Imagine the lever of Fig. 1 at rest with the force $F_2$ greater than the force $F_1$ causing the lever to incline in the direction of the force $F_2$.

The task is to examine the quantity, (*Force* × *Distance*)/*Time* for both the applied and resisting load. If the quantities are equal, equilibrium holds, but if they are not equal then motion ensues in the direction of the greater quantity.

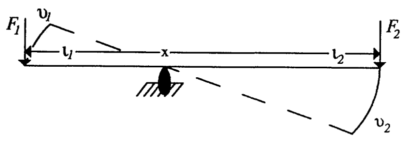



*fig. 1.*

This formulation explains two omissions which cause confusion in the modern reader. At the outset, it is unclear why an arcal trajectory is considered and whether the applied force follows the trajectory or remains vertical during the motion. Furthermore, no mention is made of the magnitude of the displacement. But if a conservation of weight momentum is understood then the arcal displacement is natural and there is no question over the direction of the force or the magnitude of the displacement. Aristotle believed that a motor force is in contact with the body and follows the body. Consequently, the force should follow the body along the arcal trajectory.

In order to apply Aristotle's principle to the equilibrium of the lever, imagine a potential, or in modern terms a virtual motion from the position of equilibrium. Since velocity is distance ($D_i$) divided by time ($T_i$), Aristotle's principle which is written

$$F_1 \cdot v_1 = F_2 \cdot v_2 \tag{1}$$

becomes

$$\frac{F_1 \cdot D_1}{T_1} = \frac{F_2 \cdot D_2}{T_2} \tag{2}$$

and since $T_1 = T_2$

$$F_1 \cdot D_1 = F_2 \cdot D_2 \tag{3}$$

If the equality of Equation (1) is satisfied, then there is equilibrium and if it does not hold, the lever inclines to the side with the largest product.

Now it can be seen from Equation (2) that this is basically the quantity work ($F_i \cdot D_i$) divided by time and since $T_1 = T_2$, it can be cancelled leaving Equation (3) which states the equality of work for both forces.

If in place of the velocities in Equation (1) one writes the respective angular velocities $\dot{\theta}$ multiplied by their distance from the fulcrum, i.e.

$$v_1 = l_1 \cdot \dot{\theta}_1$$
$$v_2 = l_2 \cdot \dot{\theta}_2$$

there results

$$F_1 \cdot l_1 \cdot \dot{\theta}_1 = F_2 \cdot l_2 \cdot \dot{\theta}_2 \tag{4}$$

and since $\dot{\theta}_1 = \dot{\theta}_2$, the expression can be simplified by cancelling $\dot{\theta}_1$ and $\dot{\theta}_2$ to obtain

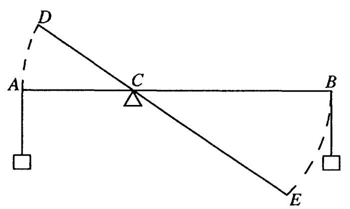
$$F_1 \cdot l_1 = F_2 \cdot l_2 \tag{5}$$

This last equation requires that the sum of the moments about the fulcrum be zero. Aristotle does not fully grasp the concept of moment although he has it vaguely from his consideration of stability. Consequently, he could not have seen this result. In addition, it must not have been obvious to later investigators who were aware of the Archimedean formulation of equilibrium that this relation existed. At least, there is no evidence to suggest such a realization existed, but it represents the link between Aristotelian and Archimedean approaches to statics.

It is clear that the Principle of Virtual Velocities provides the solution to problems of constrained motion. For example, the equilibrium of the lever or winch can be evaluated using this principle. The efforts to extend it to other problems led to its modification. The first modification was to replace the notion of a virtual displacement from equilibrium with a velocity or displacement caused by an insensible weight or force.

Galileo's interest in ridding science of metaphysical arguments led him to interpret the virtual velocity in the following fashion:

Accordingly, consider the balance *AB* (see the figure below)




divided into unequal parts in point C and weights suspended at *A* and *B* which are inversely proportional to distances *CA* and *BC*. It is already evident how the one [weight] balances the other, and consequently, how, if to one of these [weights] there were added a minimal moment of gravity, it would descend and lift the other. Thus if we add an *insensible weight* to heavy body *B*, the balance will move, with point *B* descending to *E* and the other extremity *A* ascending to *D*. And, since to make the weight *B* descend any minimal increase in gravity to it is sufficient, hence we shall not take into account this insensible amount, and so will not make any distinction between the power to sustain a weight and the power to move it. Now consider the motion which the heavy body *B* has in descending to *E* and which the other body *A* has in ascending to *D*. And we shall find without any doubt that by the amount that space *BE* exceeds space *AD*, distance *BC* exceeds *CA*, because there are formed at center *C* two opposite and thus equal angles *DCA* and *ECB*, and consequently the two arcs *BE* and *AD* are similar and have the same proportion between themselves as the radii *BC* and *CA* by means of which they are described. It then follows that the velocity of the motion of heavy body *B* in descending surpasses the velocity of the other moving body *A* is ascending by the amount that the gravity of the latter exceeds the gravity of the former. Since the weight *A* can only be lifted (although slowly) to *D* if the other heavy body *B* is moved rapidly to *E*, it is not surprising nor unnatural that the velocity of the motion of the heavy body *B* compensates for the greater resistance of the weight *A*, as long as it (*A*) is moved slowly to *D* and the other (*B* descends rapidly to *E*. And so, contrariwise if we place the heavy body *A* in point *D* and the other body in point *E*, it will not be unreasonable that the former (*D*) by falling slowly to *A* can quickly lift the latter (*E*) to *B*, restoring with its gravity that which it comes to lose as the result of the slowness of [its] movement. And from this discourse we can recognize how the velocity of motion is able to increase the moment in the mobile by the same proportion as the velocity of motion itself is increased [4].

Thus Galileo recognizes that if a system is in equilibrium any small force will cause it to move. This is the *insensible* weight mentioned in the quote. If the insensible weight is very small and the virtual displacement small, the product of the two will be even smaller and consequently, can be neglected.

It is not until the concept of *work* is recognized that the idea of a virtual displacement from the equilibrium state can be accepted. In conjunction with the former concept it can be applied to determine equilibrium. The first recognition of the importance of *work* is due to Jordanus de Nemore. We can only speculate on what brought Jordanus to comment on the *Liber Jordani* and to replace velocity with displacement. Jordanus is an algebraist and mathematician and the basic problems of mechanics lend themselves to algebraic analysis. Having seen the *Liber Jordani* he may have simply decided to supply demonstrations.

Jordanus tries to justify in the *Elementa* the axioms used by Aristotle to prove the law of the lever. But to do this, he invokes a completely new axiom.

Whatever can lift a given weight to a given height can also lift a weight $K$ times heavier to a height $K$ times smaller.

This theorem is also used in the *de ratione ponderis* to demonstrate the mechanics of the bent lever and the inclined plane.

There is no trace of this axiom in the works known to Jordanus. Hero of Alexandria had used the concept of work in his treatises on mechanics but these were unknown in the Middle Ages. By its extreme generality, it represents a clear improvement over the theory professed by Greek and Arab mechanicians. Descartes will recognize this generality and will propose to use it as a foundation for statics.

Duhem was criticised for the amount of time he devoted to the theory of the shape of the earth. Critics claimed that the investigation of the shape of the earth belongs to geostatics and not to statics. However, this criticism overlooks two important developments from this controversy which are extremely important for statics:

(1) Newton's Theory of Gravitation is founded on the fact that the earth is esentially round and that a point can be found at its center called the center of gravity at which the entire mass of the earth can be assumed concentrated. This makes it possible to treat the earth as a point in many problems and greatly simplifies the calculations.
(2) With the Principle of Virtual Work and a well-defined concept of the center of gravity, the stability of a rigid system could be analyzed and understood.

The first development is perfectly clear. With this theory, Newton was able to solve a problem which had confounded and thwarted the efforts of numerous researchers for centuries, the motion of the planets in our solar system. The second development requires further clarification.

Galileo and later Torricelli noted that for a structure to move, positive work in the earth's gravitational field had to be done. For a system, this means that the center of gravity of the system has to descend. When the system's center of gravity has reached its lowest point, equilibrium prevails. This also means that it was necessary to understand what the term center of gravity implied for the earth and for a system.

This contribution is a result of Galileo's experimental studies on system equilibrium. This is the observation that system equilibrium could be viewed from either force equilibrium or from the notion that once equilibrium had been achieved, the center of gravity of the system could not descend further. The two viewpoints are equivalent but the second is more fundamental in the sense that the second principle encompasses the first. Galileo's approach to the problem follows.

Consider as an illustration of the second viewpoint, the system of Fig. 2. Friction is absent. Consequently, the two blocks of weight $P$ and $Q$ are free to move. A pulley at point $b$ permits the two weights to move as a unit since they are joined together by a string. The position of the center of gravity, denoted by $\bar{x}$, can be found by summing moments about the line *ac*.

$$\bar{x} = \frac{P \cdot x_1 + Q \cdot x_2}{P + Q} \tag{6}$$

Now, from this equilibrium position, imagine a small displacement to take place, which is denoted by the letter $h$ and is given to the weight $Q$. If $Q$ falls by the amount $h$, the weight $P$ will rise by the amount $h \cdot sin\alpha$. This a result of the geometry of the system which constrains the weight $P$ to move on the inclined surface *ab*. It is determined experimentally that $\bar{x}$ will remain the same or increase in magnitude, where the latter corresponds to the center of gravity rising. If $h$ is very small, the magnitude of $\bar{x}$ will remain the same. Then,

$$\bar{x} = \frac{P\,(x_1 + \mathrm{h} \cdot \sin\,\alpha) + Q\,(x_2 - h)}{P + Q} \tag{7}$$

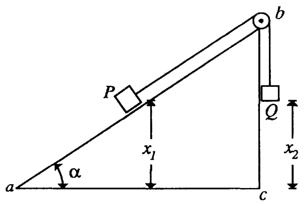



*fig. 2.*

If we subtract Equation (6) from Equation (7), one obtains

$$\frac{P \cdot h \cdot \sin \alpha - Q \cdot h}{P + Q} = 0 \tag{8}$$

or after simplification

$$P \cdot \sin \alpha = Q$$

This latter equation is, of course, the equilibrium equation for the system.

Now, this principle can be generalized and then applied to continuous as well as discrete systems. The actual formulation of this development is by Torricelli and is known as Torricelli's Principle.

In statics, the stability of a rigid system can be defined in terms of the center of gravity. If we generalize Torricelli's Principle to read:

(1) If an infinitely small displacement is applied to a system and the center of gravity rises, the structure is in equilibrium and the state of equilibrium is stable. If the center of gravity does not rise, but remains on the same level during a finite displacement, then the structure is in equilibrium but the state of equilibrium is stable or indifferent.
(2) If for an infinitely small displacement from the equilibrium configuration the center of gravity falls, the structure is unstable and by having the center of gravity fall, it will move towards the equilibrium position.

The salient feature of this formulation is that one needs to consider only one point in the system to understand whether its equilibrium is stable or not, i.e. the center of gravity.

It is obvious that to determine whether a system is in stable equilibrium we have to find where the center of gravity is located, then imagine a displacement imposed on the structure and determine whether the center of gravity rises, remains on the same level or falls in order to ascertain whether it is in stable, indifferent or unstable equilibrium. Of course, the criteria here is applicable only to a rigid body system. If the system deforms, a different and more general formulation of this concept is necessary.

Since we have completely defined the stability of the rigid system in terms of a single quantity, the center of gravity, it is only natural that we use the Principle of Virtual Work in conjunction with the concept of the center of gravity to define what we mean by stability. If we impose a small displacement on the system in harmony with the constraints and calculate the virtual work we can say the following:

(1) If the center of gravity rises or remains at the same level during a virtual displacement the virtual work will be negative or have a zero value, implying that the system is in stable or indifferent equilibrium.
(2) If the center of gravity falls during a virtual displacement, the virtual work will be positive, implying that the equilibrium is unstable.

Here stability is defined in a narrow sense as being a configuration which is near to what is conceived as the equilibrium state.

Galileo habitually thought of the virtual displacement as finite rather than the infinitesimally small displacement used today. The magnitude of the virtual displacement is immaterial if the system is in a state of indifferent equilibrium such as the problem of the straight lever. Also, Galileo's conception is due primarily to the time in which he was working where an analysis of an infinitesimal quantity was not yet possible. Today, we recognize that the virtual displacement should be infinitesimally small so that we examine the nature of equilibrium by comparing it to the equilibrium states which are infinitesimally close and so that changes in the geometry of the system causing the directions of the applied forces to change can be omitted from the problem.

It is unfortunate that Duhem concludes his investigation of the Principle of Virtual Work with the letter written by Jean Bernoulli to Varignon in 1717 [5]. There is a much better presentation of this principle in J. L. Lagrange's *Mécanique analytique*. Lagrange's formulation de-emphasizes the virtual displacement and emphasizes the concept of work. He reverts to the origin of the Principle of Virtual Work by considering the early investigations of the principles behind the operation of machines. He introduces what he calls the Principle of Pulleys and states that this device has been in use for centuries and that it is clear from its operation that *work*, i.e. force × collinear displacement, is the quantity which determines the state of equilibrium. However, this is not so obvious. Early mechanicians had studied this device and formulated an adage to work by:

What is gained in force is lost in velocity.

This adage seemed to agree well with Aristotle's law that force is proportional to velocity. Lagrange's assertion that work is the important quantity is the result of the efforts of many investigators over a long period of time.

Hence, when a system reaches equilibrium, all the positive work that could be done has been done. This is tantamount to Torricelli's Principle that at equilibrium the center of gravity of the system has reached its lowest point. Now if the system is imagined to displace from the equilibrium configuration, the work of all the impressed and resisting forces must be zero since the center of gravity can descend no further. Although the center of gravity can descend no further, it could possibly rise so that negative work could be done. The displacement is the virtual displacement which must conform to the constraints on the problem. It is obvious that the constraints on the problem are the geometric conditions which define the state of equilibrium.

Forces acting oppositely to the direction of the displacement will contribute negative work and those forces acting in the direction of the displacement contribute positive work. It is this algebraic sum which is zero. Thus Lagrange's formulation of the Principle of Virtual Work makes use of its full historical development.

*May 1990*

GRANT LENEAUX
VICTOR VAGLIENTE
GUY WAGENER

THE SOURCES OF PHYSICAL THEORY

# THE
# ORIGINS OF STATICS

Volume 1

by
P. Duhem

Member of the Institute of France
Professor in the Faculty of Sciences at Bordeaux

1905

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# PREFACE

In this work the reader will not find the order which he undoubtedly would have preferred to find and which we assuredly would have preferred to follow. Therefore, we owe an explanation at the outset to the perplexed reader concerning our singular way of proceeding, in which our arguments might seem at times repetitive.

Before we embarked on the study of the origins of statics, we read the works — few in number — which deal with the history of this science. It was easy for us to see that most of these works were rather superficial and contained few details, but we had no reason to believe that this information was in any basic way incorrect. In resuming the study of the original texts mentioned in these works, we foresaw that we would have to add or modify quite a few details, but little did we know that our research would lead to a complete rethinking of the entire history of statics.

At the very outset, this research led us to make some unforeseen observations. It proved to us that the works of Leonardo da Vinci, so rich in new ideas on mechanics, had in no way remained unknown to the mechanicians of the Renaissance, as was commonly assumed. It further proved that his works were used by many scientists of the 16th century, in particular, by Cardan and Benedetti, and that they furnished Cardan with his profound insights on the operation of machines and on the impossibility of perpetual motion. However, after Leonardo and Cardan and up to Descartes and Torricelli, it seemed to us that statics developed along a path essentially in accordance with the commonly held views.

We had already commenced retracing this development in the *Revue des Questions Scientifiques*, when we chanced upon a text by Tartaglia, nowhere mentioned in any history of statics, which proved to us that what we had done so far had to be rethought on an entirely different level.

Indeed, it was Tartaglia, who, long before Stevin and Galileo, had determined the apparent weight of a body on an inclined plane. He had very correctly deduced this law from a principle which Descartes was later to affirm in its complete generality. But this magnificent discovery, which no historian of mechanics mentions, did not come from Tartaglia. It was nothing but an impudent act of plagiarism on his part, and Ferrari bitterly reproached him for it and gave credit for this discovery to a 13th century mechanician, Jordanus Nemorarius.

Two treatises had been published in the 16th century expounding the statics of Jordanus. However, these treatises were so dissimilar and sometimes contradicted each other so explicitly that they could not possibly be the work of the same author. In other to determine precisely what mechanics owed to Jordanus and his students, we had to go back to the contemporary sources, to the manuscripts.

Thus we were forced to go through all of the manuscripts dealing with statics which we were able to find at the Bibliothèque Nationale and at the Bibliothèque Mazarine. This laborious work, in which we were very competently assisted by M. E. Bouvy, librarian at the University of Bordeaux, led to totally unforeseen conclusions.

Not only did the Occidental Middle Ages directly, or indirectly through Arab intermediaries, inherit the tradition of certain Hellenic theories concerning the lever and the Roman balance, but through its own intellectual activity gave birth to a statics autonomous from and unknown in Antiquity. As early as the beginnings of the 13th century, and perhaps even earlier, Jordanus de Nemore had demonstrated the law of the lever by proceeding from the following postulate: the same work is needed to lift different weights when the weights are in inverse proportion to the heights which they travel through.

This idea, which can be found in germinal form in the treatise of Jordanus, was progressively developed in the works of his followers up to Leonardo da Vinci, Cardan, Roberval, Descartes, and Wallis, and reached its final formulation in the letter which Jean Bernoulli sent to Varignon, as well as in the *Mécanique analytique* of Lagrange and in the works of Willard Gibbs. Thus the science which we are legitimately so proud of today grew out of a science born around the year 1200 through an evolution whose successive phases it was our task to describe.

However, it is not only through the doctrines of the School of Jordanus that the mechanics of the Middle Ages contributed to the foundation of modern mechanics. In the middle of the 14th century, Albert of Saxony, one of the doctors who brought great honour to the brilliant Nominalist School at the Sorbonne, formulated a theory on the center of gravity which was to gain great recognition and have lasting influence. Shamelessly plagiarized during the 15th and 16th centuries by a great number of mechanicians and physicists who used his theory without naming its author, the theory continued to flourish during the 17th century. Indeed, to anyone ignorant of this fact, many a scientific controversy so hotly debated at that time would remain incomprehensible. Through an uninterrupted filiation, the principle of statics proclaimed by Torricelli developed from this theory of Albert of Saxony.

Thus the study of the origins of statics led us to a conclusion which became overwhelmingly evident when more varied avenues opened as we looked back in time. Therefore, only now do we dare formulate this conclusion in its full generality: the mechanical and physical science of which the present day is so proud comes to us through an uninterrupted sequence of almost imperceptible refinements from the doctrines professed within the Schools of the Middle Ages. The so-called intellectual revolutions consisted, in most cases, of nothing but an evolution developing over long periods of time. The so-called Renaissances were frequently nothing but unjust and sterile reactions. Finally, respect for tradition is an essential condition for all scientific progress.

*Bordeaux, March 21, 1905* P. DUHEM

## CHAPTER I

# ARISTOTLE AND ARCHIMEDES (384—322 and 287—212 B.C)

Although we are left with few monuments from the profound research of the Ancients into the laws of equilibrium, those few are worthy of eternal admiration. Two texts in particular stand out because they are undoubtedly the most admirable: the book which Aristotle devotes to questions of mechanics and the treatises written by Archimedes.

The "Treatise on Statics," in which Aristotle examines the different questions concerning mechanisms — the *Mechanical Problems*[1] — is probably misnamed. Indeed, the Stagirite does not separate the theory of equilibrium from the theory of motion. He does not ascribe to the first theory its own autonomous principles totally unrelated to the second theory. He discusses in a general fashion the movements which can be produced in a mechanism. When no motion is produced, the mechanism remains in equilibrium.

The axiom which furnishes the solution to diverse mechanical problems is the fundamental law which Aristotle assigns to local motion and which, be it explicit or hidden, dominates everything he wrote about such motion. The work of the force moving a body is measured by the product of the weight of the body moved (or by its mass, because the two notions of weight and mass were at that time indistinguishable from each other), and by the velocity of the motion impressed on this body. One and the same force can, therefore, move successively a heavy body and a light body, but that force will move the heavy body slowly and move the light body quickly. The velocities of the motions imparted to these two bodies will be inversely proportional to their weights.

One can find this thought expressed in many passages. The following quote suffices because of its extreme clarity.[2]

> Whatever the force may be producing the motion, that which is smaller and lighter receives more motion from the same force . . . indeed, the velocity of the lighter body will be to the velocity of the heavier body as the heavier body is to the lighter body.

It seems that this fundamental principle of Peripatetic dynamics is the faithful and instinctive reproduction of the obvious data of daily experience. However, modern dynamics repudiates this as a major error. Yet, in order to refute this error, science will require two thousand years of reflection by the greatest minds after Aristotle up to Galileo. Someday we will undertake to retrace the main phases of this gigantic intellectual enterprise. But for now we will attempt to forget everything which modern mechanics has taught us in order to fully understand the laws acknowledged by Peripatetic mechanics. Only under this condition will we be able to understand the thoughts of the mechanicians who, century after century, contributed to the progress of statics. Thus two forces will be considered equivalent if, when moving unequal weights at unequal velocities, they cause the product of the weight times the velocity to have the same value.

Thus let us imagine a rectilinear lever divided into two unequal lengths at a point of support and at the extremities of which are suspended two weights of unequal mass. When the lever rotates about its point of support, the two weights move with different velocities. The weight furthest from the point of support describes, during the same interval, a larger arc than the weight closest to the point of support. The velocities of the two weights are in the same ratio as the lengths of the arms from which the two weights are suspended.

Thus, whenever we would like to compare the effect of these two weights, we will have to compute for each one of them the product of the weight times the length of the arm of the lever. The one with the greatest product will descend, but if the two products are equal, the two weights will remain in equilibrium.[3]

The weight which is moved, says Aristotle, is to the weight which moves in inverse ratio to the lengths of the arms of the lever. Indeed, a weight will always move all the more easily, the further away it is from the point of support. We have already mentioned the cause: the weight which is furthest from the point of support describes a larger circle. Thus, while using the same force, the weight will describe a greater path, the further it is from the point of support.

These considerations, developed with respect to the lever, are not simply based on observations which hold for only this one case. They constitute a general method and they contain a principle which can be applied to nearly all mechanisms. Thanks to this principle, mechanicians will be able to account for the various effects produced by these diverse machines simply by considering the velocities with which certain arcs of a circle are described.

For the properties of the balance[4] are deducible from those of the circle and the properties of the lever are deducible from those of the balance. In summary, most of the other unique properties exhibited by the motion of mechanisms are deducible from the properties of the lever.

Had Aristotle formulated only this single idea, he would deservedly have to be celebrated as the father of rational mechanics. This idea is, indeed, the seed from which the powerful ramifications of the Principle of Virtual Velocities[5] will sprout over the next twenty centuries.

Aristotle was not a mechanician and consequently was unable to rigorously develop from the Principle he had posited all of the consequences which could be deduced from it. In other instances, he thought it possible to apply the Principle to problems so complex that they actually far exceeded the means by which he proposed to solve them. At any rate, Aristotle ran into serious difficulties at the very beginning of his research. The line described by the displacement of the lever at the point of application of the force or of the resistance is along the circumference of a circle and does not coincide with the vertical line along which the force or the resistance acts. Aristotle merely made some rather obscure observations[6] concerning this difficulty, more suited to elicit remarks by his commentators than to satisfy the mechanicians.

Mechanicians like to see a long chain of reasoning unfold according to a perfect order faultlessly tying together some very simple and clear principles with complicated and remote conclusions. No other work is more apt to satisfy this need of the mechanician for rigor than the works in which Archimedes deals with mechanics.

These works are comprised of the *Treatise on the Equilibrium of Planes or their Centers of Gravity* and *the Treatise on Floating Bodies*. It is not our intention here to study the origins of hydrostatics. Consequently, we will disregard the *Treatise on Floating Bodies* in order to concentrate our attention on the other treatise.

Archimedes sets out to exclude from the foundations upon which he will construct his doctrine any proposition whose soundness appears doubtful. Unlike Aristotle, he will not seek his fundamental hypothesis within the science of motion, because the laws which apparently govern the motions of heavy bodies seem profoundly hidden beneath complex appearances and because the analysis of these phenomena — so varied and so difficult to observe precisely — seems ill-suited for furnishing propositions upon which everyone would readily agree. On the contrary, the everyday use of simple instruments, such as the balance for example, reveals as far as the equilibrium of weights is concerned, certain rules whose validity and generality are beyond any doubt. Following the method which his master had used in the *Elements*, Archimedes demands from those intent on following his teachings that they accept as true those few postulates from which he is going to deduce his theory. Some of these postulates follow:[7]

(1) Equal weights suspended at equal distances are in equilibrium.
(2) Equal weights suspended at unequal distances are not in equilibrium; the one suspended at the greatest distance moves downward.
(3) If weights suspended at given distances are in equilibrium and if one adds something to one of the weights, they will no longer be in equilibrium, and the weight to which something has been added will move downward.
(4) Similarly, if one removes something from one of these weights, they will no longer be in equilibrium, and the one from which nothing has been removed will move downward.

Using a method which originated with Euclid, Archimedes is able to deduce a long series of propositions from these postulates and others so obvious that it is unnecessary to list them here. Of these propositions, let us quote only the sixth and seventh,[8] which define the conditions of equilibrium for a straight lever. These propositions are as follows:

*Proposition VI.* Magnitudes[9] commensurable to each other are in equilibrium when they are reciprocally proportional to the distances at which they are suspended.
*Proposition VII.* Magnitudes which are incommensurable are in equilibrium when they are reciprocally proportional to the distances at which they are suspended.

These two propositions contain the specifically mechanical consequences of the works of Archimedes. The theorems which derive from them and by which the illustrious Syracusan determines the centers of gravity for numerous figures will attract the attention of the geometer, who will admire their elegance and ingenuity. The algebraist will discover in them the first integration ever made. For the mechanician, however, they do not reveal any novel insight into questions of importance to him.

By studying the equilibrium of weights, Archimedes comes to the same conclusion as Aristotle, but by a completely different path. He does not deduce his principles from general laws of motion. Instead, he builds the edifice of his theory on a few simple and dependable laws relative to equilibrium. Thus he founded the science of equilibrium as an autonomous science which owes nothing to the other branches of physics. In a word, he founded statics.

In so doing, he gave his doctrine perfect clarity and extreme rigor. However, one must admit that such clarity and rigor were purchased at the price of generality and fecundity. The laws defining the equilibrium of two weights suspended from the arms of a lever were taken from hypotheses unique to this problem. However, when the mechanician is faced with any other problem concerning equilibrium distinct from the one mentioned above, he is forced to resort to new hypotheses quite different from the first ones because the analysis of the first set of hypotheses will give him no direction when trying to choose a second set. Thus, when Archimedes wants to study the equilibrium of floating bodies, he will have to resort to principles which have no analogy to the postulates which he formulated at the beginning of the *Treatise on the Equilibrium of Planes*.

Even though it is an admirable method of demonstration, the path followed by Archimedes in mechanics is not a method of discovery. The certainty and clarity of Archimedes' principles are due in large part to the fact that they were gathered, so to speak, from the surface of phenomena and not derived from the very roots of things. In accordance with a comment which Descartes[10] less justly applied to Galileo, Archimedes explains quite well "quod ita sit" but not "cur ita sit."[11] Thus we will see that the most important progress in statics will derive from Aristotle's doctrine rather than from the theories formulated by Archimedes.

CHAPTER II

# LEONARDO DA VINCI (1452–1519)

The commentaries of the Scholastics dealing with the *Mechanical Problems* of Aristotle added nothing essential to the ideas of the Stagirite. To see these ideas develop new branches and bear new fruits, one must wait for the beginning of the 16th century.

If, in confronting those colossal men who appear at the beginning of the 16th century,[1] one dares show a preference, perhaps the laurel should be accorded to Leonardo da Vinci, that sublime genius who enlarged the entire range of human knowledge. In the arts, Michelangelo and Raphael could not eclipse his glory. His scientific discoveries and his philosophical research place him at the head of the learned men of his epoch. Music, military science, mechanics, hydraulics, astronomy, geometry, physics, natural history, anatomy, were all perfected by him. If all his manuscripts still existed, they would form the most original and vast encyclopedia ever created by human intelligence.

In his lifetime, Leonardo da Vinci published nothing. Various witnesses assure us that at his death he left in manuscript form certain completed treatises, notably, one on painting and one on perspective. But these works did not come down to us. The *Tratato della pittura*,[2] published in Paris by Dufresne in 1651 and often reedited afterwards, and the *Trattato del moto e misura dell'acqua*,[3] printed at Bologna in 1828, are edited versions more or less faithful to the original. The true thought of Leonardo must be sought in the notebooks, where he wrote down his thoughts as they evolved.

Many of these notebooks have been lost. However, after many peregrinations, a number of them were saved.[4] An important collection of these writings is in the library of the Institut de France. Various pages, stolen by Libri and sold by him to Lord Ashburnam, became, thanks to Mr. Leopold Delisle, the property of the Bibliothèque Nationale. Other manuscripts can be found in Italy. Among these, of particular importance is the register which is kept in the Biblioteca Ambrosiana of Milan, the Codex Atlanticus.

Under the auspices of the Ministry of Public Education and thanks to the meticulous attention of Mr. Ch. Ravaisson-Mollien, all of Leonardo's manuscripts located in France have been published. This admirable collection gives us, in six volumes in folio,[5] the photographic facsimile of each of the pages used by Leonardo for his drawings and writings as well as the transcription of all the comments inscribed upon them and finally their French translation.

The Italian government has begun to publish in more elegant form all of the papers of Leonardo in its possession. The first volume of this collection has already appeared.[6]

One cannot but feel a curiosity mixed with deep emotion when leafing through these notes left by Leonardo da Vinci. All of the thoughts, all of the images seen by the mind of the great artist are right there and are proof, by their diversity and by their very disorder, of the universal genius which conceived them.

Innumerable drawings some inked and some in color, depicting human and animal figures, leaves, churches, machines, plans of monuments or of fortresses, waves or cascades,[7] geometrical sketches, all intermingled with the crowded lines of an upright, regular handwriting reading from right to left.

The variety of topics addressed in these lines is enormous. Household accounting records, formulas for painting, personal souvenirs, anecdotes filled with a gruff Gallic humor, fragments of verse, all side by side with profound reflections on the arts and sciences. At times, these reflections go on for pages — regular and organized — and constitute almost finished outlines for a treatise on painting, a treatise on hydraulics, a treatise on perspective. At other times, the reflections are nothing but short sentences filled with erasures, revisions, contradictions, and fragments which reveal the intense labor of a thinker in pursuit of the truth.

Among these more or less finished fragments, one finds a great many dealing with the various branches of mechanics, a science which Leonardo cultivated with a passion. "La mechanica," he said,[8] "e il paradiso delle scientie mathematiche perchè con quella si viene al frutto matematicho."

In 1797, Venturi[9] drew attention to the extreme importance of some of these fragments. The study of the fragments led to the conclusion that Leonardo da Vinci, who died on May 2, 1519, already knew some of those great truths for which Galileo or some of his immediate predecessors received credit. One of these truths is the famous Principle of Virtual Velocities,[10] which has become, since Lagrange, the foundation of all mechanics.

Later, Libri,[11] quoting from even longer fragments, completed and confirmed the discoveries of Venturi. Today, now that we better understand a large part of the manuscripts left by Leonardo da Vinci, we must recognize him as the thinker who brought about the renaissance of mechanics by pushing our knowledge of statics and dynamics beyond the stage reached by Aristotle and Archimedes.

The thinker whom Felix Ravaisson[12] justly called "the great initiator of modern thought" is, as far as statics is concerned, a faithful disciple of Aristotle. His most innovative thoughts have their source in his meditations on the *Mechanical Problems* dealt with by the Stagirite.

First of all, he recognizes the law which serves as the foundation of Peripatetic statics and formulates it with great precision:[13]

*First*: If a force moves a body for some time and through some space, the same force will move half of this body in the same time through twice that space

*Second*: Or the same power will move half of such a body through all of that space in half of the time

*Third*: Half of this same power will move half of this body through all of this space in half of the time.

*Fourth*: This same power will move double this mobile body through the space in twice the time and will move a mobile body a thousandfold larger through the space in a thousandfold time.

*Fifth*: and half of this power will move the whole body through half of the space in the given time and one hundred times this body through a hundredith of the space in the same time.

*Seventh*: When two separate powers move two separate bodies in given time and through a given space, the same powers combined will move the same bodies combined through all of the space in all of the time because the former ratios always remain the same.

This law is so essential to Leonardo da Vinci that he formulates it again a little further on:[14]

*First*: If a force moves a body through some space in a given time, the same force will move half of this body in the same time through twice the space.

*Second*: If any power moves any given mobile body through some space in the same time, the same power will move half of this body through all of the given space in half of that time.

*Third*: If a power moves a body in a given time through a given space, the same power will move half of this body in the same time through half (sic!) the space.[15]

*Sixth*: If two separate powers move two separate bodies, the same powers combined will move in the same time the two bodies combined through the same space because the ratios remain the same.

However, Leonardo now qualifies this formulation. A very minute force will not impart a minute displacement to a massive mobile body. In fact, it does not move it at all. All of the theoreticians of mechanics from Antiquity through the Middle Ages recognized this fact, without, however, analyzing it as a basic law of equilibrium and motion learned from daily experience. One must complement, therefore, the preceding statements with the following propositions:

*Fourth*: If a power moves a body in a given time through a given space, it does not necessarily follow that such a force will move double the weight in double the time through double the space because it could very well be that such a power cannot move this body.

*Fifth*: If a power moves a body in a given time through a given space, it does not necessarily follow that half of that power will move this same mobile body in the same time through half of the space because it might not be able to move it at all.

These qualifications express the impossibility of certain displacements which are not precluded by Aristotle's axiom. They allow us to anticipate certain cases of equilibrium which do not derive from Peripatetic statics. We shall see the full scope of these qualifications when we discuss Leonardo da Vinci's ideas concerning perpetual motion. For the time being, we shall limit ourselves to the following consequences which result from this basic principle of Antiquity.

Among these consequences, one must first cite the one already deduced by Aristotle, which is the law of equilibrium for the scale or lever. Leonardo da Vinci formulates it in this way:[16]

The ratio which the length of the lever has with its counterlever can also be found in the quality of their weights and, similarly, in the slowness of the motion and in the quality of the path traced by their extremities when they have reached the final height of their pole.

Or, in another way[17]

There is as much accidental weight added to the driving force placed at the extremity of the lever as the mobile body placed at the extremity of the counterlever exceeds it in natural weight.

And the motion of the driving force becomes larger than that of the mobile body by as much as the accidental weight of this force exceeds its natural weight.

Furthermore, these remarks do not pertain solely to the lever. In the most complicated machines, Aristotle's axiom still allows us to compare the power of the driving forces to the resistance of the body moved:

The more a force[18] moves from pulley to pulley, from lever to lever, from screw to screw, the more powerful, yet, the slower it is.

When two forces are produced by one and the same displacement and by one and the same force, the force which takes the longest time will have more power than any other. Likewise, one force will be weaker than another force by as much as the time of the first force goes into the time of the other.

These principles illustrate well the properties of the block and tackle. Leonardo da Vinci discusses the properties of this device with the greatest possible accuracy. One can find, for example, an illustration drawn by him (Fig. 1) which he comments upon as follows:[19]

The forces which the ropes strung between the pulleys derive from the driving force are in the same ratio to each other as the ratio existing between the velocities of their motions.

Of the displacement made by the ropes on their pulleys, the displacement of the last rope is in the same ratio to the first rope as the number of ropes; that is to say, if there are five ropes and if the first rope moves by one brasse,[20] the last rope will move by a fifth of a brasse; and if there are six ropes, the last rope will move by a sixth of a brasse and so on *ad infinitum.*

The ratio between the displacement of the driving force of the pulleys and the displacement of the weight lifted by the pulleys will be the same as the ratio existing between the weight lifted by these pulleys and the weight driving the pulleys.

Let us suppose that we have a well-defined cause for a displacement as, for example, a quantity of water at rest in a reservoir and ready to be released from a given height into a lower reservoir. The cause of the displacement possesses a known mechanical power. We can distribute the use of this power, but we cannot increase its magnitude. We can make this force overcome greater resistances but only under the condition that it move them more and more slowly.

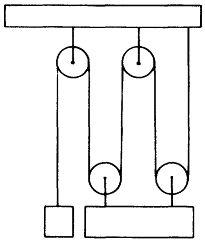

*fig. 1.*

If a wheel[21] is turned in a given time by a quantity of water and if this water cannot be increased either by flow or quantity or by increasing the height of its fall, the operation of this wheel is limited. That is to say, if a wheel drives a machine, it is impossible for that wheel to drive two machines without needing twice the time. Thus it is impossible for it to produce as much work in one hour as two machines with a second hour. However, the same wheel can propel an infinite number of machines, although over a long period of time these machines will not accomplish more work than the first within one hour.

A given weight falling from a given height produces a mechanical effect whose magnitude is independent of the circumstances surrounding this fall. The magnitude remains the same whether the fall occurs at once or is divided up.

If someone walks down stairs[22] by jumping from stair to stair and if you add up all the forces of the percussions and the weights of these jumps, you will find that they are equal to the sum total of the percussions and weights produced by the same person falling in a perpendicular line from the top to the bottom of these same stairs.

The passages just quoted include the formulation of a principle which is of extreme importance to the art of the engineer. However, this principle is in the final analysis nothing but the logical result of the axiom posed by Aristotle. Not content to bring the seeds planted by Peripatetic mechanics to fruition, Leonardo da Vinci tackles and resolves a difficulty which had caused the Stagirite to falter.

The extremity of a lever which is supported at a point on its horizontal axis describes the circumference of a circle in a vertical plane. The path followed by this extremity does not have the same direction as the weight to be lifted, which pulls down along a vertical line. The result is that the resistance[23] to be overcome in order to make this arm of the lever turn through a certain angle depends on the initial position of the arm. The closer the lever is to the horizontal position, the greater the resistance.

According to what law does the force or the resistance of a given load vary if one inclines the lever with the load applied at its extremity? Leonardo da Vinci has the following answer to this question:[24]

The ratio of the length *mn* (Fig. 2) to the length *nb* is the same ratio as the fallen weight at *d* to the falling weight at *b*.

Thus the weight suspended at the extremity of an inclined lever arm has the same effect as if it were suspended at the extremity of a

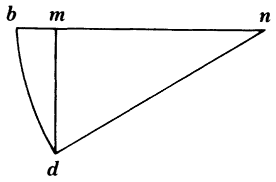


*fig. 2.*

particular horizontal lever arm. The latter lever arm is determined by projecting the point of support onto the vertical line along which the weight is exercising its traction. Leonardo da Vinci calls this horizontal lever arm the "potential lever arm".

The junction[25] between the attachments of the scales and the arms of these scales always forms a potential rectangle which exists only if these arms are inclined.[26]

The real arms of the balance are always much longer than the potential arms, even more so when they are closer to the center of the earth.[27]

The real arms are never[28] included within the potential arms (Fig. 3) of the balance unless the former are in a horizontal position.

At the extremity of a lever, one can apply a force whose direction may be different from the vertical. It suffices to use a rope stretched in this direction over a pulley and pulled by a weight. A similar rule to the preceding one allows us to determine the driving force of a similar machine. Leonardo da Vinci formulates[29] this rule in the following way:

In order to know at each degree of the displacement the quality of the force of the power which pulls as well as that of the object moved, do as you see in Figure 4. That is to say, as soon as the weight ceases to move, imagine a line which cuts at right angles the line of the driving force *mn* with *fh*.

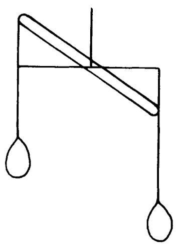

*fig. 3.*

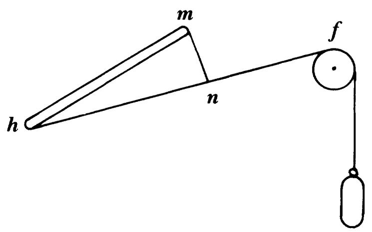


*fig. 4.*

This line *mn*, considered above as analogous to the potential lever arm, is called by Leonardo the true expression of the balance or the "spiritual arm".

A line can be called[30] the true expression of the balance when its direction meets the straight line of the rope pulled by the weight and when their junction forms a right angle as can be seen (Fig. 5) at *m* with *mA* and *pn* with *nA*, the spiritual arm.

Thus, when a force is applied to a body mobile about an axis perpendicular to this force — Leonardo da Vinci uses the term 'circumvolubile'[31] — it matters little in the evaluation of its mechanical effect to try to find the point of application of that force. One need only consider two things: the intensity of the force and the shortest distance between the axis of the 'circumvolubile' and the direction of the force.

There is always[32] the same force and resistance no matter where one has attached the rope on the line *abc* (Fig. 6) or on the line *mn*.

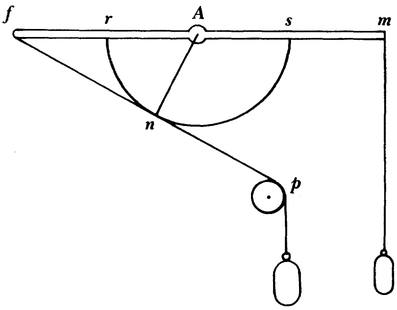


*fig. 5.*

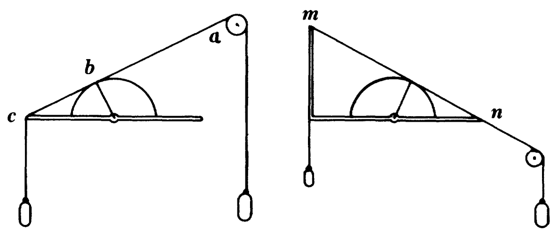


*fig. 6.*

It makes no difference where the rope *nc* (Fig. 7) is attached to the segment *ac* because one always uses a line which falls perpendicularly from the center of the balance to the line of the rope, that is to say, the line *mf.*

These various passages show that Leonardo da Vinci had a very clear conception of the notion of the moment of a force with respect to an axis, at least when a force is situated in a plane perpendicular to the axis. These passages also show that he knew how to develop the condition of equilibrium for a solid body about an axis under the action of similar forces.

It does not appear that he attempted to establish a relationship between this theory of moment and Aristotle's axiom. However, such a relationship exists. The notion of moment appears immediately if one takes as the measure of the driving force which a load suspended at the extremity of an oblique lever arm exerts, not the product of this load by the velocity with which the extremity of the lever turns, but the product of this load by the velocity with which it descends. This modification seems to agree perfectly with Aristotle's axiom and with the idea expressed by Leonardo in a passage we quoted above: namely, to take the height of the fall of a weight as the measure of the mechanical effect produced. However, in order to see this relationship between Aristotle's axiom and the notion of moment, one must refer to the definition of the instantaneous velocity of the load. This notion, however, which was to play such an important role in the development of infinitesimal analysis, was still vague in the minds of Leonardo and his contemporaries.

If there is one mechanical problem upon which the great painter often reflected, most assuredly it is the study of the apparent weight of

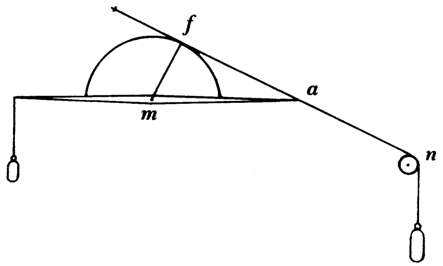


*fig. 7.*

a body descending on an inclined plane. One cannot leaf through his manuscripts without encountering over and over again one and the same drawing with minor variations: a rope stretched over a pulley with the weight on each side descending on two differently inclined planes.

The search for the laws governing the equilibrium of this mechanism occupied Leonardo incessantly. He recognized immediately that a weight descending on an inclined plane pulls less on the rope from which it is suspended than if it were to descend in free fall and, furthermore, that its pull is smaller the less the plane is inclined. However, such qualitative information cannot satisfy the mechanician, who requires a quantitative relation.

In order to obtain this relation, Leonardo da Vinci constantly varies his approaches. One of these follows, which, by rather strange reasoning, gives him a result close to the truth.

He undertakes to compare the velocities at which the same sphere descends on planes of different inclination. He observes that when the sphere is in equilibrium on the horizontal plane, the center of this sphere is on the vertical through the point where it touches the plane. the distance from the center of gravity to this vertical increases with the inclination of the plane. At the same time, the velocity of the unimpeded sphere increases as it descends on this plane. From this, he assumes that there is a ratio between the velocity of descent and the distance from the center of gravity to the vertical through the point of support. Consequently, he easily draws the following conclusion. The velocity of a sphere descending on an inclined plane is to its velocity in free fall as the ratio of the height of the fall is to the length of the line of the greatest inclination described by the moving body. Furthermore, for Leonardo da Vinci as well as for Aristotle, the efficacy of a mechanical action is proportional to the velocity which it imparts to a moving body. The preceding ratio is thus equal to the ratio between the weight of the sphere descending on an inclined plane and its weight in free fall.

Here is the passage[33] where this unusual solution is summarized.

The heavy spherical body will move faster, the further away its point of contact on the inclined plane is from the perpendicular of its central line. The shorter *ab* (Fig. 8) is in relation to *ac, the more slowly the ball* will descend along the line *ac*. It will do so, the smaller part *o* is in comparison to part *m* because with *p* as the pole of the ball, part *m, being above p,* would descend faster if it were not for the small amount of resistance caused by part *o* as a counterweight. If this counterweight did not exist, the ball would descend faster along line *ac* the more *o* goes into *m*. That is to say, if part *o goes into*

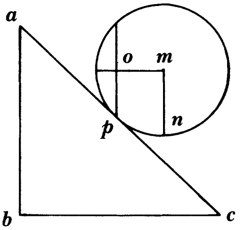


*fig. 8.*

*part m* one hundred times, part *o is not part of the rotation* of the ball, and the ball would descend faster by one hundredth of the usual time along *n* and the central line. With *p* as the pole where the ball touches the plane, the greater the distance between *n* and *p*, the faster its run down the plane.

Leonardo could not be satisfied with such a method. Thus he chose to tackle the problem of the inclined plane with a more rational approach.

He recognized that the weight accelerating the moving body towards the center of the earth could be divided into two components, one being perpendicular to the inclined plane on which the body descends, the other being tangential to this plane. It is the latter component which pulls the moving body down.

The homogeneous body descending obliquely, he says,[34] divides its weight into two different components. The proof is as follows. Let *ab* (Fig. 9) be the moving body on the oblique line *abc*. I maintain that the weight of the body *ab* divides its gravity into two components, that is to say, along the line *bc* and the line *mn*. We will discuss in the book, *On Weights*, why and to what extent the magnitude of one component is greater than that of the other and what degree of obliqueness of the inclined plane divides the two components equally.

This decomposition can be utilized in various circumstances. If, for example, a weight suspended by a rope at the extremity of a lever arm oscillates in the same way as a pendulum, it will at each instant exert a force on the lever due to the vertical component of its weight. Therefore, the weight will appear lighter, the further the rope to which it is attached is from the vertical.[35] In the same way, a body suspended by two divergent ropes will distribute its weight between the two ropes.

According to which rule does the decomposition of a weight in two

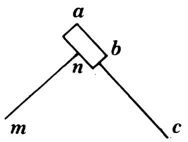


*fig. 9.*

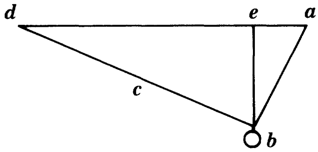


*fig. 10.*

different directions occur? Leonardo does not seem to have known about the rule of the parallelogram of forces on which the solution to the given problem depends. Repeatedly, he develops an erroneous solution. Here is a passage[36] where this erroneous solution is explicitly formulated:

The body is suspended at the angle formed by the two ropes and will distribute its weight to the two sides of the angle, and their weights will be to each other in the same ratio as the ratio of the inclination of their sides. Or: such a body distributes its weight among its supports according to the same proportion as the angles formed by the division of the angle on which this weight is suspended: it is a division of an angle produced by the straight line descending to the center of the suspended body. Thus the length *abd* (Fig. 10) being cut by the line *eb* and the angle *ebd* being 9/11 of the angle *abc*, the angle *abe* 2/11; *ab* is 9/11 of the weight and *ab* is 2/11.

This rule for decomposing a weight in two directions is repeated in another passage:[37]

Let an angle be formed by the conjunction of two oblique ropes to which a heavy body is attached. If this angle is divided by a vertical line through the center of the body, then the angle is divided into two parts which will have the same ratio to each other as the ratio by which the body distributes its weight between the two ropes.

The figure accompanying this statement shows us that in this passage as well as in the preceding one, Leonardo is taking the ratio of the two partial angles under consideration to be the ratio of the lengths which the angles intercept on the same horizontal line, in other words, the ratio between the trigonometrical tangents of these angles.

Furthermore, at times[38] a similar rule seems to define for him the ratio of the two weights supported by two unequally inclined planes and pulling on the two ends of the rope which runs over a pulley. Leonardo thinks that these weights must be in an inverse ratio to the inclinations of these planes. He takes the ratio of these inclinations to be the ratio between the tangents of the angles formed with the horizontal.

Did Leonardo always adhere to this inexact rule about the decomposition of forces? It is probable that he was not satisfied with it and that his ever active mind continued to search, and it seems as if he glimpsed the truth in this matter. This much we seem to be able to conclude from a brief and unfinished note[39] which we shall now analyze.

A rope *pmonq* is strung across a pulley rotating about an axis *d* (Fig. 11) and is stretched by two weights *p* and *q*. These weights slide on two unequally inclined planes *da* and *dc*. The two segments of the rope *mp* and *nq* are stretched parallel to the planes *da* and *dc*, respectively. Moreover, the figure is constructed so that projection *de* of the radius *dn* on the horizontal line *hf* constitutes two thirds of the radius of the pulley, while the projection *dg* of *dm* on the horizontal line *hf* is one third of the same radius. One needs to evaluate the component of the weight *q* along *ng* or *dc* and the component of the weight *p* along *mp* or *da*. Here is what Leonardo writes about this evaluation:

Because of the right angle *n* above *df*, the weight *q* weighs two thirds of its natural weight which was three pounds and which now exerts a force of two pounds. The weight *p* which was three pounds, now exerts a force of one pound because of the right angle above the line *hd* at point *g*. Thus we have here two pounds against one pound.

It is difficult to say with absolute certainty which principle led Leonardo to this accurate statement. However, the lines which we have just quoted seem to indicate that the rule invoked here in a more or less conscious fashion, is not at all the rule of the parallelogram of forces but an equivalent proposition: namely, the moment of the resultant of two forces is equal to the sum of the moments of the components![40]

Had Leonardo really acquired an understanding of this important theorem? In his published manuscripts, we have found no evidence for

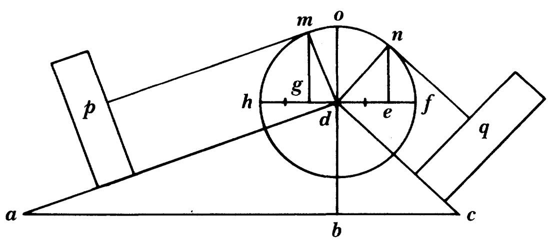


*fig. 11.*

it other than what we have just cited. Do the unpublished manuscripts, especially those comprising the famous Codex Atlanticus, contain passages which could confirm this conjecture? There are reasons for hoping so and, therefore, for wishing for the prompt publication of these precious relics.

## CHAPTER III

# JEROME CARDAN (1501–1576)

After Venturi had announced in 1797 that he had found in the manuscripts of Leonardo da Vinci some of the essential laws of modern mechanics, the surprise of some mechanicians must have been mixed with regret. On certain points, the great painter had anticipated Galileo by a full century. Imagine what an impulse the study of mechanics would have received had Leonardo only been able to publish the *Treatise on Motion* and the *Treatise on Statics* during his lifetime or at least if the fragments left behind had become known immediately after his death! In that case, at the beginning of their research Galileo, Simon Stevin and Descartes would have found this science much further advanced on the road to progress. With the efforts of Leonardo added to theirs, they could have advanced this science much further than they actually did and the entire development of the exact sciences would have progressed much faster. Thus human knowledge was believed to have suffered an irremediable delay in its march forward because the ideas of Leonardo da Vinci concerning the principles of mechanics remained unknown for centuries.

This delay, however, did not actually occur. From the middle of the 16th century, the most essential ideas of Leonardo da Vinci concerning statics and dynamics were known to those interested in these sciences. The mathematicians and the mechanicians looted the manuscript notes of the great painter and helped themselves copiously. They flaunted Leonardo's ideas in their writings without, however, revealing the source of their riches. It was petty but lucrative theft, which undeservedly increased the glory of certain of the authors, but which, nonetheless, brought to light and put into circulation a part of the treasure amassed by Leonardo da Vinci!

Among those who seized upon Leonardo da Vinci's ideas in order to analyze, comment upon, and develop them, one ought to mention first Jerome Cardan. But he was not the only one because others had either preceded him or later imitated him. To give an example, we can find the influence of Leonardo in the works of Giovanbattista Benedetti. Cardan, however, was among the first to publish the most essential results of the great painter's meditations on mechanics. Cardan's great reputation and the wide diffusion of his publications made them known everywhere. Through the writings of Cardan, Leonardo's ideas reached Galileo, Kepler, and Simon Stevin and exerted a powerful and beneficial influence upon the development of mechanics.

The view which we have just expressed has momentous consequences[1] for the history of mechanics.[2] It shows the path by which Peripatetic mechanics spread through Leonardo's and Cardan's writings into modern science so as to fecundate it after having been locked away for centuries by its Scholastic commentators. If this view is correct, it should help us to better understand the evolution which permitted the seeds contained in the science of the School[3] to burst their archaic shell and produce the science of the 17th century. Thus it is important to support our view with solid arguments.

It is unfortunately an all too certain fact that Leonardo da Vinci's manuscripts had fallen prey to looting in the midst of the 16th century. We know the negligence of those who were supposed to guard this precious trust:

> Not only did the works of the great painter perish, says Libri,[4] but the great majority of the books containing his notes were lost too. After his death, all of his manuscripts, drawings, and instruments became the property of his pupil, Francesco Melzi, to whom he had bequeathed them. Melzi, who was only a dilettante, transferred this precious heritage to his home in Vaprio, close to Milan. His descendants ignored the existence of the trust and when a man by the name of Lelio Gavardi, a relative of Alde Manuce the Younger, and tutor to the Melzi family, saw how the family was letting this fine collection go to waste, he stole thirteen of these manuscripts and took them to Tuscany in order to sell them to the Grand Duke Francis I. The prince, however, had just died and the manuscripts were left in Pisa with Alde, who showed them to his friend Mazenta. Mazenta greatly disapproved of Gavardi's conduct, who, now ridden by remorse, asked Mazenta to take the manuscripts back to Milan and to the Melzis. The head of the Melzi family, Horace, completely unaware of their value, give the thirteen volumes to Mazenta. He also told Mazenta that many more drawings and manuscripts of Leonardo were stashed away somewhere in his house in Vaprio. In this way, several interested parties obtained the drawings, the instruments, the anatomical specimens and all the rest of Lenardo's legacy. Pompey Leoni, a sculptor in the service of Phillip II, was one of those who obtained the largest share.

Thus everyone ransacked and helped himself as he pleased to the treasures amassed by the genius of Leonardo. Some treatises were kept by those particularly interested in them while the others circulated from hand to hand until they were lost. We know from Pacioli[5] that Leonardo had completed a draft of his *Treatise on Painting*. In his *Lives of the Best Painters, Sculptors and Architects*, Vasari[6] tells of having seen Leonardo's autographed treatise in the hands of a Milanese painter who wanted to have it printed in Rome. Leonardo had also completed a draft of a *Treatise on Perspective*. On the same subject Cellini repeats several times in his work published in Florence in 1568 that he had held this treatise in his own hands and that he had loaned it to Sarlio and that Sarlio had used the most important parts of the work for his own purposes.

More or less faithful copies and excerpts from these treatises of Leonardo circulated both in and outside of Italy. Dufresne, using one of the copies sent to Del Pozzo, had the *Treatise on Painting* published in Paris in 1651. Another more complete copy kept in the Vatican Library enabled Manzi to publish a more finished edition in 1817.

Painters and draftsmen took full advantage of the pillaging of Leonardo's manuscripts. The mechanicians were equally well-informed about the existence of the manuscripts because the machines which he invented were still in use in the 16th century and still carried the name of their inventor.[7] Those who were interested in the theory of equilibrium and motion were sure to find a wealth of new ideas in a collection left over to depredation through the indifference of the Melzis.

Jerome Cardan lived in Milan not far from the house in Vaprio which had so poorly guarded the treasure. Cardan was one of those universal minds which Italy produced in abundance from the 15th to the 16th century. Just as Leonardo before him and Galileo after him, Cardan seems to have been able to understand and perfect all the sciences which he studied. Although a physician of great fame, he devoted much of his time to the study of algebra and considerably advanced the theory of equations. Furthermore, he combines the boldest ideas with the most childish superstitions in a web of stupendous absurdities. Astrology and the divination of dreams occupy his mind as much as sound physics and rigorous arithmetic.

The respect he has for other people's intellectual capabilities is not hampered by any scruples. He is not ashamed to add to the list of his own discoveries many which he borrowed from his contemporaries. One example should be proof enough.

Stimulated by a question from Antonio Fiore, who owed to Ferro of Bologna a method of solving an equation of the third degree, Tartaglia[8] went on to solve all equations of that order. His discovery, which he carefully kept secret so that he could safely challenge his rivals, like a swashbuckler keeps to himself a secret thrust, was finally found out. Cardan was very curious about the solution and repeatedly asked Tartaglia or had someone else ask him to give him the method. Having been refused each time, Cardan was able to get hold of a fragment of a verse which explained how to obtain the root of any equation of the third degree. In order to obtain this information, Cardan did not hesitate to pledge his Christian faith and his word of honor as a gentleman that he would never make public the method which he was asking Tartaglia to reveal to him:

Io vi giuro, ad sacra Dei evangelia, e da real gentil'huomo, non solamente di non publicar giammai tale vostra inventione, se me le insignate . . .[9]

When he found out the solution which he had sought so ardently, he immediately proceeded to publish it in his *Ars Magna*. Tartaglia complained bitterly about this act of bad faith which allowed his own discovery to appear in print for the first time in someone else's book.

He was right in complaining, says Libri, because posterity has persisted in calling the formula which gives the solution to equations of the third degree by Cardan's name.

Cardan, however, had acknowledged the priority of Tartaglia and of his predecessors Scipion Ferro and Antonio Fiore, whereas Tartaglia never mentioned the name of Ferro when he published the solution later. The mechanicians of the 16th century were easily hurt when someone stole their own discoveries, but were nonchalant when they borrowed the discoveries of others.

It is difficult to imagine that Cardan, so eager to know Tartaglia's discovery and so prompt, despite his oath, to use it to adorn his book on algebra, was not also motivated by a curiosity to know about Leonardo da Vinci's ideas on mechanics and physics. It is equally difficult to imagine that once he knew about them, he would resist the temptation to glean some of these ideas in order to stimulate his own meditations. And, in fact, he did not resist that temptation at all.

In 1551, Cardan published his twenty-one books *On Subtlety*.[10] A second and more complete Latin edition[11] of this work was published in 1554 and was translated into French by Richard Le Blanc[12] in 1556. During the second half of the 16th century many Latin and French editions of this work followed.[13] Later, Cardan appended his *Opus* *novum de proportionibus*[14] to this work. All of the passages dealing with mechanics in these two works clearly bear the stamp of Leonardo da Vinci.

The similiarities between the views of Leonardo on statics and those of Cardan are abundant. The latter are scarcely more than a skillfully organized version of the former. But it would be idle to dwell here on these similarities, which will become clear in the following pages.

As we shall see in Chapter IV, Leonardo da Vinci and Cardan are in complete agreement regarding the impossibility of perpetual motion. The two men are also in perfect agreement on the principles of dynamics. This agreement is all the more significant since their opinions relating to diverse questions on dynamics have a peculiar form which one seldom finds in their predecessors or their contemporaries.

We hope some day to be able to retrace the origins of dynamics in the same fashion we are now retracing the origins of statics. That would be the occasion to analyze in detail the views of Leonardo and Cardan on dynamics and their influence on the development of rational mechanics. We would then see how the doctrine of the Milanese physician was inspired in its most minute details by the thoughts found scattered throughout the manuscripts of the great painter.

Cardan's borrowings from Leonardo's physics are fewer in number, although still recognizable. Thus Cardan, setting out to explain how to start a fire at the focus of a concave mirror, says:[15]

The heat obtained from concave or perfectly spherical mirrors obviously results from concentration. The reason for this concentration is not obscure, because if you distribute ten cents to ten people, each one will have one cent. If you distribute the same amount to five persons, each one will receive two cents. Thus, if the heat scattered over a wide surface is collected, all of the heat scattered over this wide surface will be concentrated in a small space. However, this large amount of heat concentrated within this small space will produce great effects. One can truly speak of great effects because through it fire can be produced.

Leonardo da Vinci had written:[16]

*On the quality of heat produced by the sun's rays in a mirror.* The heat of the sun on the surface of a concave mirror will be redirected among the pyramidal rays merging in one single point. The closer this point is to the surface of the mirror, the hotter it will be in comparison to the heat on the mirror. Also, the more often *ab*, or if you wish *cd*[17] enters into the mirror, the higher will be its heat in comparison to that of the mirror.

In another passage, he writes:[18]

One and the same property is more powerful, the smaller the space it occupies. This applies to heat, sound, weight, force and many other things.

We shall first speak about the heat of the sun which develops in the concave mirror and is reflected in the shape of a pyramid.[19] The pyramid acquires proportionately more power the narrower it becomes. That is to say that if the pyramid strikes the object at the midpoint of its length, it concentrates half of its cross-sectional area in the lower half. And if it strikes by 99% of its length, it contracts by 99% of its base and increases by 99% the heat which the base receives from the source, be it from the sun or the fire.

Cardan's answer[20] to the question, "What causes the colors of the rainbow named Iris?" can be compared, although with less precision, to Leonardo's writings on the subject of the rainbow.[21]

But Cardan, on numerous occasions, does not hesitate to diverge from his illustrious predecessor. On the subject of the tides, the scintillation of the stars, the suspension of the clouds in the atmosphere, Cardan adopts solutions distinct from those proposed by Leonardo. His theories on heat, fire and the elastic forces in gases are his own. They could well be the most remarkable part of the work *On Subtlety.*

Cardan was not a vulgar plagiarist. He was able to extract the quintessence of the ideas planted by Leonardo and then assimilate and develop them in order to nourish the science of the 16th century with ideas which, left buried within the house of the Melzi, would have remained unknown and useless without his felicitous indiscretion. As we shall see, in the domain of mechanics, where he borrows heavily from Leonardo, he was able to leave the imprint of his own originality beside the unmistakeable mark of his ingenious predecessor.

Cardan did not disdain to exercise his mathematical talents on demonstrations constructed in the manner of those of Archimedes and to fill certain lacunae left by the famous Syracusan. Archimedes, for instance, had always neglected the weight of the lever itself or of the arm of the balance from which he suspended weights when studying equilibrium. Cardan set out to determine the mechanical properties of the homogeneous horizontal arm of a balance suspended at any arbitrary point. That is the topic of the article entitled "Staterae ratio"[22] in the *De Subtilitate*[23] and which his translator Richard Le Blanc explains in the following way:

It is the standard of weight ordinarily referred to in Paris as a *sledge* and traditionally used by weavers; in Latin *statera*.[24]

Cardan bases his analysis on two propositions taken as axiomatic.

First, he states that a segment AB′ (Fig. 12) which is equal to the small arm AB of the balance will hold the latter in equilibrium. Secondly, he states that the remaining length of B′C of the large arm has the same effect as if its weight were suspended from point M midway between B′ and C.

Assume that the arm of a balance is weightless and that a uniform weight is distributed along that part of the beam which is the difference in length of the two arms of the balance. And further assume that a pin is inserted at the interior endpoint of the segment. The effect of the distributed weight will be the same as that of an equivalent concentrated weight suspended midway between the pin and the end of the segment.

These principles easily yield the solution to the given problem. Cardan treats this problem again in the *Opus novum*[25] and he arrives at this proposition:

The moments of the two arms AB and AC of the beam of the balance are in the same ratio as the squares of the lengths of these two arms.

And Cardan does not hide his satisfaction at having arrived at such a solution:

Hoc est,[26] he says, quod Archimedes reliquit intactum, cum esset maxime necessarium et ostendit magis abstrusa sed, pace illius dixerim, minus utilia.[27]

This solution was not so difficult to warrant this triumphant exclamation. Nevertheless, it had an undeniable influence on the thinking of Cardan's successors. Simon Stevin, for one, and Galileo, for another, gave up the requirements upon which Archimedes had based his reasonings on the equilibrium of the lever. They restricted their study of the lever to the consideration of a heavy homogeneous beam suspended at its center, using the very axioms which Cardan had proposed.

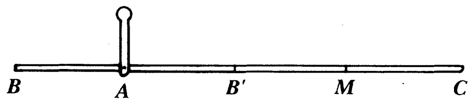


*fig. 12.*

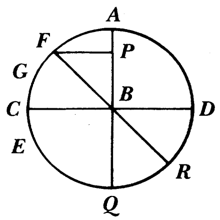


*fig. 13.*

Although Galileo might not have known the *Opus novum*, he did know at least the *On Subtlety*, which he quotes frequently in his early works. It would be implausible to assert that Simon Stevin was not familiar with any of the numerous editions of this work. At any rate, the *Opus novum* is quoted and criticized by the Flemish geometer.

These demonstrations of statics, conceived in the manner of Archimedes, do not constitute the most important part of Cardan's thoughts on the equilibrium of weights. The development he gives to Aristotle's axiom is of greater significance. By enriching and transforming this axiom with the help of the thoughts scattered throughout the manuscripts of Leonardo da Vinci, Cardan derives the Principle of Virtual Velocities in a form that will be used by Galileo and remain unchanged until the time of Descartes.

Let us begin with a quote. Later we shall analyze its rich content. This is how Cardan[28] expresses himself in the first book of *De Subtilitate*, translated by Richard Le Blanc:

On the scale and its measure. After these matters one must now consider the weights placed on the balance. Let us assume a balance which is suspended from point A (Fig. 13) and let the fulcrum be at the point where the two arms of the balance CD are joined . . . I maintain that the weight placed at C will be more powerful than if the balance were inclined to any other position such as at F. In order for us to recognize that the weight at C is heavier than at F, it is necessary that it be moved in the same time through a larger space towards the center.[29] For we see that these objects which are heavier than others for the same reason are moved more easily (faster) towards the center. I will show by two reasons that this happens with the weight of the balance placed at C rather than at F.

The first reason is that if the weight is moved instantaneously from C to E and if arc CE is equal to FG, it would descend from F to G more slowly than from C to E, and thus it will be lighter at F than at C . . . It is obvious from balances and other devices that lift heavy weights that the further the weight is from the fulcrum the heavier it is. The weight at C is a distance BC from the fulcrum and at F by the distance FP . . . Thus the following reason is of general validity. Namely, the further the weights are from the post or line of descent as measured by the perpendicular or oblique line, that is to say by the angle, the heavier they are. And thus, the tendency of the weight is to move directly to the center. But since it is prevented because it is constrained, it moves as it can.

Thus, when a heavy body descends along a vertical line, the motor power of this body, as Aristotle claimed, is measured by the velocity with which it falls. But because of the arrangement of the mechanism supporting it and because of the nature of the *connections*,[30] to use an expression which modern mechanics has borrowed from Cardan, it can happen that the heavy body does not move along a vertical line. In that case, one must take into account when measuring its motor power, not the total velocity of the heavy body, but only its vertical component, that is to say, the velocity of the fall.

If a given weight is suspended from any point on a solid body which is capable of rotating about a horizontal axis, the motor power of this heavy body increases the faster the point of suspension descends due to the effect of rotation imparted to the suspended body. From the outset, the velocity will increase the further the point of suspension is from the vertical plane which contains the horizontal axis.

Today, it is easy for us to carry out such an analysis and to deduce from the given premises the ratio between the motor power of a suspended body and the distance between the point of suspension to the vertical plane containing the horizontal axis. All we need do is refer to the definition of the velocity of the fall: a relation between an infinitesimal vertical displacement and its infinitely short duration. We can thus see that the motor power of a weight suspended from an apparatus which moves about a horizontal axis is measured by the moment of this weight with respect to the vertical plane containing the horizontal axis. However, the notion of the ratio between two infinitely small quantities had not yet been fully developed at the time Cardan was writing. Thus he could not accomplish the deduction we just described. All he could demonstrate was that the motor power of the suspended body increases at the same rate as its moment or admit intuitively the proportionality of these two quantities, as he did in the *Opus novum*.[31] The mechanical relation between Aristotle's axiom, transformed[32] into the Principle of Virtual Velocities, and the theory of moments, was, nevertheless, clearly grasped. All it needed to become more rigorous was progress in infinitesimal analysis.

We have seen how Cardan combined various ideas created or recognized by Leonardo da Vinci and then established relations between them which the great genius might not have recognized and which he did not point out in any case. In other passages, the physician from Milan appears as a faithful interpreter of Leonardo's ideas. What the *Books on Subtlety* say about the block and tackle seems to come straight out of the manuscripts which we discussed in the preceding chapter.

The fourth example of subtlety, says Cardan[33] concerns the block and tackle.[34]

After having described a block and tackle with four ropes, he adds:

The load is thus ... pulled upward by a fourth part of the force. And if each pulley block had three sheaves, the load could be lifted by one-sixth of the force. And in this way, a child could pull up a great load unless the weight of the ropes or the rigidity of the sheaves or pulleys or block and tackle prevented him from doing so. But since there is a ratio between time and the forces and powers, the child will pull four times more slowly over two sheaves and six times more slowly over three sheaves what he would be able to lift with one rope with the same force. Actually, a slightly larger (force) is required or the time will have to be multiplied by a little more than four or six to the extent that the length of the rope adds to the load.[35] Thus the child will be able to lift with a block and tackle in scarcely one hour the same load that a man, six times stronger and standing above the load, could lift at once with a single rope.

Leonardo da Vinci applied Aristotle's axiom with precision only to the lever and to the block and tackle. With respect to the screw, he was satisfied with this brief remark:[36]

The more a force moves from wheel to wheel, from lever to lever, from screw to screw, the slower and more powerful it becomes.

Cardan expands[37] on this remark in a section entitled: *The Method of Pulling and Pushing Anything With Little Force.*

The screws, he says, used in wine presses are made and constructed according to such a method ... The more threads there are in the screw and the less inclined they are, that is, the closer they are to the circle and the deeper, the smaller the applied force and the easier the movement. And the easier the movement, the slower it will be. If the screw thus measures two cubits with threads deep and flat, the weight can easily be lifted by a ten year old child. But as I said previously, the easier it is to move, the slower it will be pulled or lifted.

Cardan applies this Principle of Virtual Velocities in the *Opus novum*[38] in order to evaluate the efficiency of the screw-jack and, in the *De Subtilitate*,[39] he applies it in order to design:

A large machine for lifting bulky and heavy loads, composed of a screw and a screw-jack.

In all matters concerning the Principle of Virtual Velocities, Cardan cleverly developed and completed the ideas he had gained from his study of Leonardo da Vinci. He was less fortunate as far as the inclined plane was concerned. He does not broach the subject in the *De Subtilitate*. In the *Opus novum*, he sets out[40] to determine the weight of a moving sphere on an inclined plane. According to the principle of dynamics universally acknowledged at that time, Cardan thinks of the weight of the sphere as being proportional to the velocity at which the sphere, if left to itself, would descend along the plane. Since this velocity, zero on a horizontal plane, increases with the angle of inclination of the plane, Cardan thinks he can state the following proposition:

The weight of a sphere which descends along an inclined plane is to the weight of the same sphere descending in free fall as the angle between the inclined and horizontal plane is to a right angle.

Although this solution is erroneous, the passage in which Cardan develops it deserves to be mentioned because it undoubtedly helped suggest to Simon Stevin, on the one hand, and to Galileo, on the other, the correct solution to this famous problem. In his work on statics, Stevin quotes and discusses Cardan's *Opus novum*. When Galileo discovered the law of the inclined plane for the first time, he must have had the following passage in mind:

Let there be a sphere *a* of weight *g* (Fig. 14) placed at point *b*, and which is to be pulled over the inclined plane *bc*, with *bf* being the vertical plane. On the horizontal plane *be, a* can be moved by a force as small as one wishes, according to what was said above. Consequently, according to common opinion, the force which will move *a* along *be* will be zero. On the other hand, according to what was said, *a* will be moved towards *f* by a constant force equal to *g*. It will be moved in the direction *bc* by a constant force equal to *k*. Finally, it will be moved in the direction *bd* by a constant force equal to *h*. Thus, because of this latter requirement, *cum termini servent quoad partes eandem rationem singuli per se*,[41] and since the movement along *be* is produced by a zero force, the relation between *g* and *k* will be like the relation between the force moving along *bf* and the force moving along *bc* and like the relation of the right angle *ebf* to the angle *ebc*. And, similarly, the force moving *a* along *bf* which — as stated previously — is *g* in relation to the force moving along *bd* which — by hypothesis — is *h*, as *ebf* is to *ebd*; thus the relation between the resistance to motion of *a* along *bd* and the resistance to the movement of the same *a* along *bc*, is the same relation as between *h* and *k* which is what was to be demonstrated.[42]

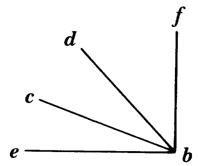


*fig. 14.*

## CHAPTER IV

# THE IMPOSSIBILITY OF PERPETUAL MOTION

It is easier to resolve the question of perpetual motion in dynamics than in statics. However, for Leonardo da Vinci and for Cardan as well as for Aristotle, there are no insurmountable barriers between these two sciences. On the other hand, Galileo and Stevin accepted the impossibility of perpetual motion as an axiom capable of providing a basis for certain demonstrations in statics. And both of them had read Cardan, where they probably found support for their confidence in this axiom. But Cardan himself, in writing against perpetual motion, had done nothing more than summarize the scattered notes of Leonardo da Vinci. Thus we cannot gain a clear and complete picture of the origins of statics without reviewing the objections raised by Leonardo da Vinci and Cardan to the *perpetuum mobile.*

The search for perpetual motion is a general term expressing two distinct utopian goals: the search for the perpetual motor and the search for the perpetually moving body.

The cruder of these two utopian goals is the search for the perpetual motor. Such is the illusion of the miller who has a given volume of water in reservior ready to be released from a given height. The miller dreams of combining marvellous gears which would enable him to mill as much grain as he wants without raising his reservoir by an inch or adding a pint to the existing volume of water.

We have seen with what precision Leonardo da Vinci, the great master of hydraulics, brings our miller's ambitions back to earth. Let him connect one hundred millstones to his waterwheel instead of one. Each one of them will then mill for him one hundred times less grain. A given weight which falls from a given height represents a given driving power. One can divide up this power or change its use infinitely, but one cannot increase it.

This truth dashes the hopes of anyone looking for a perpetual motor. However, it still gives free reign to the dreams of those seeking to realize the perpetually moving body.

Without requiring an engine to produce any external mechanical work and without exerting on it any action, could we not have an engine which, once put into motion, would move indefinitely? Could we not construct, for instance, a wheel so perfect that, once in motion, it would turn on its axis without ever stopping? Could we not construct a clock in such a way that its two equal weights would precisely counterbalance one another and that one weight descending from its highest point would lift the second weight, which, in turn, had lifted the first weight so that this perpetual clock would rewind itself?

It is absurd to expect perpetual motion from an initial impulse because the motive power of this impulse — Leonardo calls it its "forza" or its "impeto," Leibnitz will call it its living force[1] — is constantly being expended. It is equally absurd to expect from any arrangement of weights a perpetually moving body because gravity always tends towards equilibrium and any motion produced by it has as its goal a state of rest:

No thing without life, says Leonardo da Vinci,[2] is capable of pushing or pulling without accompanying the body being moved. These motors can only be "forza" or falling weight. If falling weight pushes or pulls, it can only produce this displacement of the body because the body seeks a state of rest and, since no moving body which is descending is capable of returning to its initial position, motion stops.

And if one body which moves another body is the "forza," this potential capacity, in its turn, accompanies the body being moved by it. It moves it in such a way that it expends itself. And once it is expended, no body, having been moved by it, is capable of reproducing it. Thus no moving body can move for a long time because in the absence of causes there will no longer be any effect.

Leonardo da Vinci's contemporaries readily agreed with him that the motor power of an impulse transmitted to a group of bodies will dissipate. Indeed, all the Peripatetics accepted as axiomatic that violent motion always finishes by expending itself. As they were in the habit of saying: "Nullum violentum potest esse perpetuum."[3] When Leonardo describes this continuous loss of the living force within a system in motion, he uses glowing poetic expressions:

I maintain[4] that the "forza" is a spiritual virtue, an invisible power which, through an external accidental violence, is caused by motion, and which is introduced and infused into the bodies, which are, in turn, being pulled and diverted from their natural condition. This spiritual force imparts to them an active life of marvelous power and forces all things created to change their form and place. It runs furiously to its desired death under ever changing forms according to its causes. Slowness renders it vigorous and swiftness makes it feeble. Born of violence, it dies of liberty. The more vigorous it is, the faster it expends itself. It hunts with a fury whatever opposes its destruction; it desires to vanquish and kill the cause of any obstacle and, in vanquishing, destroys itself . . . No motion caused by it endures. It waxes in weariness and wanes in rest.

Using the same rich images, Leonardo compares this loss of the living force to the continual tendency which gravity has towards rest:

If weight seeks stability[5] and if the "forza" is always a fleeting desire, the weight in itself is without fatigue while the "forza" is never exempt from it. The further the weight falls, the more it increases;[6] and the further the "forza" falls, the more it decreases. If the one is eternal, the other is mortal. Weight is natural and "forza" is accidental. Weight seeks stability and ultimately immobility. "Forza" seeks flight and its own death.

How does this continual tendency of gravity towards a final state of equilibrium[7] manifest itself in a mechanism? It becomes evident through the law stating that in a mechanism in motion:

The motor is always more powerful than the body moved.[8]

It is in accordance with this law, for instance, that

The rope descending from a pulley carries more weight and, consequently, tires out more rapidly than the opposite, ascending rope.

This disparity of invariable direction between the motor power and the resistance of the moving body can be found in any mechanism:

If, for example,[9] you want weight *b* to lift weight *a*, the arms of the balance being equal, it is necessary that *b* be heavier than *a*. If you wanted weight *d* to lift weight *c* which is heavier than *d*, it would be necessary to have it descend a larger distance than *c*'s ascent.

And if it descends further, the arm of the balance which descends with it must be longer than the other arm. If you want the small weight *f* to lift the large weight *e*, weight *f* must necessarily move faster and over a greater distance than weight *e*.

It is solely the excess of power which the motor has over the resistance of the mobile body that determines the motion. The larger this excess, the faster the motion.

No power[10] prevails over its resistance except by that amount by which it exceeds this resistance. Or, no motor prevails over its mobile body except by the amount by which it exceeds this mobile body . . . And the more the motion of the mobile body is joined with the "impeto," the larger the "impeto" of the mobile body, which is capable of increasing infinitely.

If a pulley holds two equal weights, these weights will remain immobile. If they are unequal, the heavier weight will descend with a velocity proportional to the weight it has in excess over the lighter one:

If a pound weight impacts a pound of resistance,[11] it will not change its place; it will remain at rest. But, if you add another pound weight to it, it will fall to earth in a given time. If you add to that weight yet another pound weight, all of the weight will descend with twice the velocity.

Thus a self-winding clock is an illusion. The weight with the greater motor power will always descend and when it has reached the bottom of its path, the clock will stop. Consequently, we read the following conclusion[12] by Leonardo:

Against perpetual motion: No inanimate body can move by itself. Consequently, if it moves, it is being moved by an unequal power, that is to say, of unequal time and motion or of unequal weights. And as soon as the action of the first motor ceases, the second ceases too.

Cardan summarizes these thoughts of Leonardo in *De la Subtilité*,

He demonstrates that nothing has perpetual motion.[13]

When one attempts to produce a perpetually moving body,

One is really asking the following: does a motion exist which, by itself and without any added outside stimulus, contains a cause capable of perpetuating it? The problem would be solved if clocks existed which would lift the weights back to their original height instead of initiating the movement which tells time by striking the hour. There are only three kinds of movements capable of settting bodies into motion. Either they tend essentially towards the center of the earth, or they are not simply directed towards the center like the flow of water, or else they derive from a peculiar nature like the movement of iron towards a magnet. It is known that perpetual motion can only be found among the displacements of the first two kinds.[14] It is true that when a weight is pulled more forcefully or held back more energetically than its nature allows, its motion is natural; but it is not exempt of violence. The moving weights in a clock furnish us with an example of these two instances . . . As far as the motion around a circle is concerned, it only occurs naturally in the heavens. And yet, it too is not animated in a uniform fashion. In the case of the other weights, the motion always has its origin in a displacement along a vertical line. Even water is animated by a certain motion along a vertical. Thus, to the degree that the waters of a river are generated by their source, they always descend following the gradient of the river bed. In order to have perpetual motion, the body would have to return to its original position after having been displaced and run its course. The only way this could occur is through a definite excess (of the motor power). Thus the continuity of motion either derives from the fact that the displacement conforms to nature[15] or it will not be able to perpetuate itself. Nothing which is constantly diminishing, unless it is increased by an external action, can be perpetual.

In their thinking Leonardo and Cardan not only reject the possibility of a perpetually moving body but go a step further. They affirm that all observed motions have a common tendency, the tendency of a heavy body to descend as far as possible, to search out the place of its eternal rest. This idea is ever present in Leonardo da Vinci's mind.

Every weight[16] seeks to descend to the center of the earth by the shortest path; and, where there is more weight, there is a greater tendency. The body weighing the most, left by itself, will fall the fastest . . . Weight[17] always pushes toward its point of departure . . . There is only one such point for the weight, it is the earth.

This proposition can serve as a principle explaining the equilibrium and motion of water:

The further a body is from the center of the earth, the higher it is;[18] and the closer it is to the center of the earth, the lower it is. Water does not move by itself if it does not descend and, whenever it moves, it descends. Let these four concepts taken two at a time help me to prove that water which does not move by itself has a surface equidistant from the center of the earth . . . I maintain that no part of the water's surface moves by itself if it does not descend. Since the body of water has no segment of the surface which can descend, it is thus necessary by the first concept that it not descend.

To be sure, water does seem at times to flow spontaneously upstream and some hydraulic machines take advantage of this property. But, in reality, these machines lift only a small quantity of water by means of the descent of a great mass of water. Cardan made the following observation when he analyzed "Archimedes' screw."[19] The argument seems to have concluded in this way: "Water perpetually descends in such a way that, in the end, it will be in a lower position than at the beginning. All of it does not always descend, however, but the large part which does descend pushes the smaller part forcing it upward."

Such is the general law of motion produced by gravity: no body can move upward unless a heavier one descends.

All bodies tend to move downwards,[20] and things high up will not stay there, but in time, will all come down. And thus, in time, the earth will become spherical and consequently completely covered by water.

The entire line of argumentation of Leonardo da Vinci and Cardan stems from the principles of Peripatetic dynamics: namely, the velocity and the force moving the body are proportional as are the velocity of the fall and the weight of the body. Progress in mechanics will sweep away these foundations. Yet, after mechanics has advanced even further, it will again support those earlier conclusions. So far, we have almost exclusively quoted the authors of the 16th century. However, what they tell us has a distinctly modern flavor. Their ideas are very close to those of the physicists who have read Clausius, William Thomson, and Rayleigh. This is because thermodynamics, by completing the over-simplified dynamics deriving from Galileo's *Discorsi*, partially bridged the gap separating the latter from Aristotle's dynamics.

This is not the place to dwell on this reconciliation because it would take us too far from the origins of statics. We have seen how Leonardo da Vinci's most essential ideas were published in the works of Cardan. The vast popularity of the latter will allow Leonardo's thoughts to exert a great influence on the development of science.

At the end of the 16th century, this influence took two directions: one is particularly felt in Italy where it inspired the works of Giovanbattista Benedetti, Guido Ubaldo, Galileo and Torricelli; the other, initiated by Simon Stevin, inspired Flemish science. These two currents will merge in Roberval and Descartes.

CHAPTER V

# THE ALEXANDRIAN SOURCES OF MEDIEVAL STATICS

A geographer wishing to describe a large river basin begins by making a rough draft of the course of the principal rivers which flow into the river basin. After that, he completes this provisional and tentative sketch by detailing all the meanderings of the thousand streams which feed into the main tributaries.

We intend to proceed in a similar fashion in our study on the origins of statics. At the outset, we summarized the abundant and fertile ideas contained in the writings of Aristotle, Archimedes and Leonardo da Vinci. We saw then how the ideas of the great painter had fecundated the 16th century through the fortunate plagiarisms of Cardan.

So far, however, we have only crudely sketched the development of statics from Antiquity to the Renaissance. Numerous details must now be added to the basic outline we have already sketched.

In order to firmly establish these details, we had to impose upon ourselves an onerous labor. We were forced to go through and inventory the numerous manuscripts dealing with statics in the Bibliothèque Nationale and the Bibliothèque Mazarine. We are convinced that this inventory allowed us to discover more than one unknown or ignored source which contributed significantly to the formation of modern science. However, despite our inquiry, many questions still remain obscure. We have no doubt that further research like ours, carried out in major European libraries, will yield to curious minds new findings capable of filling the wide gaps left by us and which could possibly lead to the modification of some of our conclusions.

Before we begin to study the fundamental treatise on statics produced in the Middle Ages by the enigmatic Jordanus de Nemore, we must gather the remains of the writings scattered among the manuscripts concerning the science of equilibrium which were composed in Alexandria. This will be the topic of the present chapter.

## 1. THE WORKS ATTRIBUTED TO EUCLID

The ideas which we intend to trace through a complicated evolution stem in part from Greek science. Not only will we have to clarify the influence which certain passages from Aristotle's *Mechanical Problems* had on Jordanus de Nemore during the Middle Ages, but we will also have to search for the origins of some of these ideas in a fragment attributed to Euclid.

Although Greek Antiquity does not attribute to Euclid any work on mechanics, the name of the great geometer comes up frequently in the books of Arabic authors who wrote on statics and three fragments dealing with mechanics are mentioned as being by Euclid.

The first of those fragments seems to have been unknown to Western geometers of the Middle Ages. It was first reported in 1851 by Dr. Woepcke, who translated it from Arabic and published it in the *Journal Asiatique*[1] as *Le Livre d'Euclide sur la Balance.*[2] The text of this treatise can be found in manuscript number 952.2 in the Arabic collection of the Bibliothèque Nationale. It was written in Chiraz in the year 358 of the Hegira (970 A.D.).

In another copy, says Dr. Woepcke, I found this book attributed to the Banu Musa[3] and collated with the copy by Aboul Hocain Alsoufi. This circumstance could be explained if one supposes that the Banu Musa might have either translated or revised this treatise and that a copyist had omitted the name of the original author.

In support of the view which attributes this treatise to Euclid, Dr. Woepcke points to a reference to Euclid's demonstrations on the lever in a treatise entitled *De canonio*, which can be found in a manuscript in the Bibliothèque Nationale. In the third part of the present chapter, however, we shall have to return to the treatise *De canonio* and to the reference which it contains. We shall see that this passage does not refer at all to the text translated by Dr. Woepcke but to a different text which is also attributed to Euclid.

In opposition to Dr. Woepcke's view, Curtze[4] does not hesitate to consider the treatise on the balance as an Arabic treatise written by one of Muza ibn Schakir's sons, one of the three brothers Muhammed, Ahmed and Alhazen, whose book on geometry was so well known in the Middle Ages. Heiberg shares this opinion.[5] Indeed, Curtze reminds us that according to Steinschneider,[6] one of the three brothers belonging to the Banu Musa wrote a book on the balance. According to Steinschneider, this book was further developed by Thâbit ibn Qurra and the text by Thâbit ibn Qurra which we possess is only an amplification of the text published by Dr. Woepcke.

All of these arguments seem weak to us. We shall have to discuss the book by Thâbit ibn Qurra at length in section 2 of the present chapter. By examining the very explicit accounts of the author, we shall see that his work is not at all an amplification of an Arabic treatise but a commentary on a Greek work. Furthermore, the problems discussed in Thâbit's work are, for the most part, remote from the *Book on the Balance*. Even though it is true that the problem of the equilibrium of the lever is treated in Thâbit's work in the same manner as it is in the text which Dr. Woepcke attributed to Euclid, it is, nonetheless, resolved by a completely different method, to wit, by the method of Aristotle.

Still another argument can be made to prove that the text in question is of Greek origin. Hultsch made the curious remark that the Arabic treatises translated from Greek preserved in a certain sense the stamp of their true origin in the sequence of the letters used to mark the different points on the figures and the diverse magnitudes being analyzed. These letters always have the following order:

a, b, c, *or* g, d, e, z, h, t,

and thus reproduce the order of the Greek alphabet:

$$\alpha, \beta, \gamma, \delta, \varepsilon, \zeta, \eta, \theta$$

This telling sign can also be found in the figures of the treatise published by Dr. Woepcke and thus convinces us that this treatise is a fragment of Hellenic science.

However, one cannot conclude from the above that this fragment should be attributed to Euclid, at least not in its present form. Of the four propositions contained in it, the first two are demonstrated by a sequence of reasoning full of contradictions and lacking any conclusive argumentation. We would, indeed, wrong Euclid if we were to consider this jumble of paralogisms as a product of the logical genius to whom we owe the *Elements*.

It appears that we should consider the treatise under consideration as nothing but the work of a decent geometer which ultimately became disfigured by some clumsy commentator who was trying to demonstrate two indemonstrable postulates and then combine them into the two illogical theorems which we have mentioned. These unfortunate additions are probably not of Arabic but of Greek origin, judging by the arrangement of the letters used in the figures.

Once these parasitic and defective demonstrations are removed from the treatise it appears to start with four axioms. The first two, which are actually formulated in the treatise, are given as follows:

*Axiom I.* When two equal weights are suspended from the two extremities of a straight beam of uniform thickness which, in turn, is suspended at the midpoint between the two weights, the beam remains parallel to the plane of the horizon.

*Axiom II.* When two equal or unequal weights are attached to the two extremities of straight beam which at one of its points is suspended from a fulcrum so that the two weights maintain the beam parallel to the horizon, and if then, we leave one weight in its place at one extremity and draw a straight line from the other extremity of the beam which forms a right angle to the beam on either side of the beam and if one suspends the other weight from any point at all on this line, the beam will remain parallel to the plane of the horizon. This is the reason why the weight does not change if one shortens the strings of one of the two scale pans or lengthens the strings of the other.

The pseudo-demonstrations of propositions I, II and III imply the following two axioms:

*Axiom III.* If the weights are maintaining the beam of a balance parallel to the horizon and if one suspends an additional weight to the beam's point of suspension, the beam remains parallel to the horizon.

*Axiom IV.* If any number of weights maintain the beam of a balance parallel to the horizon, and if Z and D are two of these weights suspended from the same arm of the beam and if one moves weight Z by a given length away from the point of suspension of the balance and if one moves weight D by the same length towards the point of suspension, then the beam will remain parallel to the horizon.

This axiom, which renders the demonstration of proposition III logical, leads the author to the notion of the power of weight, a notion which we would call today the moment of the weight with respect to the point of suspension. This notion shows the author that this power diminishes by degrees proportional to the diminution of the distance between the weight and the point of suspension of the balance.

These axioms produce in proposition IV an elegant demonstration of the law of the lever. Let us summarize this demonstration in a few lines.

Imagine a lever AB with C as the point of support (Fig. 15) and

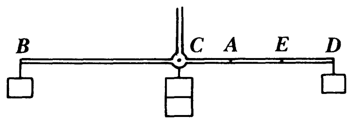


*fig. 15.*

suppose that the arm of the lever CB is three times the length of the arm of the lever AC. A weight P is suspended at B. What weight does one have to suspend from A in order to put the balance in equilibrium?

Extend CA by the length AD in such a way as to have CD = CB. Now AD will be twice the length of CA. At D, suspend a weight equal to P and at C, two other weights equal to P. True to our first three axioms, the beam will be in equilibrium.

According to our fourth axiom, we can move the weight which previously was at point D to E, the midpoint between A and D. And we can move one of the weights which previously was at point C to point A. The beam will remain parallel to the horizon. It will also remain parallel to the horizon if one moves the weight one had already moved to point E along with the second of the two weights, previously suspended at C, to point A. Thus the beam remains parallel to the horizon if one suspends a weight P at point B and a weight triple that of P at point A. This demonstration, which is easy to generalize, leads to the well-known law of the equilibrium of the lever.

Thus it appears to us that the fragment brought to light by Dr. Woepcke indicates, under various disguises and alterations which undoubtedly go back to Greek Antiquity, an interesting relic of Hellenic science. The author proposed to demonstrate the law of the equilibrium of the lever starting not from a general principle of dynamics, as Aristotle does, but by means of postulates rendered seemingly self-evident by their simplicity and by everyday experience. However, the author's method is the same method by which Euclid has given us superlative models in his *Elements*. It is the same method which Archimedes used when he undertook to deal with statics or hydrostatics. However, the application made of this method by Euclid is vastly inferior to that made of it by Archimedes in demonstrating the same law of the equilibrium of the balance. It is possible that the work on the balance of which we possess only a curiously deformed copy predates Archimedes and belongs to the time of Euclid. We shall now turn our attention to another text, which is also attributed to Euclid.

This fragment has been known for a long time. Herwagen (Herwagius) included a Latin translation of it in the edition of Euclid's works which he published in Basel in 1537. This exact same translation was also included in subsequent editions of the same works published in Basel in 1546 and 1558. Gregory included the translation with an implied and discreet criticism in the edition of Euclid which he published in Oxford in 1747. In 1565, Forcadel published in Paris the *Book on Weights* falsely attributed to Archimedes. He appended to this work a French translation of the Latin text published by Herwagen.

Herwagen[7] only furnishes us with scant information about the origin of this fragment which he entitled *De ponderoso et levi*;

When this work was nearing completion, someone brought me a small book or rather a fragment (because it appears to be mutilated) entitled *De ponderoso et levi* and I appended it.

Recently, Curtze discovered in Dresden, in a manuscript with the catalogue number Db. 86, a Latin copy of the short treatise attributed to Euclid. He published it[8] facing a slightly different version of the text published by Herwagen.

This publication leaves no doubt about the Greek origin of the fragment. The letters used to designate the magnitudes of the analysis follow each other in the order a, b, g, d, e, z, h, t, sometimes slightly altered by the copyist who reads r, for example, for z.

The exact title of the manuscript fragment is: *Liber Euclidis de gravi et levi et de comparatione corporum ad invicem.*[9] This *Book on the Heavy and Light* bears no analogy to the *Book on the Balance* discovered by Dr. Woepcke. It deals with the fundamental principle of Aristotelian dynamics of which it gives the most precise commentary which we possess. It proceeds, in effect, in the manner of Euclid, with definitions and theorems.

Let us quote from these definitions the following one which clearly bears a Peripatetic stamp:

One calls those bodies equal in power (virtus) which travel through equal spaces during equal time either through the same air or the same water. Those bodies which travel through equal spaces during different times are called different in power (fortitudo). And the body with the most power (virtus) is the one which took the least time.

The words *virtus* and *fortitudo* obviously have the same meaning as the Greek words used by Aristotle under similar circumstances.[10]

We should not be surprised to see the author of *The Book on the Heavy and Light* take into account the influence of the medium, nor should we necessarily read into this the influence of Archimedes' discoveries. Aristotelian physics also admitted that the medium affects the velocity of a falling body. The less dense the medium, the greater the velocity. In a vacuum, the velocity would become infinite. From this, the Stagirite deduces one of the main arguments against the possible existence of the void. This is not to say, however, that no valid arguments can be cited which favor the view that the *De ponderoso et levi* should be dated after Archimedes. The manuscripts from the Middle Ages include a small but elegant treatise on the determination of specific weights. Our Bibliothèque Nationale possesses at least three copies in its Latin collection catalogued as Mss. 7215, 7377b and 10252. Curtius Trojanus printed a very defective edition of this treatise right after the *Jordani opusculum de ponderositate*,[11] which he published in Venice in 1565. We are sure that this treatise, sometimes attributed to Archimedes, postdates him. The relationship of this small treatise to the *De ponderoso et levi* is, however, very clear. Right from the start, the pseudo-Archimedes' treatise repeats some of the same definitions in the *De ponderoso et levi.* Perhaps this treatise was originally simply appended to the *De ponderoso.* In any case, there seems to be no doubt that these two writings come out of the same School. Even if they are not from the very same period, the pseudo-Archimedes' treatise was probably written by someone continuing the *De ponderoso.* Father Forcadel had his reasons when he combined these two fragments which he published in French in Paris in 1565 as Archimedes' *Book on Weights.* The author of *The Book on the Heavy and Light* defines below what he calls "bodies of the same category" which are what we call today "bodies of the same specific weight:"

> One calls bodies of the same category those which, taken under equal volume, possess the same power. If bodies of equal volume have different powers with respect to the same air or the same water, they are called bodies of different categories. The body with the most power is called the body with the greatest density.

However, the properties which the Greek author associates with this notion of bodies of the same category are very different from those which we attribute today to bodies of the same specific weight. He actually demonstrates (in propositions II and III) that bodies of the same category possess powers proportional to their size. That is to say, that according to his own definition the velocities of their fall are proportional to their volume. Although contrary to what we claim since Benedetti and Galileo, such a law is essential to Aristotelian physics. The Greek author implicitly recognizes this postulate in the demonstration which he gives.

When two heavy bodies are combined into one, their velocities of descent are added.

Bendetti's claim to fame rests on his destruction of the credence accorded this postulate throughout Antiquity.

Reduced to what Herwagen and Curtze have published from *The Book on the Heavy and Light*, it appears to be the most precise account in our possession of the principles of Aristotle's dynamics and seems to have nothing at all to do with the science of equilibrium which is our concern here. Herwagen, however, already warned us that the work seemed to be only a mutilated fragment. Is it possible to find some traces of the propositions which, undoubtedly, must have been part of the original work?

Under the following title: *Incipit liber Euclidis de ponderibus et levitatibus corporum ad invicem*,[12] a manuscript in the Bibliothèque Nationale[13] and presumably dating back to the 16th century, contains a reply to the question about the work which occupies us here. At the end of the manuscript one can find the following remark made by the copyist: "Explicit, quia plus non invenitur,"[14] which confirms that *The Book on the Heavy and Light* is a mutilated fragment.

This new text contains with insignificant variations almost everything published so far by Curtze. However, another fragment is inserted in this text in a most curious way. What Curtze refers to as the fourth demonstration is barely outlined, when the text becomes incomprehensible. Then the terms no longer have any relation to the preceding ones. Soon, however, one recognizes that the incomplete demonstration is joined to the last part of the formulation of a different proposition.

We shall soon relate how and by what fortunate circumstances we were able to find the complete text of this proposition to which we will assign the letter B. Our fragment contains the brief demonstration of it followed by the very brief formulation of three other equally new propositions. We shall call them propositions C, A and D according to the order which they follow in our manuscript. The manuscript ends with the fourth and last proposition of the fragment published by Curtze.

Propositions A, B, C, and D, taken in that order, present a very logical continuation of *The Book on the Heavy and Light* and are of great importance to the history of statics.[15] Proposition A can be stated as follows:

Assume that the beam of a balance is originally parallel to the horizon. If its two extremities turn in the same time, their powers will be in the same ratio as the paths they describe.

A very short commentary is attached to the proposition in which the word power (virtus) has a meaning close to its use in *The Book on the Heavy and Light*. There is no doubt whatsoever that this commentary derives ultimately from the demonstration of the law of the lever which Aristotle gives in his *Mechanical Problems*. But on closer observation one can see that the two demonstrations proceed, so to speak, in an inverse manner. Aristotle admits in principle that the power of a weight suspended from a lever is proportional to the velocity at which this weight moves when the lever is turned. From this principle, Aristotle deduces the condition for the equilibrium of two weights suspended at unequal distances from the point of support. Our author proceeds quite differently: What was a first principle for Aristotle becomes for him a proposition in need of demonstration. Furthermore, in this demonstration our author limits himself to proving that the paths travelled by the extremities are in the same ratio as the lengths of the lever arms. The demonstration becomes conclusive only if one affirms as previously proven this proportionality between the power of a weight suspended from a lever and the distance of this weight from the point of support.

Therefore, our proposition A had to be preceded by an evaluation of this power of a weight suspended from a beam of a balance and by establishing the law of equilibrium of a lever. Its very structure alerts us to the lacuna which precedes it and points to the kind of reflections necessary to fill this lacuna. Therefore, we are now compelled to make a comparison. *The Book on the Balance* discovered by Dr. Woepcke would precisely fill this lacuna. This book, once disemcumbered of the false demonstrations which have altered it, would furnish us with the basis for the law of the lever and the proof that a weight suspended from a beam of a balance has a power or a weight potential which is proportional to the distance from its point of suspension. Our proposition A and *The Book on the Balance* published by Dr. Woepcke, now seem to synthesize in a most natural way.

The examination of proposition B only confirms this view which sees a kinship between the fragments. This proposition is set forth in the form of a problem obviously suggested by the use of the Roman balance.

If one takes a homogeneous cylinder and divides it into two unequal parts and suspends it at this point of division, what weight must one suspend from the extremity of its shortest arm in order to establish equilibrium?

Our author suggests that we measure out from the point of suspension along the longer lever arm a length equal to the shorter arm and that we call the remaining length ab. The weight sought will be in the same ratio to the weight of segment ab as the distance between the midpoint of segment ab and the point of suspension is to the length of the shorter arm.

In order to justify this rigorous solution, the author simply makes the following short remark:

Because if one concentrates the material of segment ab into a single mass, and if one places it at the midpoint of the space which it previously occupied, the beam remains in equilibrium just as before.

The demonstration implies this principle:

A homogeneous cylinder used as if it were the arm of a lever has the same effect as an equivalent weight suspended from that arm and attached at a point located at the center of the cylinder.

It is obvious that the author of our fragment establishes proposition C in order to justify this essential principle. This proposition says:

If an arm of a beam carries four equal and equidistant weights, these weights are equivalent to a single weight equaling the sum total of the four weights and suspended at the midpoint of the interval which they occupy.

One sentence indicates how the demonstration of this proposition can be deduced from the law of equilibrium of the lever. One can guess how our author establishes the transition between proposition C, established in the above way, and the principle which proposition B depends upon. He undoubtedly decomposed the cylinder into many small and equal slices and claimed for each of these slices what he wanted to prove for the entire cylinder.

This rather lax procedure, as we will see, is constantly used by geometers working on these same types of problems. The demonstration of proposition D leaves no doubt that our author also followed this procedure implicitly.

This is the proposition:

The fact that the beam of a balance is a heavy cylinder has no bearing on the behaviour of the weights attached to it.

Indeed, the text says more or less that the weight of a given segment of the cylinder will be proportional to the length of that segment's axis. Therefore, if the beam is divided into equal segments, you can choose a weight at any point on one arm of the balance which will then correspond to an equal weight on the other arm at an equal distance from the point of suspension.

The analysis of our four propositions demonstrates their importance in the history of mechanics. It was thus of great interest to discover other texts which verified the contents of the first text and which allowed us to fill the existing lacunae in it.

Under the number 3642 (previously 1258), the Bibliothèque Mazarine has a manuscript from the 13th century, or more precisely, a compilation of several manuscripts of varying format and handwriting.

Of these manuscripts which once must have been part of a very valuable collection which today is unfortunately very incomplete, the first starts with this title: *Liber Arsamidis philosophi. — Astrologium Robi. — Planispherium Tholomei. — Liber Thebit. — Elementa Jordanis. — Liber Euclidis. — Divinationes.*

There follows a long table of contents which gives us the list of numerous treatises contained in the collection; it starts with these words:[16]

In isto volumine libri subscripti continentur. cum capitulis eorumdem et figuris.

Note the following excerpt pertaining to works which will be occupying our attention:[17]

Incipiunt *elementa Jordani super demonstrationem ponderis*, cum cartulis et figuris.
Incipiunt excerpta de *libro Thebith de ponderibus*.
Incipit *liber Euclidis de ponderibus* secundum terminorum circumferentiam.
*Divinationes.*
*De Compoto.*

The title of this *The Book of Euclid on Weights According to the Circumference Described by the Extremities* seems to be a very clear allusion to our proposition A, the very text which interests us here. The sheets which should contain this *Book of Euclid* have unfortunately been lost. The title, announced in the table of contents as: *Incipiunt elementa Jordanis super demonstrationem ponderis*, is on the right-hand side of the twelfth sheet. The above entitled work continues on the back of the same sheet and does not end at the bottom of the page. However, on sheet 13, we suddenly find ourselves in the middle of the treatise: *De compoto*.

Most fortunately, we were able to find a copy of the missing pages in the manuscript from the Bibliothèque Mazarine. This copy is inserted in a manuscript preserved today in the Bibliothèque Nationale,[18] and was formerly the property of the Sorbonne as a gift from:

Magister Franciscus Guillebon, Parrhisinus, Socius Sorbonicus et Doctor Theologus.[19]

This collection starts exactly like the manuscript from the Bibliothèque Mazarine with the *Liber Arsamidis philosophi de mensura circuli*[20] and the following remark is made at the end of the treatise:

Explicit liber Arsamidis. Scriptum 1519.[21]

Similarly, the *Elementa Jordanus* which are included in the same collection end with the following remark:

Finis. 1519. 2 sa Maii.

These remarks furnish us with the date of the scientific collection which was in the possession of Master François Guillebon.

Following Archimedes' treatise on the measurement of the circle, the collection contains three short works with the following titles:

Incipiunt *elementa Jordani super demonstratione ponderum.*
Incipit excerptum de *libre Thebit de ponderibus.*
Incipit *liber Euclidis de ponderibus* secundum terminorum circumferentiam.[22]

The wording of these titles and their order suffice to suggest that the collection, copied during the 16th century and donated to the Sorbonne by Master François Guillebon, reproduces word for word a part of a collection put together during the 13th century and whose remnants remain in the Bibliothèque Mazarine. We can in fact furnish an absolutely convincing proof for this view.

The scribe to whom we owe the collection at the Bibliothèque Mazarine was very skilled in the use of both the pen and the brush. He excelled in the embellishment of the majuscules and just as he was about to copy the *Elementa Jordani*, he enlivened the margin of the parchment whith an amusing figure drawn with a few spirited brush strokes. However, the geometrical arguments which he had to reproduce in an elegant script must surely have presented to him inscrutable mysteries. Even if a sheet were missing in the original text, the copyist, unaware of the incoherence thus created, blithely continued his task and stuck together two separate and different pieces of writing. This was his procedure in copying the *Elementa Jordani*. In the middle of a demonstration in this treatise, the line of reasoning abruptly ends. The beginning of a sentence from the *Elements* is followed by a line of reasoning taken from the treatise *De canonio* which we will discuss in section 3.

The scribe — he must have been a German to judge from his handwriting — from whom Master François Guillebon had received his collection was not struck by the bizarre seam connecting two incoherent bits of text and servilely reproduced it, just as some Chinese tailors who use an old garment as their model for a new one, carefully reproduce the tears and spots on the new garment. This strange error rendered virtually useless to the geometer a writing copied in such a strange way. But it was also a fortunate error because it assures us that we possess a slavishly faithful reproduction of the missing leaves from the Codex Mazarineus. The *Liber Euclidis de ponderibus secundum terminorum circumferentiam* has as its subtitle: *Liber Euclidis de ponderoso et levi et comparatione corporum ad invicem*, and is followed by the short work published by Herwagen and republished by Curtze. But the four propositions which the title seems to promise are missing. These four propositions were detached from it and they are called *Excerptum de libro Thebit de ponderibus*. This error is quite normal. The book by Thâbit ibn Qurra to which the following section is devoted, is in the form of an Arabic commentary on our four propositions. Thus they could have been mistaken for an excerpt from this commentary. The collection of Master François Guillebon contains, nonetheless, the complete text of these four propositions in the same sequence in which we have already found them: B, C, A, D.

Furthermore, we found these propositions a second time or at least the first three, in a manuscript[23] of Italian origin, which seems to date back to the end of the 15th century. One of the sections contained in this manuscript begins with these words: *Incipit liber de ponderoso* *et levi.* Those words seem to introduce the fragment known since Herwagen. In reality, this fragment is replaced by Jordanus de Nemore's *Liber de ponderibus*. However, to this latter work, our propositions B, C, A were appended.

In conclusion, the texts attributed to Euclid which we have examined seem to furnish us with three fragments, more or less well-preserved, of Greek mechanical science.

The first of these fragments is the *Liber de ponderoso et levi.* It seems to be preserved in its entirety and develops with great precision the fundamental principle of Peripatetic dynamics.

The last fragment is composed of the four propositions which we have discussed. Their abbreviated demonstrations as well as their illogical order indicate serious mutilations. However, we can consider them a fortunate attempt to harmonize the law of the lever with Peripatetic dynamics in order to account for the weight of the lever itself and to establish a theory for the Roman balance.

In order to harmonize these two works, a straightforward theory of the lever seems necessary. *The Book on the Balance*, brought to light by Dr. Woepcke might just be the work capable of integrating the two preceding fragments. However, this work has been rendered almost completely unrecognizable because of clumsy alterations.

The remnants which we have examined in their damaged and worn out forms could be made to fit together and form a sort of treatise in which Aristotle's and Archimedes' methods would be united. Furthermore, the solution to a problem neglected by the great geometer from Syracuse: the problem of the Roman balance, would be delineated. Was this treatise the work of a single geometer or of several individual mathematicians? If so, is the author of the *Elements* one of them? These are difficult questions to resolve, but we shall soon point out some small clues for their resolution.

Montucla wrote:[24]

> We shall say nothing about the book *De ponderoso et levi* which is also attributed to Euclid. One can only compare its content to the initial stammering of a nascent physics.

This judgment certainly would give a very false impression of the importance due the work whose vestiges we have just discussed. We shall see that this work exerted a profound influence, first on Arabic science, and then on Western science.

## 2. THE *LIBER CHARASTONIS* PUBLISHED BY THÂBIT IBN QURRA

Most libraries[25] own a manuscript entitled: *Liber Charastonis, editus a Tebit filio Corae.*[26] The name of the publisher, or rather of the commentator, is that of one of the most illustrious Arab geometers. Thanks to Wuestenfeld,[27] we possess a number of precise details about his life.

Thâbit ibn Qurra ibn Marwân ibn Kârâya ibn Ibrâhim ibn Mariscos ibn Salamanos (Abû al Hasan) al Harami was born in Harran, in Mesopotamia, in 836 A.D. First, he was a money changer before he devoted himself to science. In Baghdad he acquired a great reputation as a mathematician and astronomer at the same time he was devoting himself to the study of Greek, which he soon used with the same ease as Arabic or Syrian. This perfect knowledge of Greek allowed him to translate and comment on the works of the masters of Hellenic science, Apollonius of Perga, Euclid, Archimedes, Ptolemy and Theodosius. He also produced a great many original works on arithmetic, geometry, astronomy and astrology. After a while, he returned to his native city Harran, where he was to encounter various ordeals. He belonged to the sect of the Sabians, but since he was trying to break away from some of their doctrines and practices, he found himself excommunicated. He returned to Baghdad where he remained for the rest of his life. He enjoyed the great respect of Caliph Almu'tadid (892—902) who granted him a close friendship. Thâbit ibn Qurra died in Baghdad in 901.

Thus we have precise information on the author of the commentary to which we will now turn our attention. We probably even know the translator himself. According to Prince Boncompagni[28] it was Gerard of Cremona (1114—1187), who translated a certain *Liber Charastonis* into Latin from the Arabic. Steinschneider[29] quite correctly thought that his translation was the one we possess so many copies of.

Yet, despite the fact that we know for certain who the author of the commentary was and who the probable translator was, our confusion is extreme when we try to interpret the title. How are we to translate *Liber Charastonis*? Should it be the *Book of Karaston* or the *Book on the Balance*? Is Karaston the name of a Greek geometer or the Arabic name for the Latin *statera*, our Roman balance? Both of these views have had their advocates and the choice is difficult to make.

All the scribes who reproduced the version attributed to Gerard of Cremona interpreted Charasto, Carasto, Karisto or Baracto (because all of these spellings occur and are sometimes mixed in the same copy) as the name of an author. The capitalized first letter of the noun shows this as well as the structure of the sentence which marks the beginning or the end of the book, to wit: *Incipit liber Karastoni de ponderibus*, states the Ms. 10260 (Latin) of the Bibliothèque Nationale.

Sometimes the scribe even attempted to guess who this geometer might be. Such is the case with Ms. 7310 (Latin) dating back to 1604 and kept in the Bibliothèque Nationale. This manuscript, whose fragments seem to have been copied from similar fragments contained in Ms. 10260, contain specifically the *Liber Charastonis*. The scribe to whom we owe this compilation had first worded the title in the following way: *Incipit liber Baractonis de ponderibus*. However, since he knew of no Greek geometer with such a distorted name, he crossed out Baractonis and put Eratosthenes in its place, but used the spellings Baracto, Carasto, Karasto and Charasto throughout the text that followed. These diverse spellings caught the attention of an annotator who wrote the following words on the back side of the first sheet:[30]

> Eratosthenis, sic legitur in titulo. Verum, initio libri, auctor ex quo translatus est nominatur aliter et, versus finem, diserte dicitur Charaston.

For the annotator it was Charaston[31] who was the author upon whose work Thâbit ibn Qurra commented upon.

Some modern bibliographers have shared this opinion. Heilbronner, in an *Index* of his, lists Carasto as the name of an author and interprets in the same way as Hammer in his history of Arabic literature the words Kitâb el Kurstûn which Latin translators rendered as *Liber Karastonis*.

According to Steinschneider[32] this opinion is comparable to the error of the monkey who took the Piraeus for a man's name. Karastûn could be simply the debased form of the Arabic word Karstûn. According to Fleischer, whom Steinschneider quotes, this little used word might have come via the Syrian from the Greek word *χείρ*, meaning hand and might signify the Roman balance. Kitâb el Karstûn, *Liber Karastonis*, should not be translated as the Book of Karaston, but as the *Book on the Roman Balance*.

The interpretation of the word Karaston which Steinschneider proposed, has been adopted by Heiberg[33] and by Curtze.[34] According to the latter author, the *Treatise on the Balance*, discovered by Dr. Woepcke and attributed by him to Euclid (although certain other manuscripts refer to this work as being authored by the "three brothers"), is identical to the Kitâb el Karstûn written by the Banu Musa and to which Steinschneider has called our attention based on his research on Casiri and Hammer. Furthermore, a comparison between the *Liber Karastonis* of Thâbit with the *Treatise on the Balance* translated by Dr. Woepcke would show that the first of these two works was only development of the second.

Reading the *Liber Karastonis* might be the best way to clear up this question. (I don't know if the scholars ever thought of this.) It is beyond any doubt that at the end of the work the word Charasto or Karasto should be understood as the Roman balance. After Thâbit has demonstrated how to calculate the weight of the pan which, suspended from the small arm of the Roman balance, compensates for the excess weight of the longer arm, he adds:

> We shall divide the larger arm into segments which will have a known ratio with the small arm. In accordance with the previously established rule for the case of a beam reduced to a single line, then we will know the weight of a body suspended from any segment of the charaston made (generati carastonis) in this way.

A very different impression emerges by reading the dedicatory epistle which Thâbit addresses to someone he refers to as his brother. Here is the translation of the beginning of the epistle:

> May God grant you a long life and multiply your share of well-being so that I may not be deprived of a brother like you who incites minds by his curiosity, who inspires the soul to speculation, who, by his own nature, inspires science, who improves his own mind, who rejects whatever is unassimilable in a subject and exposes what is necessary of that subject.
>
> I have read, dear brother, your letter on what I stated concerning your examination of the *Causae Karastonis* with all the clues which you pointed out in it and with all the figures which you constructed with respect to it. You discovered these ideas by putting aside all other research and by making the examination of this work your sole preoccupation. You have reflected well on this subject. I tried to test some of these obscure passages which are incomprehensible to an intelligent being. I took into account, dear brother, the difficulties of translation and the hazards of reproduction by the scribes. I could not make up my mind for a long time on this subject, because you yourself were unable to render your opinion free of all erroneous interpretation. You have asked me to give you an exposition of this work written in a simple language where the intentions of the work are made clear by methods which shorten the discourse and simplify the difficulty of its argumentation. Therefore, I shall respond on the subject you have asked of me and I shall finally speak to you with sufficient information and solid demonstrations about those things you desired to have clarified. You shall know where falsehood

lies and how it multiplied until it had taken over the entire work. You already know how widespread it is. May God direct you and illuminate your heart's wisdom. Let the absence of geometrical figures in the *Causae Karastonis* be no excuse . . .

It is clear from this passage that Thâbit wants to restore in a coherent form a work which had become incomprehensible because of translators and scribes. Therefore, it is not a question of an Arabic work needing commentary — a treatise deriving from the Banu Musa, for instance — but rather a Greek text. It seems very difficult to interpret the sentences just quoted, without seeing in the words *Causae Karastonis* the title of the work and the author's name and without translating these words as *The Book on Causes* by Charaston.

Thus, if in different passages at the end of our manuscript the word Charaston clearly signifies the Roman balance, it appears that at the beginning of the same work it designates the name of a Greek mechanician. Is there any cause for astonishment in this double meaning? Do we not see everyday how an instrument in the mechanical arts bears the name of its inventor or of the person who perfected it? Will our descendants perhaps not also find some difficulty in deciding if Vernier was the name of a person or of a divided scale? And do we not roughly estimate weight on the Roberval in our laboratories while Roberval remains the name of an illustrious geometer?

It seems to us quite possible that Charaston designates the name of a Greek author who had written a treatise on the Roman balance to which his name was subsequently given. That would explain the existence in Arabic of this word of Greek origin — karstûn — to designate a balance.

Is it possible to discover any further trace of this Greek geometer? We read in Montucla:[35]

Several of Ptolemy's books contain this dedication: ad Syrum fratrem. This is proof that Ptolemy had a brother of this name who was probably versed in astronomy and who might even have collaborated with him on his observations and calculations. I also found that he had a son, named Heriston, about whom we could say the same. We discovered this in the title of an extremly rare book, printed in Venice in 1509, entitled: *Sacratissimae astronomiae Ptolomei liber diversarum rerum quem scripsit ad Heristonem filium suum* . . .[36]

Kastner, who saw this extremely rare book,[37] has given us a detailed description[38] and a summary. It is written in Gothic characters bearing the following complete title: *Sacratissimae astronomie Ptholemei liber diversarum rerum, quem scripsit ad Heristonem filium suum, tractans compendiose de diversis rebus, ut habetur in tabula que est in principio istius libri.* MDVIII. Felicibus astris prodeat in lucem, ductu Petri Liechtenstein. Cum privilegio.[39] At the end of the book, one finds the following lines: Explicit liber diversarum rerum Ptholemei philudiensis Alexandrini, astronomorum principis clarissimi. Anno virginei partus 1509, die tertio aprilis. Venetiis, in edibus Petri Liechtenstein Coloniensis Germani.[40]

Thus, according to the book printed by Peter Liechtenstein, Ptolemy had a son named Heriston, who was well-versed in astronomy. Heriston is not Charaston, but the difference is slight, especially to those familiar with the strange distortions Greek names undergo when translated into Arabic and from Arabic into Latin. Steinschneider[41] has called our attention to some of these distortions:

Hero became Iran and Iranius, Menelaüs was changed into Milleius; Archimedes appears at times as Arsamites, at other times as Aramides and also as Archimenides.

We have ourselves come upon the following names for Archimedes: Arsamides, Arsanides, Ersemides (Bibliothèque Nationale Ms. 16649 (Latin) Bibliothèque Mazarine Ms. 3642), Arsamithes (Bibliothèque Nationale Ms. 9335 (Latin), Alaminides (Bibliothèque Nationale Ms. 10525 (Latin)). It would hardly come as a surprise to see Charaston changed to Heriston.

Yet the real name of the author whom we are studying here is neither Charaston, nor Heriston. The name should most likely be read as: Charistion.

Any Greek dictionary will give the following information for the word *χαριστὶων*, a kind of balance invented by Archimedes. Furthermore, Bailly's dictionary[42] tells us that the term was used by Simplicius in his *Commentaries* on Aristotelian physics.

The only passage where Simplicius[43] used this word furnishes us with valuable information. In this passage, Simplicius comments on the fundamental axiom of Peripatetic dynamics, which is the ratio between the motor power, the weight moved and the space travelled through in a given time. When Simplicius proposes to discuss the restrictions one must apply to this axiom he says that:

By establishing the ratio between the motor power, the weight moved and the space traversed, Archimedes came up with an instrument capable of weighing, which is called the charistion.

Thus at the time of Simplicius (6th century) the Roman balance was not only referred to by the term "charistion," but it was common practice to relate its theory to the principles of dynamics established by Aristotle. This is precisely the goal of the various works we are now analyzing. Furthermore, the real author of these reflections on mechanical problems had been forgotten, because they were attributed to Archimedes, although the illustrious Syracusan had used in the analysis of similar problems quite different methods.

It appears beyond doubt that this author's name was Charistion, and that through a phenomenon quite frequent in the mechanical arts, the instrument took on the name of its inventor or the name of the person who studied it. How else can we explain the fact that the root *χάρις*, Greek for grace, could have furnished the name for a balance? On the contrary, it is not in the least surprising to see this same root word furnish a proper name since it had already produced[44] the woman's name *Χαριτώ* and as names for men: *Χαρισθένης*, *Χαρισιάδης*, *Χαρίσιος*, *Χαρίστιον*, and *Χαρίτων*.

It is true that certain authors did not think of Charistion as the name of the Roman balance, but as the windlass designed to haul boats along the shore. Yet, Simplicius' text is explicit, and the words "an instrument capable of weighing" can only be understood as implying a balance.

It could be argued that *charistion* also came to designate the name of a device used in ports, but it seems more probable to me that this is the result of a relatively recent misunderstanding.

According to Simplicius, it was the invention of the charistion which caused Archimedes to exclaim:[45]

Give me a firm point and I shall move the earth.

Similarly, Tzetzes[46] attributes these words to him:

Give me a place to stand and I will move the entire world with a charistion.

These words apply admirably to the Roman balance, where a small weight, suspended from the longer arm of the beam, will lift a large weight suspended from the shorter arm.

However, other authors do not believe that these words of Archimedes had any reference to the Roman balance. Plutarch, who quoted Archimedes as follows:

Give me a place where I may stand and I will move the earth.

does not make any mention of any machine to which he refers. More explicitly, Pappus claims that Archimedes in his joy at having constructed a powerful windlass cried out:

Give me a place where I may stand and I will move the earth.

Furthermore, Pappus[47] gives a description of the windlass which permits a small force to overcome a large resistance by means of multiple gears. Furthermore, he assures us that he is borrowing this description from Hero of Alexandria.

The instrument is actually described by Hero of Alexandria[48] who, however, does not refer to it as coming from Archimedes. But neither Hero of Alexandria nor Pappus refer to this windlass with the term "charistion" which they certainly would have done if it had been so named in their time at Alexandria.

To be sure, the passages which interpret the celebrated statement of Archimedes on the possibility of moving the world as referring to the "charistion" have been questioned by those who see in this word an allusion to the windlass. It was, therefore, concluded quite erroneously that the "charistion" was a windlass. We can't name with certainty the author responsible for this confusion. We know only that it was accepted without question by Stevin,[49] who informs us that the description of the "charistion" had been discovered by Jacques Besson. As far as we are concerned, Stevin's opinion has no foundation and "charistion" designates the Roman balance.

Let us assume that the Greek book which Thâbit ibn Qurra set out to restore was the work of an Alexandrian geometer by the name of Charistion, who was probably the son of Ptolemy. Let us further assume that the Roman balance studied by Charistion took on his name first in Greek, then in Arabic where it was called "karstûn." Finally, let us assume that the whims of translators produced from this word the two names Charaston and Heriston.

The study of Thâbit's work will give us further information. At the outset, Thâbit tells us that the work which he is about to comment on is closely related to the book attributed to Euclid:[50]

Hoc autem capitulum innixum est super librum qui dicitur *Liber Euclidis*.

Thâbit refers anyone seeking detailed information to this book. All he does, by way of introduction, is to cite from this book everything necessary to the understanding of the work he is about to analyze.

These declarations immediately precede the following statement:

The spaces which two moving bodies travel through in a given time are in the same ratio as the powers of these two moving bodies.

No demonstrations follow this statement; he only uses one example to clarify it. However, this statement formulates the fundamental axiom of Peripatetic dynamics in the very same terms as those used in the treatise *De ponderoso et levi*. We are thus certain that Thâbit knew the short work *De ponderoso et levi* and we are further certain that this work already bore Euclid's name at the time of Thâbit and that the *Causes of Charistion* were based on this book.

To get from this axiom to the law of the equilibrium of the lever, Thâbit proceeds by two propositions which develop simply and with great precision the demonstration of this law as it is formulated in the *Mechanical Problems* of Aristotle.

In the process of these demonstrations, as well as in the following two propositions, it is assumed that the lever is weightless:

If the beam of a balance in equilibrium carries two equal weights suspended at unequal distances from the point of support, it is possible to retain equilibrium by replacing these two weights with a single weight equal to double the weight of one of the weights and by suspending it midway between the initial two points of suspension.

In the same way, if one of the arms of a beam of a balance in equilibrium carries a certain number of equal weights suspended at equal distances from each other and if all these weights are then replaced by a single weight, equal to the sum total of the weights and suspended from the midpoint between these weights, the balance remains in equilibrium.

Thâbit establishes the demonstration of this general statement by supposing that there are four weights to be combined. Yet, it is easy to see how the demonstration can be generalized.

These propositions which hold true for beams without weight cease to be true if the beam is a rod which has thickness and weight and two unequal arms. Thâbit proposes to demonstrate how this case, the case of the Roman balance, can be derived from the consideration of a beam without weight.

To this end, Thâbit considers first a beam reduced to a line without any thickness and which is partially covered by a heavy cylinder. He proposes to prove that this cylinder is equivalent to an equal weight suspended at the point which marks the center of the cylinder. Basically, the demonstration which is deduced from the preceding proposition amounts to admitting for certain portions of the cylinder what one wants to prove for the entire cylinder.

Once this proposition is admitted, it becomes easy to demonstrate the following one:

A cylindrical, homogeneous, and heavy beam *ab* (Fig. 16) with unequal arms *ag, bg*, can be maintained in a horizontal position by suspending a given weight *e* at the extremity of the short arm *ga*. If *bd* is the segment by which the long arm exceeds the length of the short arm and if *u* is the midpoint of *bd*, then weight *e* will be to the weight of segment *bd* as the length *gu* is to the length *ga*.

Thâbit deduces from this the following rule:

If *p* is the total weight of the beam, the weight *e* is given by the formula

$$e = p\frac{bd}{2 \cdot ga}$$

Since this weight is known, a pan having exactly this weight can be suspended from the short arm of the balance or else an additional load of this same weight can be put at the extremity of this arm. The *karaston* constructed in this fashion can now be considered a beam without weight.

And now, dear brother, adds the Arab geometer, I have shown you what can lend support to the work of your mind, what can aid you in the effort of knowledge and what can give you healthy ideas in the light of the truth and what can entice your soul to pursue its study . . . This art is supported by demonstration and verified by experi-

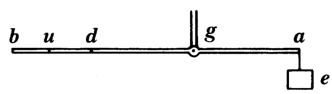


*fig. 16.*

ence. Thus, when you have made use of what has been demonstrated, when you have understood through demonstrations what we formulated at the outset, then with what we have disclosed to you, you will be able to break the shackles of hesitation, to avoid all erroneous assimilation and to see clearly where truth lies and to recognize the pitfalls of error. This then is the end.

This short analysis clearly shows that Thâbit's remarkable text is in no way a development of the *Treatise on the Balance* which Dr. Woepcke translated. On the other hand, this text is closely related to the four propositions which we discovered and summarized. The resemblance is such that a scribe might well have mistaken these four propositions for a summary of Thâbit's book.

One difference, however, deserves to be pointed out. Thâbit deduces the demonstration of the law of equilibrium of the lever from the fundamental axiom of Peripatetic dynamics. He does so by strictly following the method given in the *Mechanical Problems* of Aristotle. The method outlined by the author of proposition A is quite different. It assumes that the law of equilibrium of the lever has been directly established and deduces its application to the lever from the law of dynamics stated in the *Physics* as well as in the *On the Heavens* of Aristotle, and in the *Liber de ponderoso et levi* attributed to Euclid. While Thâbit's text is intricately connected with the *De ponderoso et levi*, an intermediary text must be assumed to exist between this book and our four propositions. As we have indicated earlier, this intermediary fragment could very well have been the source of the work published by Dr. Woepcke.

However, despite the fact that our four propositions have a very close relationship with the Greek work which Thâbit set out to reconstitute, they do not seem to be that same text. One other consideration supports this view: Thâbit not only mentions the obscurity of the text upon which he is commenting, but also its prolixity, which he wants to abridge. This remark cannot apply to our four propositions, whose demonstrations are reduced to a few extremely concise formulations.

Our four propositions do not seem to be a fragment from the *Causae Karastonis*, but rather a summary of them. Even more likely, they might depict an older topic on which Charistion might have written a more developed but slightly modified commentary.

These remarks suggest a hypothesis. Ptolemy had written a treatise *On Weights*, which is unknown to us. Thurot had already expressed the supposition[51] that certain fragments which have come down to us —

especially the *De ponderoso et levi* attributed to Euclid, could very well be the remnants of the *On Weights*. Could not this supposition be equally true for our four propositions[52] as well as for the work which Dr. Woepcke published in a distorted form as the *Treatise on the Balance*? Following this supposition, Charistion would have simply developed and adapted more closely to the Peripatetic method the *On Weights* composed by his father.

## 3. THE TREATISE *DE CANONIO*

Only one *Liber Karastonis*, that of Thâbit ibn Qurra, was translated into Latin. However, it is not the only *Kitâb el Karstûn* written by Arab geometers. In the article which we have quoted several times, Steinschneider lists four such treatises which he found in the indexes and catalogues of various libraries. The treatises are the following:

(1) A *Kitâb el Karstûn* by the "Three Brothers," the Banu Musa.
(2) A *Kitâb el Karstûn* by Thâbit ibn Qurra.
(3) A *Kitâb el Karstûn* by a famous philosopher and physician, Arab by birth, but of Christian faith, Kûstâ ibn Lûkâ, who lived from 864 to 923 and was, therefore, a contemporary of Thâbit.
(4) A *Kitâb el Karstûn* by Abu'Ali al Hasan ibn al Hasan ibn Alhaitam, who died in 1038 after gaining fame under the name of Alhazen through his *Optics*, which was translated into Latin.

If one agrees with Curtze that the *Kitâb el Karstûn* attributed to the Banu Musa is identical to the *Treatise on the Balance* translated by Dr. Woepcke, one is still left with two unknown treatises with the same title but which have not come down to us.

It is tempting to assume that one of these treatises is the book *De canonio*. The Bibliothèque Nationale possesses one copy of this text under manuscript 8680A (Latin collection) and a second, modified copy under manuscript 7378A. And finally, there is the important fragment which was attached in such a peculiar way to the text of Jordanus in the 13th century manuscript kept in the Bibliothèque Mazarine under the number 3642 and which we pointed out in section 1. This last fragment is reproduced in the collection[53] which Master François Guillebon gave to the Sorbonne.

However, a more thorough examination of this treatise leads us to believe that we have here not only a treatise of Greek origin, but a treatise which might have been directly translated from Greek into Latin without having gone through an intermediate Arab translation.

The letters used in the figures are in the following order:

a, b, g, d, e, z, i, t

and indicate by their sequence that they were originally Greek letters. However, the Greek letter *eta* is depicted not by the letter *h* as would be the case in those works having passed from Greek into Latin through the intermediary of Arabic, but by the letter *i*. This detail seems to indicate that the translator knew the pronunciation of the *eta* already in use by the Greeks of the Middle Ages.

Furthermore, there is in this short work an abundance of Greek words which are not translated but merely transcribed. To designate a beam of a balance of considerable thickness, the translator rendering the four propositions analyzed in section 1 from Arabic into Latin, uses the term "longitudo teres." The translator of Thâbit's text uses "perpendicularis cum crassitie" (or "crossitie" or "grossitie"),[54] Jordanus who was writing directly in Latin says "oblongum" or "regula." Our treatise has retained the Greek word *κανών* by simply latinizing it as "canonium." In all of the other works we quoted from, a line parallel to the horizon is called "parallela orizonti." Here it is called "parallela epipedo orizontis," a wording clearly derived from the Greek name for a plane *to epipedon*. Not only do we find the word "parallelogrammum" in the book *De canonio*, but a triangle is called "trigonium" instead of triangulus.[55] Finally, "demonstratio" is sometimes replaced by "apodixis".

It is clear that the work we are about to discuss is a direct translation from a Greek text. Its contents, when compared to what Thâbit ibn Qurra informed us about the work of Charistion, shows us that the treatise *De canonio* is either a replica of this work or, better yet, a work meant to complete it by furnishing an elegant geometrical solution to the calculation done by Charistion.

The goal of the short treatise *De canonio* is to provide the solution to the problem to which one is led by the *Causae Karastonis*, as preserved for us by Thâbit. What weight must be suspended at the extremity of the short arm of a beam of a Roman balance in order to compensate for the excess weight of the long arm so as to be able to theorize about this instrument as if the beam were a line without weight?

The author does not establish a second time the law of the lever. Furthermore, he does not attempt to demonstrate that a portion of a heavy cylinder parallel to the axis of the beam has the same effect as a body of the same weight suspended from the point marked by the center of the cylinder because he considers these propositions as already established. With respect to the second proposition. he refers the reader to his predecessor's works:[56]

Monstratum est in libris qui de his loquuntur quonian nulla est differentia, sive pondus *db* sit equaliter extensum super totam lineam *db*, sive suspendatur a puncto mediae sectionis.

With respect to the first proposition, the text of Ms. 8680A states:[57]

Sicut demonstratum est ab Euclide, et Archimede, et alliis et haec est radix circa quam versantur omnes.

As we shall see in Chapter VII, Section 1, this statement has disappeared in manuscript 7378A.

Thus, when all these preliminary considerations are disregarded, the treatise *De canonio* can be reduced to four theorems. The first of these theorems is identical to the one which ends Thâbit's book. Its object is to state the rule by which one can calculate the weight required to compensate for the excess weight of the long arm of a Roman balance. When these two statements are compared to each other, it is clear that they represent two translations of the same original Greek text. The numerical example to which the formula is applied is also the same in the two works. We have here, most certainly, one of the propositions of Charistion's book.

The second theorem is converse to the first one and its statement and demonstration were truly superfluous.

In the third theorem, the author sets out to find a cylinder of the same diameter and of the same material as the beam which would weight exactly as much as the compensatory weight. Here is the elegant construction by which he determines the length of this cylinder:

Let *ab* (Fig. 17) be the beam; let *g* be the point of suspension; let *ga* be the short arm and *gb* the long arm. Starting from point *g*, let us take on the long arm *gb* a length *gd* equal to the short arm *ga*. Through point *d*, let us draw at *ab* a perpendicular *de* which

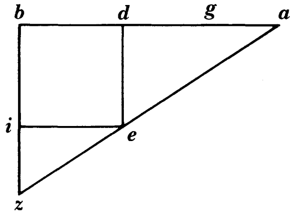


*fig. 17.*

is equal to *bd*; let us join *ae* and extend this line until it meets at *z* the perpendicular drawn to *ab* through the extremity *b* of the long arm; *bz* will be the length which we are trying to determine.

The demonstration of this theorem is easily deduced from the formula which can be translated in modern terms as the first theorem of the *De canonio*.

From this construction, the author deduces the solution to the following problem: to determine the point of suspension if the length of the beam is known as well as the weight necessary to compensate for the excess weight of the long arm over the short arm. The answer to this question makes up the fourth and last theorem of this short work. It is an elegant example of the Alexandrian approach to mechanics.

CHAPTER VI

# STATICS DURING THE MIDDLE AGES JORDANUS DE NEMORE

The fragment *De ponderoso et levi* attributed to Euclid; the four propositions called *Liber Euclidis de ponderibus secundum terminorum circumferentiam*; the treatise *De canonio*; the *Liber Karastonis* published by Thâbit ibn Qurra: all of the above works as well as the *Mechanical Problems* of Aristotle seem to be the sole remnants of the Greek works on statics used by medieval mechanicians. They do not seem to have known of Archimedes' method because they never use it in their works. As for the Arabs, they seem to have merely transmitted to the Western world the remnants of Alexandrian science.

We shall now see how the Western mind seized these remnants and incorporated them into the mechanical systems which it will construct. We will observe a prodigiously intense and powerful work of transformation and orgainzation which will produce modern statics. However, we are rarely able to determine the name of any author of these ingenious efforts by which the Middle Ages will create several concepts of a fecundity unexhausted to this day. Those who created these concepts will remain forever anonymous. Their discoveries, however, were to enrich the work of the one who was undoubtedly the master of them all. Although his name is known to us, we have no other reliable information with which to sketch the character of the man called Jordanus de Nemore.

## 1. WHAT DO WE KNOW ABOUT JORDANUS DE NEMORE?

Montucla tells us:[1] "Jordanus Nemorarius, who lived around the year 1230, was a very intelligent man in matters of geometry and arithmetic." Chasles also writes,[2] "Jordan was a very erudite geometer, who wrote about all branches of mathematics, including statics. Only much later did he attact followers in this latter field. During the Renaissance he was very well-known to the Italian geometers and Luca de Burgo quotes him often."

From the 13th century on, the *Arithmetic* by Jordanus or Jordanis seems to have been considered a classical work, to judge by the great number of manuscripts of the *Arismetica* or the *Elementa Arismetice* which are to be found in various libraries. Thus Lefèvre d'Etaples (Faber Stapulensis) saw to it that this work was printed almost immediately. Without altering the text of Jordanus, he added new theorems with his own demonstrations. The Gothic in folio edition[3] appeared in Paris in 1496. A second edition, also due to Lefèvre d'Etaples, appeared in 1514.

Jordanus composed a treatise entitled *De numeris datis* or *De lineis datis*, which Regiomontanus[4] mentions in the highest terms: "Tres libros de datis numerorum pulcherrimos edidit Jordanus."[5] Maurolycus did not attach any less importance to this work, since he placed it on the list[6] of treatises he intended to print. Chasles[7] pointed out its great significance for the history of algebraic calculations. However, it was only recently published by Treuttlein[8] and then by Curtze.[9]

In 1534, Johannes Schöner published with Petreius de Nüremberg a treatise entitled *Algorithmus demonstratus.* The manuscript had been found among the papers of Regiomontanus. And it is probably justifiably attributed to Jordanus.[10]

Besides the algebraist in Jordanus there was also the geometer whose remarkable talents of invention are evident in the treatise *De triangulis*, published by Curtze.[11]

Further important geometrical demonstrations can be found in a work on cosmography published in Basel in 1507, 1536 and 1558.[12] But further investigation will be necessary before we can attribute this work to Jordanus with certainty. We found the text of a work on the astrolabe in the collection of mathematical and astronomical works kept in the Bibliothèque Mazarine under the number 3642 (formerly 1258). It is bound together with a work which, under the title *Compotus manualis,* shows how the human hand can serve as a perpetual calendar. In the table of contents which precedes the collection, one reads: *Compotus Manualis. Liber Jordani de Astrolabio. Liber compoti manualis. Liber tractatus Jordani de Astrolabio.* However, in the text itself, this work is not attributed to Jordanus, but to someone called Hermann: *Tractatus Hermanni de Astrolabio.*

Heilbronner[13] mentions the existence of a *Tractatus Jordani de speculis* in a manuscript in the Bodleian Library at Oxford. The authenticity of this work, known only by its title, has been questioned by Cantor.[14] We were quite fortunate to have found a copy of this work in a very precious manuscript collection stemming from the hand of Arnold of Brussels and kept in the Bibliothèque Nationale under the number 10252 (Latin collection). On the recto side of sheet 136, one reads: *Incipit tractatus Jordani de speculis cum comento super eodem*;[15] and on the verso side of sheet 140: *Explicit liber de speculis — Incipiunt elementa Jordani de ponderibus*.[16] This treatise on optics is written in the clear and sober fashion characteristic of Jordanus.

If to this treatise *De speculis* one adds the work on statics entitled *De ponderibus*, which we shall analyze in detail in section 3, one will obtain an impression of the intellectual power of the author generally known by the name of Jordanus Nemorarius. However, we should temper our admiration for such fecundity with some reservations. We shall see that from the 13th century on, Jordanus was thought to be the sole author of three separate treatises on statics which came out of the same school, but in fact, were quite different from each other and bear the marks of at least three separate authors. It should not surprise us that such a misunderstanding could have reoccurred under different circumstances and that the collection of mathematical works we listed here was the work of a coterie of geometers whose names were all forgotten but one. If this is true, the reputation they themselves would justly have merited probably served to increase that of Jordanus.

Do we have any personal information about this author? We know nothing, neither the country he was born in, nor the time in which he lived. All that we can say about this great geometer amounts to nothing more than vague and contradictory conjecture.

Let us first mention the view of the *Biographie Universelle* of Michaud, where the author of *De ponderibus* is identified as Raimond Jordan, provost of the Church at Uzès in 1381 and author of works deposited in the Bibliothèque des Pères under the strange pesudonym *Idiota*.[17] This opinion is untenable, since we have many manuscripts of the *De ponderibus* and of other works by Jordanus which date back to the 13th century.

On the other hand Daunou[18] tells us that certain historians had Jordanus living in Germany around the year 1050 during the reign of Emperor Henry III. Giuseppe Biancani, a Jesuit, who under the name of Blancanus published a *Clarorum mathematicorum chronologia*[19] in 1615, places him in the 12th century. However, these statements by Blancanus should be viewed with caution.[20]

Daunou has attempted to establish the period in which Jordanus lived with the following reasoning:

He is supposed to have quoted Campanus of Novara and is supposed to have been quoted by him as well . . . Campanus is sometimes placed in the 11th century, but most likely belongs to the 12th century.

From this, the author of the *Histoire littéraire de la France* concludes that Jourdain le Forestier could have begun his work shortly before 1185 and ended his career in 1235. However, this conclusion is wrong because its premisses are based on a false estimate of when Johannes Campanus of Novara lived. Roger Bacon mentions Campanus in Chapter XI of his *Opus tertium*[21] and calls him one of the finest mathematicians of his time. Campanus was chaplain to Urban IV, who was Pope from 1261 to 1281. Thus, if Jordanus had been his contemporary, he would have written much later than Daunou supposes.

Chasles,[22] because he assumed that Jordanus Nemorarius had composed his works during the 12th century, was sharply attacked by Libri[23] who insisted that Jordanus Nemorarius be placed in the 13th century. The great geometer, being thus challenged, attempted later to prove[24] that Jordanus had lived at the end of the 12th or the beginning of the 13th century. He did not hesitate to declare as erroneous the quote from Campanus by Jordanus which Daunou had used in his arguments. Chasles adds:

The careful study of some of his works, in particular, his *Algorisme* has persuaded me that they predate the works of Fibonacci, Alexander of Villedieu, Sacrobosco, Campanus, etc.

Chasles had very good reasons to call into question the quote from Campanus which Jordanus was supposed to have used. It is absolutely true that the *Liber Jordani Nemorarii viri clarissimi de ponderibus*[25] published in Nüremberg in 1533 by Peter Apian,[26] refers the reader to the additions made by Campanus to the *Elements* of Euclid. However, as we shall see in Chapter VII, section 1, the treatise published by Apian is an extensive revision of a manuscript widely circulated in the 15th century and known as either the *Liber Euclidis* or the *Liber Jordani de ponderibus*. This manuscript was itself the result of attaching the treatise *De canonio* to the original text of Jordanus, and then attaching a more prolix version of the latter. The quote from Campanus is not to be found in the *Liber Euclidis de ponderibus* and, more importantly, is also missing in the original text of Jordanus.

More recently, a new hypothesis on Jordanus Nemorarius has been proposed by Boncompagni and by Treuttlein[27] and supported by Curtze in the introduction to his edition[28] of *Jordani Nemorarii de triangulis libri quatuor.*[29] According to this hypothesis, this geometer is no other than the Dominican, Jordan the Saxon.

One tradition ties Jordan the Saxon to the family of the Counts of Eberstein, another to the von Drach family. According to some sources, Jordan the Saxon was born in Borrentrick or Borrentreich next to Warburg in the bishopric of Paderborn, consequently in the forests of the Eggebirge. This would explain his surname Nemorarius.[30] According to other sources, he was born on the estate of Dassel, belonging to the diocese of Hidelsheim.[31]

In 1220 in Paris, Jordan the Saxon entered the order founded by St. Dominic. After Dominic's death in Bologna in 1221, the chapter met in Paris in 1222 and chose Jordan as the Master General of the Order. The two main pieces of evidence in support of the identity of Jordan the Saxon and Jordanus Nemorarius are as follows:

First of all, there is a passage which Boncompagni discovered in a chronicle composed in the 14th century by the English Dominican Nicolas Trivet. Trivet discusses the election of 1222 which made Jordanus Saxo the Master General of the Order of Preachers. He states that the newly elected general enjoyed a great reputation in the scientific world as a mathematician and it was believed that he had composed two extremely useful treatises: *De ponderi* and *De lineis datis.*

Secondly, there is the chronicle of this order composed in 1420 by the Dominican, Jacob von Soest. Jacob twice states that the Master General Jordanus had written, besides other works, a *Geometricalia delicata.*[32]

This evidence is explicit. Certain authors, however, question it since neither of these two witnesses is a contemporary of Jordanus Saxo and, at that time, similarities in names quickly gave rise to confusion. Furthermore, it is hard to explain why no ecclesiastical document mentions the name Nemorarius and why no mathematical manuscript is attributed to Jordanus de Saxonia. Consequently, the Reverend Father Denifle[33] denies that Jordanus Saxo and Jordanus Nemorarius are one and the same person. Cantor reserves judgement in this matter.[34]

To these attempts at removing the veil which so completely conceals

Jordanus Nemorarius, may we be allowed to add some observations which might help our successors lift a corner of this veil?

These observations concern, first of all, the name of our geometer. The custom of calling him Jordanus Nemorarius prevailed. However, this name is not in any of the numerous manuscripts which have come down to us. Most of the manuscripts read simply Jordanis, Jordanes or Jordanus. Sometimes these different spellings can even be found in one and the same manuscript.

When another name accompanies this first name, it is never "Nemorarius", but "de Nemore." Among all the manuscripts whose titles we were able to discover in the Parisian libraries, only those which contain the *Arithmetic*[35] by our geometer bear the name "de Nemore". Curtze,[36] however, points out a manuscript, the Ms. F. 33 in the Library of Basel, which contains under the title *Jordanus de Nemore et Euclides de ponderibus* the rhapsodic work generally known as *Liber Euclidis de ponderibus.*

In the 13th and 14th centuries, in composite names like Jordanus de Nemore, the second name, the one that follows the preposition "de," is usually a place name such as the place of birth or origin: Alexander of Villedieu, Campanus of Novara are called Alexander de Villa Dei, Campanus de Novarra. No one thinks of translating Johannes de Sacrobosco as John of the Sacred Woods, but by John of Holywood; Johannes de Muris is not called John of the Walls, but John of Murs. Thus, instead of translating Jordanis or Jordanus de Nemore by Jourdain or Jordan the Forester would it not be more natural to see in the name the latinisation of,[37] Giordano de Nemi?

It should not surprise us if Jordanus de Nemore later became Jordanus Nemorarius. Examples of analogous transformations are plentiful. Pierre de Maricourt (Petrus Peregrinus), whom Roger Bacon calls Petrus de Maharne-curia, became Petrus Maricurtensis. Johann Muller of Koenigsberg, who called himself Johannes de Monte-Regio, came to be known finally as Regiomontanus. Furthermore, the name Jordanus Nemorarius seems to have been first used by Lefèvre d'Etaples, who wrote his own name in Latin as Faber Stapulensis.

If one sees in the words "de Nemore" or "Nemorarius" a reference to the village of Nemi, as now seems plausible, our great geometer would be Italian and his identification with John the Saxon would no longer be tenable.

Let us add to these remarks on the name of Jordanus de Nemore an observation about the dates when he could have composed his works. We will show here and in the following chapter that the manuscripts of the 13th century attribute to three distinct works the same name of *Liber Jordani de Ponderibus*. The first one, apparently the original text, is the one analyzed in this chapter. The second one is a new version of the same text by a Peripatetic philosopher, who profoundly transformed some of its fundamental ideas. The third, more developed, is by a mechanician to whom we owe the notion of moment,[38] the theory of the inclined plane and several other essential discoveries. How else can we explain the fact that works so different and at times so contradictory are attributed to the same geometer unless we suppose that they are so ancient that the true names of their authors had already been forgotten? But how can we make such a supposition if the oldest work does not go back more than a century? Thus, if Jordanus is the author of the oldest of these treatises, one would have to conclude that he must have written no later than the 12th century.

## 2. SOME PASSAGES FROM ARISTOTLE'S *MÈCHANICAL PROBLEMS*

Although the statics of Jordanus appears as a truly original work and not simply a compilation of earlier works, it deduced its principles, nevertheless, from Greek science. On the one hand, there are ties between it and the *De ponderoso et levi* attributed to Euclid, as well as the four propositions which sometimes accompany it. On the other hand, it has ties with certain passages of the *Mechanical Problems*. Thus it is necessary to closely examine these passages and to clarify the ideas which the Stagirite presented in it.

Aristotle was much preoccupied with the composition of velocities. He states this law with great precision[39]

> If a body moves in two directions in such a way that the spaces traversed in the same time have an invariable ratio, the body moves in a straight line along the diagonal of a parallelogram which has as its sides two lines in the same ratio.

He gives the now classical demonstration of this fundamental law.

On the contrary, if the ratio of the two component spaces traversed in the same time by the body varies with time, the moving body will not follow a straight line.

So that a curved trajectory results when the body is moved in two directions in a ratio which varies with time.

Let us consider, specifically, a circle in a vertical plane and a body descending along the circumference of the upper half of the circle.

It is clear that this body is borne by two simultaneous movements. One of these movements makes it descend vertically, the other one displaces the vertical trajectory so that it moves away from the center.

These propositions, so precise and exact, belong to what we would call today kinematics. Aristotle deduces from them results which belong to dynamics and which concern the composition of forces. The transition is not explicit, but it can be easily supplied by recalling the fundamental principle of Peripatetic dynamics. The force moving a given weight is directed along a line described by this weight and is proportional to the space traversed in a given time.

Thus a body describing the upper half of a circumference in a vertical plane is acted upon by two forces. One pulls it vertically downward while the other one tends to move it in a horizontal direction away from the circle. In the same way, if a heavy body describes the lower half of this circumference — and Aristotle only considers this case from now on — it will be forced to descend vertically according to a natural movement because of its gravity, and it will be pulled horizontally, against nature, towards the inside of the circle.

Moreover, if two bodies describe, on vertical planes, unequal semicircles, they will not have moved horizontally the same distance after descending by the same amount from the horizontal diameter. Given the same natural movement, the body which describes the small circumference will have moved further against nature than the body describing the large circumference. Although the force of gravity will be the same for both of these weights, the force "which pulls towards the side and towards the inside" will be larger for the first body than for the second.

One can see that of these two descending bodies, the body on the larger circumference moves faster than the other one, or stated differently, is acted upon by a more powerful resultant force because the natural force of gravity is countered by a force against nature of a lesser intensity.

If on a radius descending about a center one chooses several points of unequal distance from the center, these diverse points will describe during the same time unequal natural movements and unequal unnatural movements. However, for each one of these points, the ratio between the natural movement to the unnatural movement remains the same. The contemplation of this equality concerned Aristotle for a long time and he seems to have seen in it a somewhat mysterious correlation with the law of the lever. It would be difficult for us to trace the rather confusing considerations to which the Stagirite was led by this contemplation. Even among those propositions which we have stated, there are some which are hard to reconcile with the principles of modern dynamics. However, as inaccurate as they may be, they still played an important role in the development of mechanics because they were the first to suggest the idea of a composition and decomposition of forces. What we have said here will suffice to show how the Peripatetic school understood this composition of forces and to explain certain conceptions of Jordanus.

We must now say a few words about another question which preoccupied Jordanus and his successors very much and which had already been examined by Aristotle.

Aristotle considers[40] a balance which has a beam BC (Fig. 18) in the form of a prismatic rectangular ruler (what he says about it is proof that he attributes this shape to it). He assumes that this beam is suspended from a rope DA attached at point A on its upper edge. He then asks how the beam, displaced from its horizontal position to position EF, will return to its original position, if let go. In other words, he asks himself why the equilibrium of such a balance is stable.

He answers in the following way: If the left side of the beam is lowered as in Figure 18, the segment of the ruler which is on the right side of the vertical line DAM is larger and, therefore, heavier than the segment on the left side of the same vertical line. The right segment descends while raising the left segment and the beam will thus return to its original position.[41]

He then takes a beam BC in the same form as the preceding one, but resting on a support D at point A on its lower edge (Fig. 19). After comparing this configuration with the preceding one, he states the following:

> The opposite occurs when the support is underneath.

He should have concluded from this that the beam displaced from its

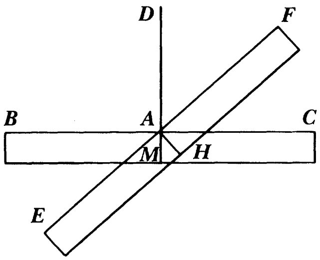


*fig. 18.*

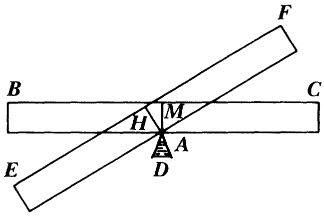


*fig. 19.*

horizontal position would not cease to move until it has become vertical, or in other words that the original equilibrium was unstable.[42] By a strange oversight he concludes that this beam would remain in stationary equilibrium in any position it might be placed. This easily refutable error persists in several authors up to the middle of the 16th century.

## 3. THE ELEMENTS OF JORDANUS ON THE DEMONSTRATION OF WEIGHTS

This title seems to have been the original title of the treatise on statics written by Jordanus. The original version of this treatise was not fully appreciated for a long time because variations and commentaries on it were composed over the centuries and other works were joined to it sometimes with and sometimes without a natural justification. All of these effusions were modified and developed and then reproduced by the printing press, which gave rise to books with very little resemblance to the work whose name they carried.

A manuscript[43] in the Bibliothèque Nationale preserves a text of the original work by Jordanus, which seems complete and almost free of any alteration. The date of this text, composed in an elegant and regular handwriting of the 15th century, is known exactly because it ends with the words "8 kal. novembris 1464." It is not signed but some comparisions easily allow us to determine the scribe's name. In fact, the same volume contains various other texts all in the same hand. Two of these texts are not only dated, but also signed. The first, *Algorismus de integris per Joannem de Sacro Boscho* ends with the words: Finis.

Neapoli, per Arnaldum de Bruxella, 1476, die 11 februarii, ante ortum solis.[44] The second one, *Tractatus de ponderibus secundum Magistrum Blasium de Parma*, whose importance we shall consider in the following chapter, ends with the words: 1476, 5 Januarii, Neapoli, per A. de Bruxella.

In another similar collection[45] we find more detailed information on this Arnold of Brussels. The astronomical tables, works by a Blanchinus or de Blanchinis, end with this formula:

Finis; 8 kal. Aprilis 1468 imcompleto, Expliciunt canones super tabulis clarissimi mathematici et artium doctoris Johannis de Blanchinis in armis milities strenuissimi factoris generalis Ill. Borsii, ducis Mutine et Regii, comitis Rodrigii, marchionis Estensis et Ferrarie, completi per Arnaldum de Tiishout de oppido Bruxella, ducatus Brabancie. Anno 1468, incompleto 8 kal. Aprilis 2e indictionis. In urbe Parthenopes.[46]

Arnold of Tijshout adds this piece of astronomical information as a remembrance of his fatherland[47]

Bruxelle polus elevatus g. 40.

From one of his quotes we know that Arnold of Tijshout from the city of Brussels and the Duchy of Brabant sometimes stayed up almost till sunrise in order to finish copying a valuable manuscript in regular, well-aligned, Gothic letters. However, he did not limit himself to the profession of a scribe. At Naples, where he became established, this Fleming, or "il Fiamengo" as he was called there, displayed characteristic Flemish initiative. He became a printer and many a famous work came from his press.[48]

Thus we owe to Arnold of Brussels the collection in which we find after Jordanus' treatise *De speculis*, an almost flawless text of the *Elementa de ponderibus*.

The Bibliothèque Nationale has another complete text[49] of the same work which differs only slightly from the first one and which the scribe incorrectly titled the *Liber de ponderoso et levi*, which is the name generally given to the fragment attributed to Euclid. Furthermore, the scribe appended to the work of Jordanus three of the four propositions which we studied in section 1 of the preceding chapter.

The Bibliothèque Mazarine has in its possession a text from the 13th century[50] entitled *Elementa Jordani super demonstrationem ponderis*. Unfortunately, this text is not complete. We noted in the first section of the preceding chapter in what a strange way the beginning of a proposition of Jordanus was continued by the inclusion of a theorem from the *De canonio*. We also noted how this peculiar union had been scrupulously reproduced in the manuscript collection[51] belonging to Master François Guillebon, Doctor at the Sorbonne. However truncated the text of the Bibliothèque Mazarine and its reproduction may be, they allow us, nonetheless, to verify a portion of the treatise copied by Arnold of Brussels. They show us that this portion had not undergone any noticeable alteration between the 13th and 15th centuries.

The clarity and the conciseness of Jordanus' statements and demonstrations confer to this treatise a very elegant form which his commentators subsequently altered. The treatise is, however, very short. It opens with seven axioms or definitions and develops from them nine propositions.

Moreover, we do not seem to possess the complete text. In the demonstration of the third proposition, Jordanus writes the following: "Sicut constituimus Praeexercitaminibus."[52] These Praeexercitamina constitute, undoubtedly, a kind of preamble where certain preliminary geometrical lemmas are demonstrated. In two other passages where Jordanus demonstrates the second and fifth propositions, he indicates another reference: "Sicut declaratum est in Filotegni-sicut declaravimus in Filotegni."[53] These two references also relate to geometrical propositions. Thus Jordanus appears to have written, besides the numerous works known to us, a treatise on geometry which has been lost. He seems to have given this treatise a Greek title: *Filotegnis*, "the friend of the art," a rather peculiar thing to do at the time in which he was living.[54]

However, nothing leads us to suppose that the *Elementa super demonstrationem ponderis* is, like the *De canonio* for example, a simple translation of a Greek work, because it contains no other Greek expression besides the title we just noted. If we follow the order of the letters which designate the different points of the figures or the diverse magnitudes where the author is computing, we no longer see the sequence of the Greek alphabet. In the first demonstration, the letters are introduced according to the Latin alphabet: *a, b, c, d, e, f*. In other places, we see two similar lines marked *dy* and *ez* and even *dh* and *eg*. Everything seems to indicate that we are here in the presence of a seminal work, born of Western genius.

This does not mean that the author of the work was not familiar with some of the original Greek texts which we have previously analyzed. As soon as one opens the *Elementa*, it is immediately obvious that the author knows the *De ponderoso et levi* attributed to Euclid. His first two axioms and his first proposition form a sort of summary of this fragment.

On the other hand, the ninth and last proposition of the *Elementa* sets out to prove that a cylindrical mass formed like the arm of a beam of a balance, weighs exactly the same as if its weight were concentrated at its center. In order to prove this, Jordanus is content to remark that two equal weights suspended at two different points of the beam weigh as much as a single weight equal to the sum total of the two weights and suspended midway between these two weights. It is impossible to read the last proposition without thinking that the author must have had before him the four propositions which comprise the *Liber Euclidis de ponderibus secundum terminorum circumferentiam*, or, at least, the two propositions B and C.

The main interest of the proposition which ends the *Elementa* of Jordanus, lies in the fact that it allows one to calculate the compensatory weight which must be loaded on the small arm of the Roman balance. Since this book does not mention this application, it has an unfinished look about it. From the 13th century on, and the strange union in the text of the Bibliothèque Mazarine testifies to it, one was in the habit of placing the *De canonio* after the treatise of Jordanus. This association, which we shall discuss again in Section 1 of the following chapter, was very natural, because the *Elementa super demonstrationem ponderis* ended with the very theorem postulated in the following terms by the *De canonio*:[55]

Monstratum est in libris qui de his loquuntur.

This association appears so natural that one wonders if it might not have been intended by Jordanus and if in his *Elementa* he did not intend to write a kind of introduction to the *De canonio*, which he may have translated.[56]

The resolution of weight into different directions plays an essential role in this work of Jordanus. This constitutes the main interest in the introduction and distinguishes it from all the works we previously studied, with the exception of the *Mechanical Problems*, with which it is quite similar in the above respects.

Jordanus considers a body compelled to descend along a non-vertical trajectory. In his computation, he introduces the component of weight along the trajectory as representing the sole motor force. He calls this component the weight relative to the position of the moving body, "gravitas secundum situm."[57] The quantitative relationship between this relative weight to the weight per se is not known to Jordanus. He merely formulates a qualitative rule: the more oblique the trajectory, the weaker the "gravitas secundum situm." Furthermore, in order to compare the obliquity of diverse trajectories it is necessary to take trajectories of the same length and evaluate the vertical descent to which they correspond. The one corresponding to the smallest vertical descent is the most oblique.

Those are the principles which Jordanus states at the beginning of his work in the following words:

Omnis ponderosi motum esse ad medium, virtutemque ipsius potentiam ad inferiora tendendi et motui contrario resistendi . . .
Gravius esse in descendendo quando ejusdem motus ad medium rectior.
Secundum situm gravius, quando in eodem situ minus obliquus est descensus.
Obliquiorem autem descensum in eadem quantitate minus capere de directo.[58]

These principles would have been easy to apply to straight trajectories, and it seems that the problem of the inclined plane ought to have occupied Jordanus first. However, his attention seems not to have been focused on this particular problem, but only on the problems posed by curvilinear motion in the study of the lever and the balance. But these latter problems are harder to treat with the concept of "gravity relative to the position" of the body. In order to calculate the obliquity of the trajectory, it would have been necessary to compare the length of an infinitely small path traversed along this trajectory with the infinitely small descent corresponding to this path. Such infinitesimal comparisons were impossible to carry out in the 12th and 13th centuries.

It seems nevertheless, that these comparisons occurred to Jordanus during the demonstration of one of those important propositions whih were to significantly influence the subsequent development of statics.

Jordanus considers a material point fixed at the extremity of an arm of a lever moving about point *b* (Fig. 20). This arm of the lever is first horizontal and the weight is at *a*. Then one inclines it in such a way that the weight is either at *d*, above point *a*, or at *e*, below point *a*. In both cases, the positional weight decreases.

In fact, says Jordanus, let us mark beneath points *a, d* and *e* the arcs *az, dh* and *eg* "as small as we wish", or as the Latin says, "quantulumcunque parvi," but equal to each other and let us note by *bf, kn* and *tx* by how much the trajectories of these arcs project on the vertical.

Since *kn* and *tx* are obviously smaller than *bf*, the descent of *dh* and *eg* is more oblique than the descent of *az*. Thus the positional weight is weaker at *d* and at *e* than at *a*.

If the infinitesimal method appeared for an instant to a medieval geometer, it must have been like a flash instantly disappearing again. Generally, Jordanus considers finite arcs and compares them to their vertical projection, just as Aristotle had done in the *Mechanical Problems*. This sometimes causes paralogisms. Here is such a reasoning which gave rise to much debate during the 16th century:

A lever *bac* (Fig. 21), carries equal weights at the extremities *b* and *c* of its equal arms. This lever is not at all horizontal, with arm *ac* higher and arm *ab* lower. Jordanus sets out to prove that weight *c* is positionally heavier than weight *b*, so that the first will cause the second to rise, bringing the lever to a horizontal position which will then be a position of stable equilibrium.

To construct this proof, Jordanus takes beneath points *b* and *c* equal arcs *bg* and *cd* which he projects on the vertical *kl* and *zm*. Since *kl* is smaller than *zm*, the arc *bg* projects less on the vertical than arc *cd*, which establishes the stated proposition.

Aristotle had understood very well that the stability of a rectilinear beam was due to the fact that the point of suspension was above the center of the lever. This correct concept is not clear to Jordanus or his commentators.

Other contradictions can be found in Jordanus. In his time, it must have been quite natural to allow the majority of these errors. It seems

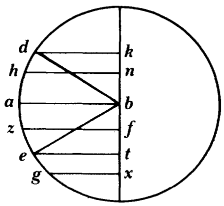


*fig. 20.*

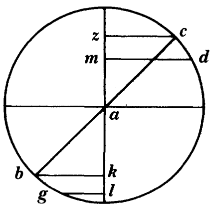


*fig. 21.*

natural, for example, to think that in a mechanism a weight would lift another if the positional gravity of the first exceeded the positional gravity of the second. From this admittedly plausible but nonetheless inaccurate principle, Jordanus will deduce formally an erroneous proposition. This proposition clearly shows us how far removed its author was from understanding the concept of moment.

Jordanus considers a bent lever (Fig. 22) with its short arm *ca* horizontal while the long arm *cf* is inclined. He assumes that the distance *fe* between point *f* and the vertical is precisely equal to the horizontal arm *ca*. We know today that equal weights placed at *a* and *f* produce equilibrium. Our author sets out, on the contrary, to prove that the weight put at *a* will cause the lever to tip in its favor.

To this end, he remarks that any descent of the weight *a* takes place along the quadrant *ak* with *c* as the pivot point and *ca* as the radius, while any descent of the weight *f* takes place along the arc *fz* with *c* as the pivot point of *cf* as radius. Let us measure along these two trajectories and below weights *a* and *f* equal arcs *al* and *fm*, respectively. The length *cn* which the first arc projects on the vertical is superior to the length *ed* projected on the vertical by the second. Weight *a* is thus positionally heavier than weight *f*. From this Jordanus believes that he is justified in concluding that the proposition formulated is proven.

Among Jordanus' demonstrations, one deserves special attention because it does not require the concept of positional gravity, and the principle which it depends upon is not explicitly stated. But on the other hand, this principle shows through so clearly that it is impossible not to recognize it and formulate it in the following way: Whatever can lift a given weight to a given height can also lift a weight *k* times heavier to a height *k* times smaller. This is the principle which Descartes will take as the foundation of all statics and which, due to Jean Bernoulli, will become the Principle of Virtual Displacements. There is more: we shall see that the current of ideas which carried this principle to Descartes and which had its source in the *Elementa* of Jordanus, did not suffer any discontinuity in its development. Descartes did, indeed, borrow this postulate from the commentators on Jordanus.

Jordanus implicitly appeals to this principle in order to justify the law of equilibrium of the lever.

Let us assume, says Jordanus, the beam *abc* (Fig. 23) with weights *a* and *b* placed at its extremities. And let us assume, furthermore, that the ratio between *b* and *a* is equal to

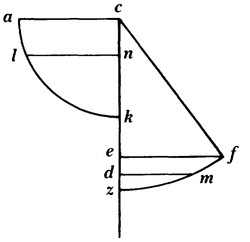


*fig. 22.*

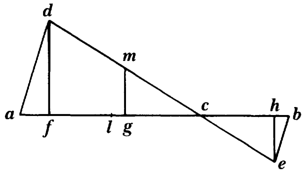


*fig. 23.*

that between *ca* and *cb*. I maintain that the beam will not move. Let us assume that it will incline on the side *b* and take the oblique position *dce*. Then *b* will descend by the vertical distance *he* and *a* will ascend by the vertical distance *fd*.

If a weight equal to weight *b* were to be placed at *l*, at a distance from point *c* equal to *cb*, that weight would ascend at the same time by a vertical distance *gm*, equal to *he*.

The triangles *ceb* and *cda* are obviously similar. The ratio between *df* and *eh* is thus the same as *ac* to *cb*, and consequently, as weight *b* to weight *a*. Thus *df* is to *gm* as weight *b* is to weight *a*, or as weight *l* is to weight *a*. Therefore, what is needed to move weight *a* to point *d* would be sufficient to move weight *l* to point *m*. But we have shown that weights *b* and *l* counterbalance exactly.

Therefore, neither the assumed displacement nor a displacement in the opposite direction will take place.

This demonstration of the law of equilibrium of the lever was a great advance over that given by Aristotle and followed later by Thâbit ibn Qurra. The latter took as its foundation the axiom of Peripatetic dynamics, the proportionality between force and velocity. The revolution which took place in dynamics in the 16th century was to render it finally void. The former demonstration, on the contrary, brought together the equilibrium of the lever and the equality between the virtual work of the driving forces and the virtual work of the resisting forces. It was the germinal formulation of a principle which was not to see its final and full development until the end of the 18th century in the *Mécanique analytique* of Lagrange. One of the primary goals of this investigation of the *Origins of Statics* will be to study the evolution of that apparently infinitesimal germ up to the finished form by which we know it today.

CHAPTER VII

# THE STATICS OF THE MIDDLE AGES (CONTINUED) THE SCHOOL OF JORDANUS

## 1. THE GENESIS OF THE *LIBER EUCLIDIS DE PONDERIBUS*

The ideas propounded in the *Elementa Jordani de ponderibus* provoked very intense intellectual excitement during the Middle Ages. Philosophers, geometers and mechanicians vied with each other in discussing, commenting upon and developing these ideas. From the 13th century, the *Elementa de ponderibus* inspired treatises quite different from their source.

This intellectual movement produced works which generally have neither the simplicity nor the rigor of the Alexandrian texts known to the medieval geometers. Whereas the former works dealt almost exclusively with the problem of the Roman balance, the medieval treatises pose infinitely more varied questions. Quite often, they succeed in resolving them by means of profound intuitions which disclose to their authors insight into some of the essential principles of statics.

Among the different currents which come out of the ideas of Jordanus, we shall study, first of all, the one current which led geometers to connect the *Elementa Jordani* to the *De canonio* and thus to compose the treatise usually called the *Liber Euclidis de ponderibus.*

The connection between the *Elementa* of Jordanus and another work was quite natural since the treatise of Jordanus seemed to have no definite conclusion but seemed to be a sort of introduction to another treatise, and furthermore it ended with a kind of lemma calling for additional theorems. The commentators were thus searching among the other works on statics for the completion which the *Elementa de ponderibus* seemed to require.

At times, the fragment which was joined in this way with the *Elementa* was made up of propositions contained in the *Liber Euclidis de ponderibus secundum terminorum circumferentiam.* It is this configuration, which a 15th century manuscript kept at the Bibliothèque Nationale[1] presents to us under the following title: *Incipit liber de ponderoso et levi.* This configuration was quite strange, because the proposition from the fragment joined to the treatise of Jordanus, proposition C, did double duty with the last proposition of the treatise. In this way, the law of the lever was established twice and by two incongruous methods. The combinations of these two texts is too disparate to conform to the intentions of Jordanus.

This combination of texts does not occur frequently since it only appears in one manuscript. Generally, one gave the *Elementa Jordani de ponderibus* the ending which it seemed to need by appending the treatise *De canonio*. This seems to have been standard practice from the 13th century on. How else can we explain the strange mistake which joins the ending of the *De canonio* to the beginning of the *Elementa* in the "Codex Mazarineus?" In the Ms. 7378 A (Latin) in the Bibliothèque Nationale, the *De canonio* does not follow the original treatise by Jordanus, but rather two other treatises deriving from it. Nonetheless, when the *De canonio* has to invoke the law of equilibrium of the lever, the text reads: "ut patuit in penultima superiorum demonstrationum."[2] This reference makes no sense, considering the place of the *De canonio* in our manuscript. However, it becomes perfectly clear if we assume that the *De canonio* follows immediately upon the text of Jordanus. It probably was placed there by the author who joined these two treatises where the following reference occurred: "sicut demonstratum est ab Euclide, et Archimede, et aliis,"[3] a reference contained in other texts of the *De canonio*. There is more: these two treatises follow each other so naturally and the book of Jordanus ends so accurately with the proposition invoked by the author of the *De canonio*[4] to demonstrate his first theorem that Jordanus himself seem to have intended this association. As we have said before, this geometer seems to have the intention of writing an introduction to the *De canonio*.

This logical connection between the *Elementa* of Jordanus and the *De canonio* cannot conceal, however, the diversity of their origins or cause us to mistake them for the two parts of a single work. The Greek text is clearly transparent through the Latin of the *De canonio*, while nothing in the *Elementa* indicates a Hellenic origin. Moreover, when the author of the *De canonio* invokes this proposition:

> A heavy cylinder, serving as the beam of a balance, weighs as much as an equal weight suspended from the center of the cylinder

he does not refer the reader to the last proposition of Jordanus, but to "books dealing with these matters."

The association of the *Elementa Jordani super demonstationem* *ponderis* with the *De canonio* soon became accepted as classical. It was in the 15th century, perhaps in the 14th, that a geometer whose name is unknown to us reshaped this rhapsodic work. He gives a verbose expansion to the clear and concise demonstrations of Jordanus and he repeatedly provides us with arguments about which Jordanus merely refers to the reader to the *Praeexercitamina* or the *Philotechnes*[5] and he supports them with lemmas borrowed from the *Elements* of Euclid or from the *Almagest* of Ptolemy, works which Jordanus had never invoked. On the other hand, he reproduced the propositions of the *De canonio* in the form they had been given in the 13th century, without eliminating the incongruity between the two parts of the text,[6] and without replacing the words "monstratum est in libris, qui de his loquuntur"[7] with a reference to his own ninth proposition.

This gradual association of the *Elementa Jordani* with the *De canonio* can be found in two manuscripts[8] kept in the Bibliothèque Nationale and bearing the title *Liber Euclidis de ponderibus*, a title that seems to have been given frequently.

The close analogy between the *Liber de ponderoso et levi* attributed to Euclid and the first proposition of *De ponderibus* of Jordanus could have led, quite naturally, to the attribution of the latter treatise to the Greek geometer. A 14th century manuscript[9] presents the *Elements of Geometry* followed by a long series of propositions formulated without demonstrations. In it we can find in sequence the different theorems of the treatise *De speculis* attributed to Euclid, propositions from the *Elementa Jordani de ponderibus*, from the *De canonio*, from the *Liber de ponderoso et levi*, and finally, from a treatise on perspective, which is also attributed to the author of the *Elements of Geometry*. Thus, from the 14th century on, the fame of Euclid occasionally increases at the expense of Jordanus.

Sometimes, however, the name of Jordanus reappeared next to that of Euclid in the titles of treatises which join the *Elementa de ponderibus* and the *De canonio*. Curtze[10] found in manuscript F.33 of the Library of Basel, a text entitled: *Jordanus de Nemore et Euclidis de ponderibus*. The extremely brief description which he gives of the text, nevertheless, allows us to recognize that such an association exists here too. Valentin Rose[11] found in the Codex Amplonianus, kept in Erfurt, next to *Liber Jordani de ponderibus* a work entitled *Liber ponderum Jordani, secundum quosdam vero Euclidis*. It ends with these words: "Explicit liber Euclidis de ponderibus secundum quosdam."[12] Curtze,[13] who saw this work, gives us rather superficial information about it, but does seem to indicate that it is the same association which we discussed earlier. The same author gives us[14] a more complete analysis of another treatise attributed to Jordanus, which is contained in a manuscript kept at Thorn and which is surely the same compilation.

This compilation still had the name of Jordanus when it went to the printer. It was, indeed, one of the essential elements of the book published in 1533 in Nuremberg by Peter Apian, Professor at the University of Ingolstadt. But another element was added to the preceding one to comprise the treatise which Peter Apian published, We shall now study this second element.

## 2. THE PERIPATETIC TRANSFORMATION OF THE *ELEMENTA JORDANI*

Under the number 7378 A, Latin collection, the Bibliothèque Nationale keeps a collection of disparate pieces on mathematics, astronomy and mechanics. Among these pieces is a 13th century manuscript[5] on parchment which contains a long and important document on statics. This document which begins with the following words: *Incipit liber Jordani de ponderibus*, is actually a sequence of three distinct texts.

The third of these texts contains a series of problems on the equilibrium of the "canonium" which we discussed at length at the end of Chapter V. The second text will be discussed in a later paragraph. The first will now receive our attention.

A rather long preamble, giving a general overview of the problems treated and the methods used to solve these problems, precedes the list of axioms used by Jordanus as the principles upon which he bases his deductions. Thirteen propositions follow these axioms. The order of these propositions and the form of these statements are exactly the same as those in the *Liber Euclidis de ponderibus*, but the demonstrations and explanations accompanying them are, as we soon shall see, completely different. The result of the analysis we shall make of them will justify the name of the *Peripatetic Commentary on the Elementa of Jordanus*, as we will henceforth call this treatise.

The *Liber Euclidis de ponderibus* on the one hand, and the *Peripatetic Commentary* contained in our 13th century manuscript on the other hand, were combined in a single work which was printed during the 16th century. It was published in 1533 in Nuremberg by Joannes Petreius with the title:[16]

Liber Jordani Nemorarii, viri clarissimi, de ponderibus, propositions XIII et earumdem demonstrationes, multarumque rerum rationes sane pulcherrimas complectens, nunc in lucem editus, cum gratia et privilegio imperiali, Petro Apiano, mathematico Ingolstadiano, ad XXX annos concesso. MDXXXIII.[17]

Here is how the famous cartographer, Peter Apian, Professor at the University of Ingolstadt, undertook to compose this small book.

After a dedicatory epistle addressed to Leonhard van Eck, Vuolffeck and Randeck, he reproduced the preamble which begins the *Peripatetic Commentary*. Then the postulates of Jordanus, followed by thirteen propositions (nine by Jordanus and four from the *De canonio*). These propositions are in the same form and sequence which they had in the treatises from which they were taken and which they had in the *Liber Euclidis* and the *Peripatetic Commentary*.

Each proposition is followed by two demonstrations. The first demonstration is the pure and simple reproduction of argumentation given in the *Peripatetic Commentary*. The second demonstration, introduced in these general terms: "Sequitur aliud commentum,"[18] has as its outline the demonstration given in the *Liber Euclidis*, which is itself an amplification of an original deduction by Jordanus.

Peter Apian renders their argumentation even more diffuse and verbose than it was in the *Liber Euclidis* by belaboring it with geometrical digressions. It is in one of these digressions, by the way, that he twice quotes Campanus. This quote was afterwards attributed to Jordanus and continued to mislead his chroniclers. These long and tangled constructions of Apian are a far cry from the argumentation of the *Elementa Jordani*, whose clarity and objectivity reveal the work of a true geometer despite grave errors in certain principles.

Let us return to the *Peripatetic Commentary*, which furnished Peter Apian with one of the consitutive elements of his edition. This commentary begins as follows:

The science of weights is subordinated as much to geometry as it is to natural philosophy. It is, therefore, necessary that certain propositions of this science receive a geometrical proof, while others need a philosophical proof.

Consequently, the author proves to be much more concerned with considering the laws of equilibrium and of motion from a philosophical perspective than did Jordanus and the author of the *Liber Euclidis*.

A reading of the *Commentary* will but confirm this first impression. Several times in the course of his discussions, the author refers to the principles of Aristotle's physics. Thus, in order to determine the positional gravity of a material point placed on a circle, he assumes, like Jordanus, that the point descends along a small arc. But are the conclusions reached in this way, which are valid when the point is in motion, also valid for an immobile point? The author poses for himself the objection which, so often and for so long, will be opposed to the Method of Virtual Displacements. In order to answer this objection — and he does so in a very obscure and inconclusive way — he considers rest as the limit of motion. In that state, nature is entirely "enacted" while during the duration of the motion, nature is partially potential. These are the very same principles developed in Aristotle's *Physics* on the subject of natural motion.

Although our author is more knowledgeable about and more concerned with Peripatetic physics than Jordanus was, he is certainly inferior as a geometer and logician. He almost always substitutes for the precise and clear argumentation of the original text of the *Elementa Jordani* vague considerations without a trace of a conclusive argument.

The influence of the *Mechanical Problems* on Jordanus seems undeniable. However, Jordanus was able to reflect on the ideas which he found there and to impose on them his own personal form. The originality of his doctrine might even lead one to believe that he had no direct and immediate knowledge of Aristotle's treatise. Our commentator followed the *Mechanical Problems* much more slavishly; even worse, he forced out of them an erroneous conception of the concept of positional gravity, which Jordanus was careful to avoid.

Indeed, the *Mechanical Problems*, do not have the same beautiful and logical order of most of Aristotle's works. There, opinions are intertwined in such a way that it is difficult to disentangle them. This lack of order is certainly one of the best arguments critics can invoke who claim that this work should not be attributed to the Stagirite, but to one of his disciples.

What is the cause which makes the gravity of a material point descending on a circle vary, according to Aristotle? If one goes by the passages which we quoted in the preceding chapter, one can see that the Philosopher considers this gravity a resultant of two forces: natural gravity and an unnatural resistance directed along a horizontal line. These two forces are in the ratio to each other as the two components of the path traversed by the weight in its curvilinear trajectory. If one wants to understand the effects of gravity in circular motion, one must take into consideration the length of the veritcal projection which corresponds to a given displacement on the circumference, that is to say, the obliquity of the trajectory. It was in this way that Jordanus interpreted Aristotle's ideas when he took them as the foundation of his statics.

In the *Mechanical Problems*, the passages yielding the interpretation given by Jordanus are mixed together with other passages according to which the gravity of a mobile point on a given trajectory depends on the curvature and not on the obliquity of the trajectory. An example is the following pasage:

> If two bodies are moved by the same force and the trajectory of one body curves more than the trajectory of the other body, it is logical that the body describing the least curved trajectory moves faster than the body describing the most curved trajectory. This seems to happen for the bodies moving on the largest and the smallest of two circles drawn from the same center. The point which moves on the smaller circle is closer to the center than the point moving on the larger circle. Furthermore, the point which moves along the larger circle is carried more rapidly than the point moving along the smaller circle as if it were pulled in the opposite direction, that is, towards the center. In addition, the same thing happens to any point describing a circle: it is carried naturally along the circumference and also unnaturally, towards the side and towards the center. But the point describing the smaller circle is carried by a larger unnatural motion because by the very fact that it is closer to the center it is restrained and subjected to a greater force.

It is this obscure passage which seems to have caught the attention of our commentator. When he tries to consider curvature, where Jordanus very wisely had only introduced the analysis of oblique lines, everything which was clear and precise in Jordanus becomes confused.

The scientific value of our *Peripatetic Commentary* is, therefore, zero. Its influence, however, will be felt for a long time, even by very great geometers. Even Tartaglia, Guido Ubaldo, and Mersenne are not entirely immune to this influence.

## 3. THE PRECURSOR OF LEONARDO DA VINCI: DISCOVERY OF THE CONCEPT OF MOMENT. SOLUTION TO THE PROBLEM OF THE INCLINED PLANE.

The work to which we shall now turn our attention is, on the contrary, one of the most important in the history of mechanics. It can be found in the same manuscript[19] which contains the *Peripatetic Commentary* and is a continuation of it. Thus this work cannot be later than the 13th century, the era when the manuscript was copied.[20] It can also be found in more correct form and illustrated with many precise figures in another 13th century manuscript[21] under the same title: *Liber Jordanis de ratione ponderis*. Furthermore, these copies seem to be much later than the original. Certain demonstrations have lacunae, incomprehensible passages which it would be impossible to attribute to the author of this treatise because he was surely a good geometer.

This treatise was printed in the 16th century. Among his papers, Tartaglia left us a copy which he had illustrated with several figures. He bequeathed it to his friend, Curtius Trojanus, the great Venetian publisher, with a charge to publish it. And, indeed, in 1565 Curtius Trojanus published it as a small book[22] to which he appended the *Treatise on Weights* by the pseudo—Archimedes as well as several experimental conclusions on specific weights by Tartaglia.

Curtius Trojanus published this work as if it were the result of corrections Tartaglia made on the original manuscript. As a matter of fact, Tartaglia had corrected nothing at all. If one compares the printed treatise with the manuscript text which we actually saw, one can see right away only one change: the division into four books in the manuscript no longer appears in the book form. Several additions can also be discovered and most of them are unfortunate. For the most part, the original text has been reproduced purely and simply by the printer, who was, however, rather clumsy in deciphering a 13th century script. Thus the numerous mistakes committed by the scribes multiply here. The word "pondus" has become "mundus", and "regula" is constantly replaced by "responsa".[23] Moreover, the figures are just as bad as the text. Written in indistinguishable letters which hardly correspond to the notations of the argument and filled with poorly drawn lines where horizontals sometimes become verticals, these figures further increase the disorder and confusion. In a word, everything combined to render unrecognizable the new mechanical ideas which distinguished this treatise so much from the *Elementa Jordani*.

We shall now analyze these ideas. Yet, one question about them comes to mind: which geometer conceived these ideas? We find ourselves unable to answer this question. The manuscripts which contain these ideas imply that they stem from Jordanus. It is true that the treatise of Jordanus was the inspiration which led to the work which occupies us here. However, the latter cannot be considered a simple development of the former. While it corrects certain mistakes of the *Elementa Jordani*, it develops in a successful manner the fruitful suggestions contained in the treatise of Jordanus. It is an original and convincing work. To cite it under the name of Jordanus de Nemore would be to increase the already rich patrimony of that author by attributing to him something which is not his at all. However, since we do not know the name of the true author, we shall call him the Precursor of Leonardo da Vinci.[24] We shall see that the ideas of this unknown geometer certainly exerted a profound and productive influence on the investigations into mechanics by the great painter.

The fourth book of the treatise by the Precursor of Leonardo da Vinci is concerned more with dynamics than statics. It deals with, above all, the effects of a fluid medium — be it air or water — on the bodies moving within that medium. To be sure, one does not expect a 13th century mechanician to express precise and profound ideas about problems which even today seem to us almost inaccessible. In any case, these ideas really have no place in a study on the *Origins of Statics*. We shall mention, however, some of these views expressed by our author on dynamics, because they are quite original and easily identifiable so as to allow us to recognize their imprint in the works of his successors, especially in Leonardo da Vinci.

Any medium resists the motion of a body which is travelling through it.[25] This impediment to motion depends on a great number of circumstances. First of all, it depends on the shape of the moving body.[26] The more pointed its shape and the smoother its surface, the more easily it will travel through the medium. Secondly, it depends on the density of the fluid which the body travels through.[27] A more dense medium is more difficult to penetrate than a less dense one. Water resists more than air. According to our author, every medium is compressible. Since the upper layers of fluid push down on the lower layers, the latter ones are denser than the former.[28] The deep layers will thus resist motion more than the upper layers.

At the bow of the moving body a portion of the medium is compressed and adheres to the body.[29] But other portions of the medium repulsed by the moving body bend back and come to occupy the space vacated by the body.[30] This curved motion of the lateral parts of the medium can be compared to the bending of a bow. When the middle part of a body is fixed, an impulse exerted on the extreme parts will easily bend this body.[31]

Aristotle attributed the continuation of the projectile's motion after the projectile had left its motor to the motion of the medium. This opinion was shared by Alexander of Aphrodisias, Themistius, Simplicius, Averroes, and by St. Thomas Aquinas. What our author attributes to the motion of the medium is not the continuation of the projectile's motion, but the acceleration of this motion. It is the motion of the air which explains the acceleration of the fall of heavy bodies. This acceleration was already known to Aristotle and his commentors and was considered by them as the effect of an increase in weight. Let us quote the passage where the Precursor of Leonardo da Vinci formulates this curious theory on accelerated fall:[32]

The longer a heavy body descends, the faster it moves. This is more true in air than in water, because the air is suitable to all kinds of motions. Thus a heavy body descending in its first motion pulls the fluid immediately behind it and puts into motion the fluid directly below its immediate contact point. The parts of the medium which are thus put into motion, in turn, put into motion those which follow them in such a way that these, already in motion, produce a smaller impediment to the descending body. Because of this the body becomes heavier and imparts a stronger impulse to those parts of the medium being pushed in front of it, so that these parts are no longer simply pushed by it, but they actually pull it. Thus it happens that the weight of the moving body is increased by their traction and, conversely, their motion increases with this added weight so that this motion continually increases the velocity of the falling body.

Such an explanation of the accelerated fall of heavy bodies seems to have been unknown to the Ancients. Simplicius, who lists all the different views of his predecessors on this phenomenon, does not mention it. On the contrary, many authors of the Middle Ages and the Renaissance viewed it favorably. Walter Burley (Burlaeus), who wrote on Aristotle during the first half of the 14th century, adopts it in his commentaries to Book II, Chapter 76 of the *Physics*. These few ideas concerning the influence exerted by the medium on the fall of heavy bodies, which we have just expounded, seem to have been a kind of breviary for Leonardo da Vinci, which he never ceased to contemplate and which was the source of a great many of his views on the topic of dynamics. We find these views again in the works of Cardan. The explanation which our 13th century author had given for the accelerated fall of heavy bodies is also accepted by Cardinal Gaspard Contarini in the first book of his *De elementis*, printed in 1548, six years after the death of its author. In 1576, Benedictus Pererius adopted this explanation in his *De communibus omnium rerum naturalium principiis.*[33] Finally, Gassendi adopted them in 1640 in a letter addressed to P. Casrée, where the modern theory of the accelerated fall of heavy bodies is presented for the first time in a complete way.

From this, one can understand the essential role played by our 13th century geometer in the development of the science of motion. However, this role should not concern us now. Our present object is to evaluate the contribution to the science of equilibrium by the Precursor of Leonardo da Vinci. This contribution, as we shall see, is enormous.

The first three books of the treatise which we are analyzing deal with statics. The second book, restricted to problems related to the *De canonio*, contains few new ideas. Only the first and third books will concern us.

The first book begins exactly like the *Elementa Jordani.* The same axioms follow each other in the same order. The first proposition in both works sets forth in the same form the fundamental principle of Peripatetic dynamics. But from the second proposition on,[34] the originality of the Precursor of Leonardo becomes evident.

We have already analyzed the formulation and demonstration of this proposition in the preceding chapter.

Jordanus considered a lever *bc* (Fig. 21) rotating about point *a* with equal arms carrying equal weights. This lever is displaced from the horizontal position in such a way that weight *b* is lowered and weight *c* elevated. In this position, weight *c* should be heavier than weight *b*, because the descent along the arc *cd* is less oblique than the descent along the equal arc *bg*.

It is true that the Precursor of Leonardo borrows the formulation and demonstration of this proposition from Jordanus, but he adds to the demonstration some considerations which refute it conclusively.

The excess of obliquity of arc *bg* over the obliquity of arc *cd*, says our author, can be reduced by any arbitrary amount. Therefore, if one puts some weight at *c* and at *b* a weight which exceeds the weight at *c* by any fixed amount, then weight *b* will descend and cause weight *c* to ascend (regardless of obliquity).

After having made this correction to an error of Jordanus, our author reproduces several of his predecessor's propositions without any important modifications. Specifically, he borrows from him the elegant demonstration of the equilibrium of the straight lever and finally comes to the following problem:[35] If a balance (Fig. 24) with two unequal arms *ca* and *cb* which form a fixed angle have the end-points *a* and *b*, which are equidistant from the vertical line passing through the point of support *c* and if they carry equal weights, is the balance in equilibrium or not?

Jordanus had treated this specific problem or, at least, a particular case of the problem. He had concluded that the balance would not be in equilibrium under the prescribed conditions and that the balance would incline with the smaller arm *cb* descending and the longer arm *ca* ascending.

The Precursor of Leonardo, on the contrary, gives the correct answer to the problem posed. The balance will remain in equilibrium. However, he does not only formulate this correct answer but he proves it with a most remarkable demonstration.

On both sides of the arm *ca*, he draws two radii *cx* and *cl* which form equal angles with *ca*. Then, on both sides of the other arm *cb*, he draws two radii *ch* and *cm* which form equal angles with *cb* as well as with the angles first drawn.

Having done this, he asks if weight *a* would incline the balance to its side and then states that this will not happen, because arms *ca* and *cb* of the lever would assume, respectively, positions *cx* and *cm*. Weight *a* will descend by the distance *tx*, and will cause the equal weight *b* to ascend by a distance *pm*, larger than *tx*. In the same way, weight *b* will not be able to incline the balance to its side, because arm *cb* will assume position *ch* while arm *ca* will assume position *cl*. Weight *b* descending by a distance *rh* will cause the equal weight *a* to ascend by a distance *nl*, larger than *rh*.

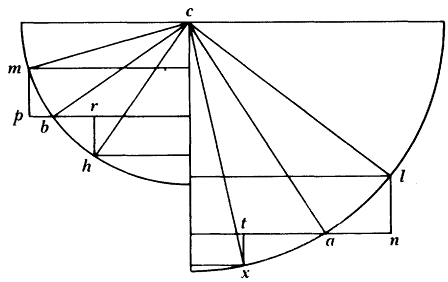


*fig. 24.*

This demonstration shows an obvious relationship to the demonstration given by Jordanus for the law of equilibrium of the lever. But this new application of the Method of Virtual Displacements presented certain difficulties not encountered with the first method. Indeed, in the case of the straight lever, the equilibrium is not affected, so that in any completed virtual displacement the work of the driving forces equals the work of the resisting forces. But in the case of the bent lever, the equilibrium is stable and equality between the motor work and the resisting work will not occur except for infinitely small displacements, which a 13th century geometer would not have been able to treat. The Precursor of Leonardo da Vinci understood how to overcome these difficulties in a most felicitous manner. One can easily achieve the condition of equilibrium of any arbitrary bent lever with any arbitrary weight suspended from its arms by combining the demonstration which we have just seen with the following principle:

What suffices to raise a given weight to a given height also suffices to raise a weight *n* times smaller to a height *n* times larger.

In the demonstration on the law of equilibrium of a straight lever, Jordanus had implicitly recognized this principle and he demonstrates it as just shown. If, he says,[36] a bent lever *abc* (Fig. 25) supports at *a* and *b* unequal weights, it will assume an orientation such that distances *ad* and *be* from the points *a* and *b* to the vertical *ch* through the point of suspension are in inverse proportion to the weights suspended from these same points.

These rules can also be stated in the following way: the effect of a weight suspended from the extremity of an arm of lever inclined in any arbitrary way is measured by the "moment" of this weight with respect to the vertical through the point of support.

This way of formulating the problem we have just discussed does not escape the attention of our author.

If, he says,[37] one lifts a load and if one knows the length from the weight to the pivot, one can determine the weight of the load for any position . . . The weight of this load lifted to point *e* by the arm *be* (Fig. 26) will be in the same ratio to the weight lifted to point *f* by *fb* as *el* is to *fr*, or as *pb* is to *xb*. . . . The weight placed at *e* at the extremity of the lever *be* will have the same effect as if it were placed at point *u* on the lever *bf*.

In this way, the law which Jordanus had formulated by defining posi-

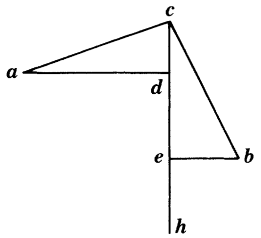


*fig. 25.*

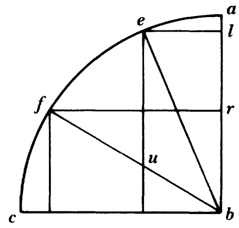


*fig. 26.*

tional gravity as decreasing as the arm of the lever approaches the vertical, is finalized and put into a quantitative form.

Once this concept was understood, it was easy to recognize that a balance is in a state of stable equilibrium when, by joining the point of support to the points of the suspensions of the weights, one is able to form an angle with the summit directed upwards. It is certain that our author perceived this truth.[38] If the same angle is turned downwards the only state of equilibrium which the balance can achieve is a state of unstable equilibrium. The Precursor of Leonardo also formulates clearly this proposition,[39] which contradicts an assertion in the *Mechanical Problems*. Aristotle, indeed, had claimed that such a state of equilibrium was indifferent.

The law of the equilibrium of a bent lever, obtained by an ingenious application of the Principle of Virtual Displacements, and the clearly understood concept of moment are two discoveries which in themselves would assure the Precursor of Leonardo a prominent place among the seminal minds in the field of statics. But they are not the only discoveries by which he enriched mechanics. This science also owes to him the solution to the problem of the inclined plane.

Only a single geometer of Antiquity, Pappus, concerned himself with this problem and gave an erroneous solution, which we shall discuss in the following chapter. The mechanicians of the Middle Ages do not seem to have known his solution and it had no influence whatsoever in their research.

It is surprising that Jordanus de Nemore did not consider taking up the problem of the inclined plane, because in no other problem was the application of the concept of positional gravity simpler and more obvious. Circular motion, the only kind considered by this great geometer, can not be applied as easily to the consideration of positional gravity.

If Jordanus had thought of treating the problem of the inclined plane with his principle and if, unsatisfied with merely asserting that the positional gravity is greater the more a given path measured along the trajectory projects on the vertical, and if he had also recognized that the positional gravity is proportional to the length of this vertical taken by an oblique descent of a given length, he would have immediately succeeded where Pappus had failed.

Once again, this application seems to fit so naturally the postulates of the *Elementa Jordani super demonstrationem ponderis* that one is surprised to see the author of this treatise let others gain the glory of carrying it out.

This honor was reserved for the Precursor of Leonardo, who observes first of all[40] that the positional gravity of a weight resting on an inclined plane is the same whatever the position of this weight on the plane. He then turns to the comparison of values of this positional gravity on differently inclined planes. Let us translate extensively from the 13th century manuscript the statement and demonstration of this essential proposition:[41]

If two weights descend on two differently inclined planes and if these weights are directly proportional to the inclination, these two weights will have the same capability in their descent.

Let *ab* (Fig. 27) be a horizontal line and *bd* a vertical line. Let us assume on either side of *bd* two oblique lines *da* and *dc* and that *dc* is of greater relative obliquity. By proportion of obliqueness I mean the proportion of the inclination, not the proportion of angles, that is to say, the proportion of the lengths of the lines measured up to their intersection with the horizontal in such a way that they project equally on the vertical.

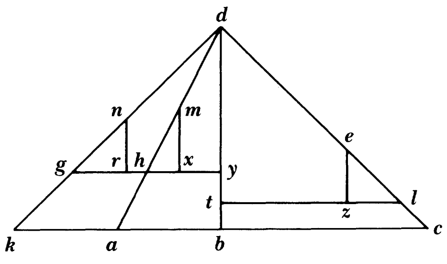


*fig. 27.*

Secondly, let *e* be the weight put on *dc* and *h* the weight put on *da*. Let us assume that *e* is to *h* as *dc* is to *da*. I maintain that in such a situation these two weights will have equal capability.

Let there be a line *dk* with the same obliquity as *dc* and let there be on this line a weight with the same obliquity as *dc* and let there be on this line a weight *g* equal to *e*.

Let the weight *e* descend to *l* if possible and let it pull[42] weight *h* to *m*: let us assume *gn* equal to *hm* and, consequently, to *el*. Let us draw a perpendicular to *db* through the points *g* and *h*. And let *ghy* be that perpendicular. From point *l* let us draw the perpendicular *let* to *db*. Let us then draw the perpendiculars *nr* and *mx* to *ghy*, and the perpendicular *ez* to *lt*.[43] The ratio between *nr* and *ng* is equal to the ratio between *dy* and *dg* and it also is the same as the one between *db* and *dk*.

The ratio between *mx* and *mh* is equal to the ratio between *db* and *da*. Thus *mx* is to *nr* as *dk* is to *da*, that is to say, as the weight *g* is to the weight *h*. But since *e* could not lift *g* to *n*, it also cannot lift *h* to *m*. Thus the weights will not move.

The demonstration is copied from the one Jordanus gave of the law of the equilibrium of the lever. It puts into play the same implicit postulate. What suffices to lift weight P to height H, can also lift the weight P/k to the height kH. This postulate not only led Jordanus to a convincing proof of the law of equilibrium of the straight lever, but also allowed the Precursor of Leonardo da Vinci to resolve both the problem of the bent lever and the inclined plane. The fecundity of this principle becomes obvious from the 13th century on. It will continue to be productive up to our day. This principle is the true origin of the Method of Virtual Displacements. Its breadth and power are still admired by modern physicists. This principle, through the meditations of Jordanus and the Precursor of Leonardo, will be further developed in the works of Leonardo, Guido Ubaldo, Galileo, Roberval, Descartes and Jean Bernoulli and will come to full fruition in the works of Lagrange and J. Willard Gibbs. But before we are able to accurately estimate the influence exerted by the Precursor of Leonardo on the mechanical research of the great painter, it remains for us to analyze a final treatise coming out of the School of Jordanus.

## 4. THE *TREATISE ON WEIGHTS* ACCORDING TO MASTER BLASIUS OF PARMA

Biagio Pelacani, otherwise known as Blasius of Parma, came from an illustrious family of Parma known for the physicians, scientists and philosophers which it produced, such as Antonio Pelacani, who died in

1327 in Verona, or Francesco Pelacani, who practiced medicine in 1438 in Parma.

Blasius[44] was also a physician, but as with so many other physicians of his time no area of science was unknown to him. After having become a Doctor of the University of Pavia in 1374, he taught astrology in Bologna from 1378 to 1384. He then taught till 1388 in Padua and then again in Bologna. In 1404, 1406 and 1407 we find him again in Pavia. He also found time to travel to Paris. From 1408 to 1411 he resumes his chair at Padua and dies in his native town of Parma on April 23, 1416. Tiraboschi calls him "filosofo e matematico insigne."[45] Indeed, Luca Pacioli cites him as one of the authors he made use of to write his *Summa de arithmetica geometria*. Leonardo da Vinci and Cardan mention his research on statics. Some of his works and, in particular, his commentaries on Nicolas Oresme's *De latitudinibus formarum* were printed[46] in the 16th century. His numerous changes of residence seem to be due to some defect in character. The students in Padua refused to take his courses because of his rudeness and his avarice. The Parisians, finally, composed this unflattering maxim about him: "It must be the devil, unless it is Blasius of Parma — *Aut diabolis, aut Blasius Parmensis*."

The manuscript[47] which preserved for us the *Tractatus de ponderibus secundum Magistrum Blasium de Parma* stems from the pen of Arnold of Brussels, who finished his copy of it on January 5, 1476 in Naples.

This work is composed of three parts. In it Blasius attempted to set forth in a coherent form the doctrines of the School of Jordanus. Blasius only partially achieves the goal he sets for himself in his work. *The Treatise on Weights* contains more than one inconsistency.

The third part deals with hydrostatics. It will be of great interest to us when we study the development of this science. It is obvious that in this third part Blasius of Parma uses the *Treatise on Weights*, falsely attributed to Archimedes, whom he calls Alaminide. This book is, however, not the only one which he consulted. The description which he gives of the constant weight aerometer closely resembles the description given in a certain *Carmen de ponderibus* or *de ponderibus et mensuris*,[48] which the collection of the *Poetae latini minores*[49] falsely attributed to Priscian. But since we are concerned here with the general history of statics and not with the narrower history of hydrostatics, let us not dwell further on this third part but be satisfied with having briefly referred to it.

The first two parts of the treatise of Blasius of Parma deal with the study of statics. The principles of statics discussed there are clearly connected with the School of Jordanus. However, Blasius does not seem to have used the original text of Jordanus de Nemore. His work is a kind of synthesis of the book we called the *Peripatetic Commentary* and the treatise by the unknown author whom we have called the Precursor of Leonardo da Vinci. Other works unknown to us might also have contributed to parts one and two. The first part of the book clearly shows a familiarity with the preamble to the *Peripatetic Commentary*. But two theorems among the geometrical propositions demonstrated in this part are precisely those considered by Jordanus to have been established in the *Philotechnes*. The preamble to the *Peripatetic Commentary* mentions only one of the two theorems. Could it not be possible that Blasius of Parma had before him this *Philotechnes* which has since been lost?

The influence of the *Peripetatic Commentary* on the mind of Blasius of Parma is especially evident in the first part of his treatise. The affinity is obvious from the first page on: "Cum scientia de ponderibus sit subalternata tam geometriae quam philosophiae naturali,[50] says the 13th century author at the beginning of his work, and Blasius begins his work with these words: "Scientia de ponderibus philosophiae naturali vere dicitur subalternari."[51]

It is pure Peripatetic physics which guides Blasius to the concept of positional gravity.

A heavy body, he says, situated outside of its natural place, tends to descend by the chord rather than by the arc, because when it is outside of its natural place where it must stay for its conservation and perfection, it tends to return to this place as soon as possible and by the shortest possible path. A heavy body is all the heavier the more directly it descends towards the center. The obliquity of the curvature of its trajectory decreases its positional gravity.

In many cases Blasius of Parma diverges from Jordanus and draws closer to the *Peripatetic Commentary* by attributing the decrease of gravity to the curvature and not to the obliquity.

Blasius of Parma deduces a curious corollary from the concept of positional gravity which we find nowhere in the works of his predecessors:

If a balance with equal arms carrying equal weights is moved away from the center of the earth, these weights seem heavier the higher the balance is placed.

Indeed, the line along which each of the weights tends to fall forms with the vertical which passes through the point of support of the beam an angle all the more acute the further the balance is from the center of the earth. It is rather amusing to remark that Mersenne and Descartes will repeat this same argument word for word.

The second part of the treatise by Blasius is essentially formed by combining the *Elementa Jordani* with the *De canonio*, just as are the *Peripatetic Commentary* and the *Liber Euclidis de ponderibus*. Blasius of Parma attempted to erase any differences existing between the two works so combined. The first proposition of the *De canonio* no longer refers to "books dealing with these matters." It invokes the last theorem demonstrated by Jordanus. In the propositions borrowed from Jordanus (who, by the way, is never mentioned), the beam of the balance is sometimes called a "canonium" by Blasius. However, the joint between these two heterogeneous texts remains visible, and can scarcely be hidden behind this semblance of a transition: "Nunc, datis ponderibus volo notitiam brachiorum indagare (sic!)."[52]

In the second part of the treatise, it is still the *Peripatetic Commentary* which unfortunately, continues to inspire the demonstrations of Blasius of Parma.

Let us take, for example, the demonstration which he substitutes for the argumentation of Jordanus, so rich in consequences, when he attempts to justify the law of equilibrium of the lever.

He considers a lever (Fig. 28) with one arm *bc*, four times the length of the other arm *ab*. He draws the arcs of the circle *am* and *cn*, which

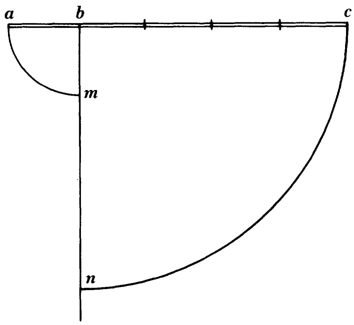


*fig. 28.*

weights placed at *a* and *c* would describe in their descent. If these weights are equal, they cannot be in equilibrium

because the quadrant *am* is more curved than the quadrant *cn* so that the weight suspended from the longer arm will be heavier to the same extent that the corresponding quadrant will be straighter. . . . And since the degree of curvature of quadrant *cn* is in the same ratio to the degree of curvature of quadrant *am* as *bc* to *ab*, the positional gravity of the weight placed at *a* will be four times that of the positional gravity of an equal weight suspended at *b*.

Blasius of Parma, however, does not escape the influence of the Precursor of Leonardo da Vinci. It is from this author that he undoubtedly borrowed the following statement and demonstration. He assumes that equal weights are attached to points *a* and *c* (Fig. 29) on the line *abc* with *b* its midpoint. He further assumes that the point of support is at *o* on the vertical drawn downward from point *b*. Under these conditions, he declares that,

it will be difficult to put the weights in equilibrium.

Let us suppose, indeed, that *a* descends by as little as one wishes. The arm to which *a* is attached becomes longer, while the other arm become shorter; consequently, *a* becomes increasingly heavier and *c* lighter.

When Blasius of Parma borrows his statements from the Precursor of Leonardo, he unfortunately does not always borrow his demonstrations. He considers for instance, a bent lever *acb* (Fig. 24) with unequal arms *ca* and *cb* but with their extremities *a* and *b* being equidistant from the vertical passing through the point of suspension *c*. In accordance with the Precursor of Leonardo, Blasius asserts that equal weights

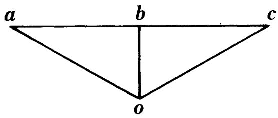


*fig. 29.*

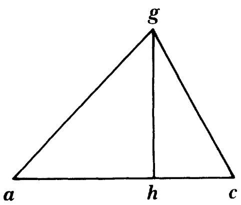


*fig. 30.*

placed at *a* and *c* will remain in equilibrium. But he substitutes for the elegant demonstration of his predecessor reflections lacking any recognizable trace of proof.

When Blasius of Parma read the Precursor of Leonardo he could not have helped but notice the theory of the inclined plane. Yet, he apparently did not grasp the elegance and rigor of that theory. He seemed only to have been struck by the following fact which appeared paradoxical to him: A weight sliding over a steeply inclined plane can cause a heavier weight than itself to rise along a plane less steeply inclined. Here is, by the way, the curious passage in which he expounds his thoughts:

... And furthermore this can be demonstrated by virtue of our sixth principle in the following way: Every heavy body tends to descend by a vertical line rather than an arc, provided that the heavy body can follow its natural tendency. Therefore, let *hg* be the vertical (Fig. 30). It follows from the preceding demonstration that the closer a descent is to the vertical *gh*, the more this descent participates in natural motion, as long as the weights considered are always equal. From this follows what we now propose to demonstrate.

That it is so evident. Let us draw two straight lines *cg* and *ag* with the line *cg* being less inclined from the vertical than *ag*. The angle *cag* is more acute than the angle *acg*. This angle *cag* cuts a smaller arc.

But here a question arises in our mind: The weight *c* could possibly life a heavier weight than weight *a* which is equal to *c*? And consequently, it seems that it must be so. Then, indeed, since weight *c* in such a position is heavier than the weight *a*, its gravity in this position exceeds that of weight *a* by a given amount. From this, it follows that the weight *c* can lift a weight which is heavier than *a* itself. Otherwise, the active force would admit an affirmative term, *per maxim.*

But this conclusion seems to imply a contradiction: because it is certain that *c* and *a* are equally heavy bodies which necessarily entail the following conclusion: whatever is heavier than *a* is also heavier than *c*. It follows that anything heavier than *a*, put on the balance with *c*, causes the weight *a* to necessarily descend to the bottom, in conformity to our third conclusion. I shall say here nothing about this process ... Videant tamen philosophantes.[53]

Moreover, Blasius seems to have been of a paradoxical and skeptical mind. He takes pleasure in pointing out surprising consequences, in juxtaposing contradictory propositions and in bringing up objections to the theorems furnished by his predecessors. The passive resistances which hardly seemed to bother the geometers of the School of Jordanus disturb Blasius of Parma. He observes that these resistances prevent one from drawing any certain conclusion from the equilibrium of the balance about the equality of the weights in their pans. He remarks that the accuracy of more than one proposition of statics requires that one neglect resistance on the part of the medium. He even attempts, in a naive form, to treat a problem of equilibrium by taking into account just such a resistance.

To be sure, the treatise by Blasius of Parma lacks the forceful originality so evident in the work due to the unknown author whom we have called the Precursor of Leonardo. The former work contains nothing which influenced the development of statics in any positive way. It only represents a curious monument to the knowledge spread among the physicists at the beginning of the 15th century. Yet, it is not without interest as far as our study here is concerned, because as we shall see, it is one of the channels by which the mechanics of the Middle Ages reached Leonardo da Vinci and Cardan.

CHAPTER VIII

# THE STATICS OF THE MIDDLE AGES AND LEONARDO DA VINCI

Science knows no spontaneous generation. The most unexpected discoveries were never created *in toto* by the mind which gave birth to them, but they always issued from a seed first planted in the mind of a genius. The role of the genius was limited to making the small seed within him germinate and grow until the tree in full foliage might offer its flowers and fruits. When we studied the statics of Leonardo da Vinci,[1] we admired the most luxuriant and dense growth of new ideas imaginable. We shall now search for the seeds which gave birth to this veritable forest of discoveries. Our knowledge of medieval statics will be of great help to us in our search. It will be possible for us to disentangle the influences exerted on the mind of the great painter. We shall see how his views on the topic of statics grew, sometimes by developing certain propositions formulated by a geometer belonging to the School of Jordanus, and sometimes by refuting assertions made by a mechanician from the same School.

## 1. THE SCHOOL OF JORDANUS, THE TREATISE OF BLASIUS OF PARMA AND THE STATICS OF LEONARDO DA VINCI

We need not leaf through Leonardo da Vinci's manuscripts for very long to recognize that Leonardo's attention was caught by the problems which take into account not only the weights suspended from the beam of a balance, but also the weight of the beam itself, as well as by the questions treated by Charistion and Thâbit ibn Qurra.

> If you want to test a man, he says,[2] and see if he has a true understanding of the nature of weights, ask him at what point one must cut one of the two equal arms of a balance so that the segment cut off and attached to the extremity of the remainder will precisely counterbalance the opposite arm. Since this is never possible, if the man points out the place to you, he is a dismal mathematician.

The premises used by Leonardo da Vinci to treat this sort of problem are quite analogous to those given in the book published by

Thâbit ibn Qurra or more summarily set forth in the last proposition of Jordanus. Let us quote several examples:

*On weight.*[3] If the two arms of a balance (Fig. 31) are divided equally and if a pound weight is placed at each of the points *a, b, c, d,* and *e*, the question is how many pound weights will be needed at *f* to counterbalance these weights?

This is how to do it: *a* counterbalances a one pound weight placed at *f, b* two pounds, *c* three pounds, *d* four pounds and *e* five pounds so that the sum total will counterbalance fifteen pound weights placed at *f*.

*On weight,* If a balance (Fig. 32) has a weight which is equal in length to one of its arms, let it be MN, which weighs six pounds, how many pound-weights placed at F will counterbalance it? I maintain that three pounds will suffice because if the weight MN has the same length as one of the arms, you will be able to recognize that it is placed at the midpoint A of this arm of the balance. Thus, if there are six pounds, six other pound-weights placed at R will counterbalance them and if you move forward again the distance R to the extremity of the balance to point F, three pounds will counterbalance them.

*On the center of gravity.*[4] The center of suspended gravity is on the central line of the rope which supports it. One can prove this by considering the weights B and D suspended from the first arm (Fig. 33). If these weights were one single weight, they would have their center of gravity at the mid-point E between the two lines of suspension. One must admit this, because weight A counterbalances weight B, since both have equal moment arms from the fulcrum on the balance — and because the second weight C counterbalances weight D. Thus the distances proportional to the weights are MN and ME, which are in a ratio of 3 to 2 as are the weights but in an inverse manner; that is to say, D and B counterbalance weights A and C. Therefore, it is proved that the point E is the center of gravity for the suspended weights B and D, whether the weights are considered individually or combined. I believe that I have proved the same thing for the second figure (Fig. 34).

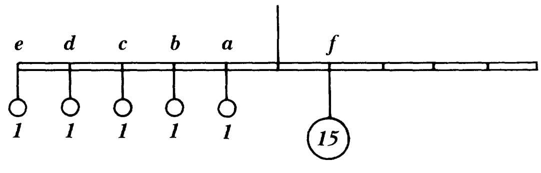


*fig. 31.*

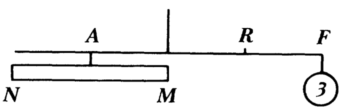


*fig. 32.*

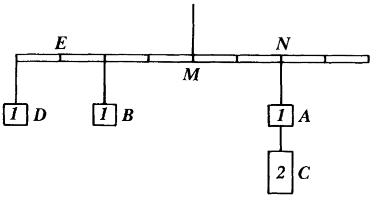


*fig. 33.*

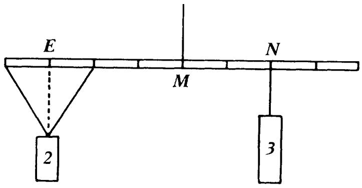


*fig. 34.*

These quotes, for which we could find numerous other examples,[5] show us that Leonardo was familiar with the mechanical science developed by his predecessors. Will we be able to say where he had obtained this knowledge and can we cite the manuscripts which he perused? There is one treatise among the ancient treatises we analyzed, which we know Leonardo had read and criticized; it is the *Treatise on Weights* by Blasius of Parma.

In his disparaging rage, Libri had torn out a number of pages from Leonardo da Vinci's notebook kept in the Bibliothèque de l'Institut. After having been sold to Lord Ashburnam, together with a part of the collection which Libri had amassed by pillaging all the libraries put in his charge, these sheets returned to France with the collection of which they were a part. They are kept at the Bibliothèque Nationale. Among the sheets which France was able to regain thanks to the efforts of Mr. Leopold Delisle, there are two which were undoubtedly torn out of Notebook A and which are of inestimable value for us. The drawings and brief remarks which cover these sheets will show us how the erroneous principles of Blasius of Parma were transformed by the genius of Leonardo da Vinci. They will allow us to observe within a mind of genius the blossoming of some of the major ideas of statics.

On one of these sheets,[6] we recognize a figure borrowed from the treatise of Blasius. Using the same figure which we reproduced above in Figure 28 he attempts to justify the law of the lever. Leonardo wrote the following lines next to this drawing:

Blasius maintains that the longer arm of this balance will descend faster than the shorter one, because its descent describes a straighter quadrant than that of the shorter one, and because the weights seek to fall by a perpendicular line, it will slow down in proportion to the curvature of the circle.

Leonardo then draws a windlass (Fig. 35) and adds:

The figure *mn* (sic) destroys this argument because the descent of its weights does not move in a circle and, nonetheless, the weight of the longer arm *m* descends.

Leonardo had in mind the treatise of Blasius of Parma and immediately recognized the inaccuracy of some of his principles, which he is going to replace with more precise concepts.

First, he is not satisfied with what Master Blasius of Parma had said following Jordanus in order to explain the decrease in weight of a load suspended from an arm of a lever, when this arm is displaced from the horizontal and approaches the vertical. Here is his drawing (Fig. 36) and the accompanying commentary.[7]

The object which is the furthest from its point of support is less sustained by it. Since it is less sustained, it retains more of its freedom and because free weight always descends, the extremity of the beam of the balance, which is more distant from its point of support and which is heavier, will necessarily descend on its own faster than any other part.

This argument is confused. It betrays the misgivings in Leonardo's mind. Then, suddenly, a flash of genius and the great painter traces the following sweeping lines:

Because the wheel (Fig. 37) has its outer edge equidistant from its center, all the weights placed on the circumference will have the same force as similar weights placed where their perpendicular line intersects the line of equality *qz*.

One of the most important ideas in all of mechanics — the notion of moment — has just been discovered by Leonardo. At the precise moment when his mind first discovered this idea which he was to make

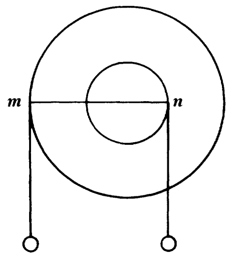


*fig. 35.*

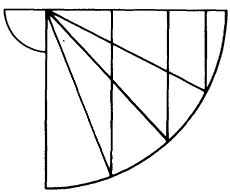

*fig. 36.*

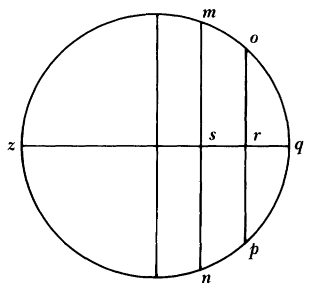


*fig. 37.*

such extensive use of, he had before him, we know it by his own testimony, the treatise of Blasius of Parma, and he was trying to refute the erroneous methods in it. But is this work the only one he read? Did he not borrow the prevously formulated concept of moment from the medieval treatise of that unknown author whom we have called the Precursor of Leonardo? How could we have any doubt after having read the comments written on the following page of the manuscript?

Leonardo is searching[8] for the condition of equilibrium of a bent lever with unequal arms. He hesitates and gropes at first:

The (balance) (Fig. 38), with unequal arms, had its extremities tending to reach the central perpendicular and if it is of uniform thickness,[9] one of them approaches more closely than the other to the extent that it is longer. And if you want to know how much point *c* draws closer to *d*, consider how many times *ab* goes into *ac*. If it goes into it three times, divide *ab* in thirds, and the third part of this line will be the distance between *c* and *d*.

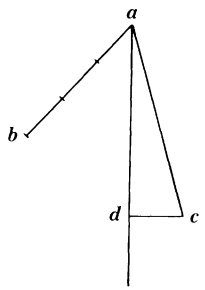


*fig. 38.*

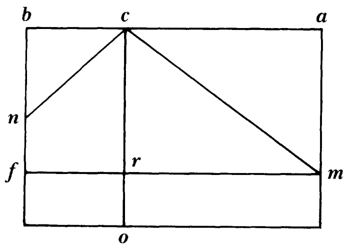


*fig. 39.*

Then the correct rule appears:

The ratio between line *cb* (Fig. 39) and line *ac* will be the same as the ratio between the weight and the length of *cn* to the weight *cm*.[10]

The problem which concerns Leonardo in this way is the very same one which his Precursor treated immediately before defining the concept of moment. There can be no double that when the notion of the moment of a force occurred to him, he had before him not only the treatise of Blasius of Parma, but also the works of his Precursor.

We stated before that the 13th century manuscript which Curtius Trojanus was to publish later seems to have constantly inspired Leonardo da Vinci. The great painter borrows from it several of the ideas on dynamics and the resistance of fluids to a moving body which constantly reappear in his notes. In addition to the effects of the treatise of Blasius of Parma, the influence of this manuscript can be frequently sensed in that it corrects the errors the former work was inclined to make.

Leonardo recognized immediately the inaccuracy of what Jordanus, and following him, Blasius had written on the stability of the balance. A rectilinear beam with equal arms, supported at its midpoint and carrying equal weights at its extremities, is not in stable equilibrium, but in indifferent equilibrium.

The weights A and B (Fig. 40), he writies,[11] will be stable in any position.

Elsewhere[12] he states even more formally that a heavy body suspended by its center of gravity will remain in indifferent equilibrium:

If the equilibrium of the balance is achieved with the pivot close to the mathematical point which is the center of gravity of the balance, then the equal arms of the balance will remain in whatever inclined position it is placed.

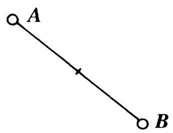


*fig. 40.*

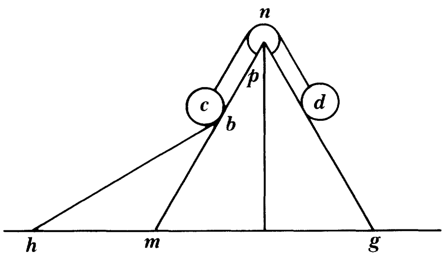


*fig. 41.*

By formulating such a conclusion, Leonardo merely restores to the center of gravity the property which Pappus had used as the definition for this quantity. Indeed, this geometer, whose work Leonardo undoubtedly knew — as we shall see in the following paragraph — called[13] the center of gravity of a body a point where, if one were to conceive of the body as being suspended from this point, it would remain in equilibrium in any position given to it by an arbitrary rotation.

This conclusion is contrary to the one formulated by Blasius of Parma and by Jordanus. Thus their reasoning must be wrong at some point. This is what Leonardo immediately shows by means of a very thorough analysis.

Jordanus had considered each of two equal weights carried by an inclined beam and he compared what would happen during the descent of one with what would happen during the descent of the other. He concluded that the more elevated weight would descend along an arc closer to the vertical than the trajectory which the less elevated weight would take. Form this he concluded that the former weight, not the latter, would turn the scale. That was poor reasoning. He should have noticed that any descent of either of the weights causes the other weight to ascend and he should have compared these two displacements made correlative to each other by the connection between the weights. Since these two displacements have an equal obliquity, it should have been obvious that neither of the two weights would have turned the scale.

In order to make the accuracy of this approach absolutely obvious and to show the inaccuracy of Jordanus' reasoning, Leonardo devised[14] an extremely ingenious apparatus.

Over a pulley *n* (Fig. 41) passes a wire carrying two identical spheres *c* and *d*. These spheres touch two inclined planes *pm* and *pg* of the same inclination: But, in addition, one of these spheres — sphere *c* — touches a second inclined plane *bh* of greater inclination. Let us apply to this apparatus the approach taken by Jordanus. The descent of weight *d* will take place along a path closer to the vertical than the descent of weight *c*. The first of these two weights must therefore be heavier than the second and it will descend while causing the other to ascend.

This result is obviously false and, on the contrary, the two weights *c* and *d* are in equilibrium.

The weights *c* and *d* are in equilibrium if they are situated on the two equally inclined planes *pm* and *pg*, but if the weight *c* is on the inclined plane *bh* then weight *c* decreases as the inclination of plane *bh* supporting it increases. Thus the weight *t* will never ascend on plane *bp* and the weight *d* will never descend on plane *pg* because the inclination of those planes is equal, just as the weights are equal which are in balance on the inclined planes.

It is clear that weight *c* will not descend further on plane *bh* and raise weight *d* on plane *gp*, because under these conditions, weight *c* would be less heavy than weight *d*.

Conclusion. Having concluded in the penultimate paragraph that equal weights placed on equally inclined planes remain in equilibrium and that things equal to each other will not outdo each other, we further conclude that the balance will not move with equal weights *e* and *h* on equally inclined plances *ab* and *cd* (Fig. 42). These inclined planes are proven equal to each other, because they are parallel. If you were to say that the arcs *ef* and *gh* were not parallel even though their chords are parallel, I would answer that it is sufficient that such arcs are similar and equal and that the centers of the weights which move through such arcs are always equidistant from the center of the balance and that the centers of equal weights are always equidistant from the center of the balance.

Leonardo successfully replaces the method devised by Jordanus for analyzing the descent of each of the weights in equilibrium with the Method of Virtual Displacements, which gave to the different weights simultaneous displacements which are compatible with the constraints of the device. Jordanus had applied this latter method, incidentally, to the straight lever and the Precursor of Leonardo had applied it to the bent lever and to the inclined plane. We saw in Chapter II what beautiful results Leonardo was able to deduce from this method.

Since the argumentation of Jordanus was unable to account for the stability of the balance, another explanation had to be found. Leonardo

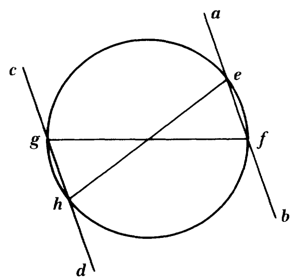


*fig. 42.*

is at first satisfied with[15] the explanation given by Aristotle. A rigid and heavy beam is surely in stable equilibrium when the axis of suspension is located above the center of gravity of the beam.

In another passage, however, Leonardo gives a more complete explanation, valid even when the beam has no weight. The principle of this explanation is as follows: when the balance is in equilibrium, the axis of rotation is above the line joining the points where the weights are suspended. If one displaces the balance from a position of equilibrium, the weights remain equal but the lever arms do not. The weight which is higher and which corresponds to the now larger arm of the lever will turn the scale and bring it back to a position of equilibrium. Leonardo expresses this as follows:[16]

*Why the balance with equal weights and equal arms stops in a position of equilibrium.* The angle formed by the junction of the central line of the arm of the balance and the central line of its appendage is never rectangular. The junction between the real arm of the balance and its real appendage is never rectangular. The lines of the gravitational forces are always at a rectangular junction.

The balance[17] with equal arms and equal weights, displaced from the position of equilibrium, will cause the product of arms and weights to be unequal, whence the necessary constraints to reestablish to lost equilibrium of the arms and weights. One can prove this . . . because the highest weight is further removed from the center of the circumvolution than the lower weight and thus having less support, it will descend easier and raise the opposite weight joined to the extremity of the shorter arm.

It would be unfair not to point out that these arguments are the development of those brief and somewhat confusing considerations which the Precursor of Leonardo had set forth in his eighth proposition. In a certain way, they reciprocate the arguments used by this author and, after him, Blasius of Parma, to study the case of the unstable equilibrium of the balance.

## 2. THE COMPOSITION OF FORCES

Leonardo was not content merely to refute and transform the inaccuracies contained in the principles of Jordanus and Blasius of Parma. He seized everything which was sound and productive in these principles, but in so doing he developed and perfected them. An example is the concept of positional gravity, or, as we would say today, of the component of the weight along its trajectory. He formulates this concept in his turn, but adds the following thought, which his predecessors, except for Aristotle, had not pointed out: positional gravity is nothing but one component of the weight. A second component, normal to the trajectory, must be added to it.

When one compares the treatise of Blasius of Parma with the following passage, it is obvious that these ideas on the decomposition of weights into two rectangular forces were suggested to Leonardo by that work of Blasius.[18]

*On the descent of a heavy body.* Any natural action happens in the shortest way. That is why the free descent of a heavy body is towards the center of the earth since that is the shortest distance between the moving body and the lowest part of the earth.

The uniform heavy body descending obliquely divides its weight into two different components. One can prove it. Let *ab* be[19] a mobile body on the oblique line *abc*. I maintain that the weight of the heavy body *ab* divides its weight into two components, that is to say, along line *bc* and along line *nm*. Why and by how much greater the weight is for one component over the other and what the obliquity is which divides the two weights into equal parts will be started in the book "On Weights".

This proposition, obviously derived from the principles of Jordanus, is frequently put to use by Leonardo when he studies the flight of birds. Such study more than any other seems to have led his genius to meditate upon the principles of mechanics. But the great architect in him could immediately sense another, not less important application of this proposition to the resistance of materials, because the proposition is, indeed, related to the following problem: If a heavy body is sustained by two supports forming a right angle, how does the force[20] distribute itself among the two supports? This question[21] occurred to Leonardo immediately after he had written the passage quoted below:

The heavy body which does not tend towards the center of the earth always has two or more components of force. One can prove it: Let *abcd* (Fig. 43) be a heavy body which has no component along the central line *be*. Thus it exerts weight through the two supports *ba* and *bd*.

Every heavy body exerts weight in the direction of the component where it is inclined to descend. One can prove it with the demonstration of *ba* which weighs on *d* and *bd* which weighs on *a*, because in these positions *ba* and *bd* are inclined to descend.

Conclusion. The heavy body exerting weight at two locations does not have its weights in a single place.[22]

Leonardo constantly turns his attention to this question. It prompts him to devise several solutions to the problem of the composition of

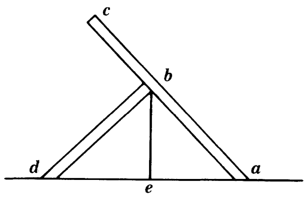


*fig. 43.*

forces. In Chapter II we saw the kind of solution he seems to have been satisfied with, erroneous as it was.

But if this erroneous solution seems to have received Leonardo's final approval, it nonetheless was not the only solution which occurred to him.

At the end of Chapter II, we quoted a curious solution, unfortunately only briefly sketched out, to the problem of the inclined plane and we observed that this solution seemed to imply accurate notions of the law of the decomposition of forces. A large part of what Leonardo wrote would remain incomprehensible if the author did not recognize the accuracy of the following proposition: The moment of the resultant of two concurrent forces is equal to the sum of the moments of the components.

We added the following thought to the above remark: had Leonardo finally come to an understanding of this important theorem? In his published manuscripts, we have not seen any further evidence of it other than what we have already cited.

While reviewing Leonardo's notes after Chapter II had gone to press, our attention was drawn to several sheets of manuscript E of the Bibliothèque de l'Institut.[23] An examination of these sheets fully confirms our hypothesis, to wit, Leonardo knew and used the following theorem.

If one considers two concurrent forces and their resultant, the moment of the resultant with respect to a point taken on one of the two components is equal to the moment of the other component with respect to the same point.

In Leonardo's reasoning, the two components are the tensions in the two ropes; the resultant of the tensions is equal and directly opposed to the weight supported by the two ropes.

On many occasions, the great artist applies this theorem which we have formulated above, to a weight N suspended at the midpoint B of a rope which has its extremities A and C situated on the same horizontal line (Fig. 44). From point A, he drops a perpendicular AF to rope CB or to its extension and another perpendicular AD to the vertical through B. He declares that the tension of the rope CB and the weight N would keep in equilibrium a rigid body formed by two potential lever arms AF and AD, if this body were simply capable of turning around point A. Since on the other hand, as we have seen in Chapter II, Leonardo knows how to express the condition of equilibrium of a circumvolubile, which amounts to the equality of the moments of weight and tension with respect to point A, he immediately obtains the theorem quoted above.

Here are several passages[24] which seem unquestionably clear.

First: A is the pole of the angular balance AD and AF; their appendages are DN and FC.

Second: the more the angle of the rope increases which supports the weight N (Fig. 45) at its midpoint, the more the potential lever decreases and the more the counterlever supporting the weight increases.

And after Leonardo has drawn the figure in such a way that AB is four times the length of AC, he marks with 1 the weight N and puts the number 4 on the rope FD in order to indicate its tension.

He continues in the following terms, which clearly shows us how his genius related the study of the equilibrium of concurrent forces to the law of equilibrium of the bent lever so familiar to him:

This figure (Fig. 46) represents the potential arms ACB of the preceding figure; but since what is real has weight and not what is potential, I add the arm MN as a counterweight to arm O.

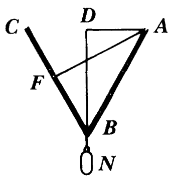


*fig. 44.*

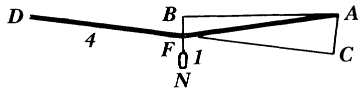


*fig. 45.*

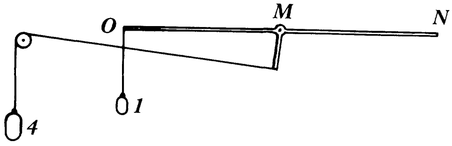


*fig. 46.*

Later, when Roberval gives another demonstration of the same theorem, he too will deduce it from the condition of equilibrium of a bent lever. But the artifice he uses will be much more complex and much less directly accessible than the device conceived of by Leonardo da Vinci in this passage.

Returning to Figure 45, Leonardo adds:

AFD provides the real support for weight N, and the lines AC and AB indicate the potential lever and counterlever of weight N respectively; the semi-real appendages CD and BF are such that one is joined to the potential lever and the other to the potential counterlever AB.

The counterlever AB can never change no matter how much the angle formed by the real rope AFD can change. And the lever AC can never have a permanent length caused by the change of angle AFD, but it will be decreased all the more the angle AFD increases.

If the two points A and D as well as the weight N remain fixed, the tension of the rope DF will be inversely proportional to the potential lever AC:

Wherever a potential lever is in existence,[25] a force will always be in existence. The force will be all the greater the smaller the magnitude of the potential lever.

The rope DFA (Fig. 45) can never be straight, because with the potential lever AC equal to zero, the tension in the rope DF would be infinite:

No rope or force whatsoever, placed in a rectilinear situation with the opposite extremities, can ever[26] come to its original position, once any weight has been placed at its midpoint.

The potential lever[27] is never exhausted by any force.

In no case is the tension of each of the ropes half of the supported weight, as common sense would suppose. In order to be so, the two ropes would have to be parallel, and that cannot be:

If the lever AD (Fig. 47) were double[28] its counterlever AB, then the chord DE would carry half of the weight F; that cannot happen if the lever AD is not in a position of equality (a horizontal position). This can not be unless the appendages which are concurrent at the suspension point of weight F, are equidistant from each other.

So far we have seen Leonardo da Vinci apply the stated theorem to a particular case. The verticals drawn through the supported weight bisected the angle of the two ropes supporting this weight. But he also knew and used this proposition in the general case as evidenced by the passage which we will now summarize:[29]

Leonardo draws two figures and in each one there are two ropes forming a given angle and supporting a weight with a vertical which does not bisect this angle at all. In one of these figures (Fig. 48), the lever DR of the rope FE and the counterlever DS of the weight Q are equal to each other. Therefore, Leonardo assigns the same number 3 to the weight Q and to the tension of rope FE.

In the other figure (Fig. 49), the lever AB of the rope FG is three times the length of the counterlever BC of the weight E. Leonardo also assigns the number 3 to the weight E, and 1 to the rope FG in order to indicate the tension. This second figure is accompanied by the following commentary:

If a rope is deflected by a weight suspended at its midpoint, it is all the easier to draw it taut the less oblique the deflected segments. Therefore, rope BGF is easier to straighten than the preceding rope DEF, as evidenced by the magnitudes of the levers and counterlevers of both segments. Indeed, the lever AB on the pole B is threefold its counterlever BC. Therefore, the semi-real appendage AF, with a force of one unit, counters a force of three units in the opposing semi-real appendage CE. And in the preceding case, 3 units of force counteract 3 units of resistance.

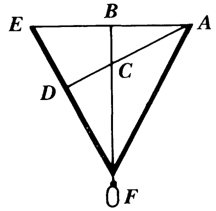


*fig. 47.*

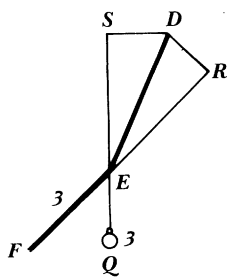


*fig. 48.*

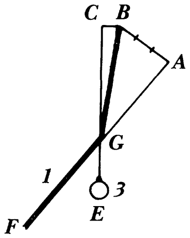


*fig. 49.*

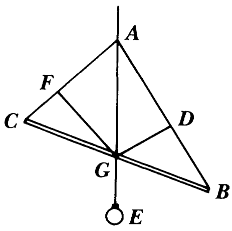


*fig. 50.*

These various quotes are convincing evidence that Leonardo had a very accurate knowledge of the previously stated theorem. Implicit in this theorem are all the rules of the composition of concurrent forces.

Specifically, the following corollary can be deduced from this theorem by means of a very simple demonstration. With respect to a point taken along the direction of the resultant, the two components have moments with opposite signs but with equal absolute values. Did Leonardo recognize this corollary? Only an affirmative answer to this question seems to render meaningful a fragment[30] which contains a very explicit drawing (Fig. 50) and a commentary which is, unfortunately, very obscure. Here is the commentary:

If two ropes with different and contrary inclinations descend from the same point and attach at the opposite extremities of a beam positioned in an arbitrary inclination, the center of gravity of that beam is always located on the intracentral line and at the intersection of the supreme heights from the two ropes supporting the beam.

The "intracentral" line of which Leonardo speaks is the vertical through the point of suspension A. As far as the "supreme heights" are concerned, they can only be the lines GF and GD;[31] otherwise, why were they drawn, unless they are the potential levers of the two ropes AB and AC?

The various fragments we just quoted and commented upon state very accurate ideas on the composition of concurrent forces. Why then did Leonardo turn away from these ideas at the precise moment he had mastered them and immediately embrace an entirely different and a completely erroneous rule?

On the very same page[32] as the fragment we just analyzed, Leonardo writes:

For the two ropes of different inclinations joining the point of suspension of the same heavy body, the ratio between the portion of the weight supported by them is the same as the ratio between their inclination. Let us prove it: Let us assume two ropes of different inclinations AD and CD (Fig. 51) which are such that one of them is double the other, as is the case with the arms of the balance. BC is double the arm BA. The inclinations of the appendages AD and CD descend from the extremities of these arms. Thus rope CD bears half of the weight that rope AD bears.

The number 3, which Leonardo puts under the weight E, indicates that he considers it equal to the sum total of the tensions in the two ropes. Thus he accepts an erroneous conclusion which he had rejected a few pages earlier. One can read on the preceding page:[33]

The heavy body suspended from the angle of the rope divides the weight for the ropes in the same ratio as the ratio of the angles between the rope and the central line of the weight. One can prove it: Let there be an angle BAC in the given rope (Fig. 52) from which is suspended the heavy body G by rope AG. Let the angle be cut in a position of equality (the horizontal direction) by the line FB, and then draw the perpendicular DA to meet at vertex A and to continue in a straight line with rope AG. And the ratio between length DF and length DB will be the same as between the weight borne by rope BA and by rope FA.

In the following pages,[34] Leonardo constantly applies this incorrect rule which can also be found in many other passages[35] in his notes.

In the mind of Leonardo, ideas burst forth tumultuously. But occasionally the great painter lacked the power to grasp and hold for good the truth which the impetuous torrent of his thoughts intermingled pellmell with error. Thus it happened quite often that the truth which had revealed itself to him for an instant, emerging to the surface from an undercurrent of incomplete and erroneous views, again submerged to wait for the future to be brought to light again. This happened with the concept of positional gravity conceived by Jordanus. The great artist da

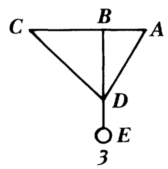


*fig. 51.*

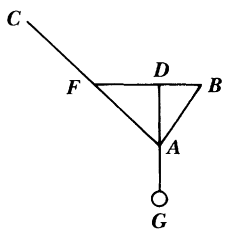


*fig. 52.*

Vinci had succeeded in extracting from it the idea of the resolution of a force into its components, but this idea, which he was able to perceive for a moment in its entirely, becomes obfuscated again until the great painter could only see in it an erroneous law for the composition of forces. The study of the problem of the inclined plane will offer us another opportunity to ascertain the hesitations and fluctuations of Leonardo's thought.

### 3. THE PROBLEM OF THE INCLINED PLANE

As early as the 13th century, the great and anonymous mechanician whom we called the Precursor of Leonardo da Vinci had resolved the problem of the inclined plane with such an elegant and cogent method that everyone should have immediately accepted it. But it is not enough for a truth to be discovered nor is it enough for it to be justified by perfectly clear and accurate argument to rank among the body of doctrines which constitute universally accepted science. The human mind must first become accustomed to a new idea before it grasps it, just as it is not enough for a light to shine in the darkness for us to see it, but our eyes must become accustomed to the glare of the light.

Sometimes the mind is not prepared to accept a truth even with a perfectly convincing proof and it can take years or even centuries before such acceptance occurs. Afterwards, the historian of science is amazed that mankind took so long to perceive the light. The historian is astonished at the prolonged blindness and forgets that his own vision has been strengthened by prolonged familiarity.

There has probably been no other case where a light capable of blinding the mind and causing it to misunderstand an all too brilliant truth has been so apparent as in the case of the inclined plane.

As we have seen, the solution to the problem of the inclined plane was complete as early as the 13th century. But at the beginning of the 15th century, Blasius of Parma has before him the treatise containing the solution. Does he adopt it? Absolutely not! Instead, he counters it with a false argument, with an obviously untenable conception of equilbrium between two weights, and ends by rejecting it as a paradox. Furthermore, the geometers at the end of the 15th century do not seem to have accepted as true the solution of the problem of the inclined plane.

Regiomontanus, an admirer of the treatise *De numeris datis*, had undoubtedly read the *Elementa super demonstrationem ponderis* and we can assume that in reading this his attention had been drawn to the problem of the inclined plane. However, it is equally true that in a letter of July 4, 1471 addressed to Christian Roeder, a Professor at Erfurt, Regiomontanus poses the following prlblem:[36]

Two weights (Fig. 53) are attached and are positionally equivalent (*secundum situm equipollentia*). If they were freed from their common bond, one of them would descend vertically and the other obliquely. The oblique path of the second weight forms a twenty degree angle with the horizon while the right angle is ninety degrees. I am asking what the ratio between these two weights is. I call equivalent weights those which prevent each other from descending. Let us then consider *bc* a horizontal line, *ab* a straight line directed toward the center of the earth. Let *ac* and *bc* form a twenty degree angle, and the lighter weight *d* tend to descend along *ab* and the heavier weight *e* tend to descend along *ac*, if one were to do away with the bond connecting them.

The use of the words *secundum situm equipollentia* leads us to conjecture that this problem, in all probability, was suggested by the principles of Jordanus.

Maximilian Curtze has pointed out that the problems posed in the letters of Regiomontanus are generally problems to which he actually had or believed he had the solution. We can thus assume that this geometer believed he had solved the problem of the inclined plane. In any case, it is quite certain that he was convinced that his predecessors had not solved the problem. But then again, he might not have known of the remarkable treatise composed by the Precursor of Leonardo da Vinci in the 13th century.

Moreover, geometers must have hesitated to accept as true the solution given by this author for the apparent weight of a heavy body placed on an inclined plane, when they knew of the totally different solution given earlier by Pappus.

Pappus seems to have been the only geometer of Antiquity to have considered the problem of the inclined plane. This mathematician lived

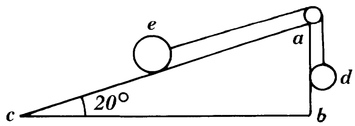


*fig. 53.*

in Alexandria during the 4th century of our era where he wrote his *Collections*.[37] Although this work has not come down to us in its entirety, it is, nevertheless, extremely important for the history of Hellenic science.

The eighth book of these collections carries the following title: *It Contains Various and Delightful Mechanical Problems.* Among the "various and delightful mechanical problems," is the problem of the inclined plane.[38]

From the beginning, the solution of Pappus is in absolute contradiction with modern statics. Indeed, Pappus assumes that in order to move a given weight W on a horizontal plane, it is necessary to draw it parallel to this plane by a given force F. Common sense agrees with him. Only at a higher level of abstraction and analysis did it become obvious that the resistance felt by a weight moving on a horizontal plane was due to friction. Only a higher level of abstraction and analysis could have led geometers to the following principle, in total opposition to the one proposed by Pappus: any force, however small, is sufficient to put into motion any weight on a perfectly smooth, horizontal plane.

In order to pull the same weight W on an inclined plane, one needs a force P. Pappus attempts to determine the ratio between force P and force F, and this is the approach he thinks will achieve it: On the inclined plane BD (Fig. 54) there is a heavy sphere of weight W which touches the plane at C. Pappus first asks how this sphere could be kept in equilibrium. He treats this problem of statics like the problem of the balance. The balance which he considers, would have its point of support at C and it would carry, suspended at the center G of the sphere, the weight of this sphere W. Weight H, meant to equilibrate it,

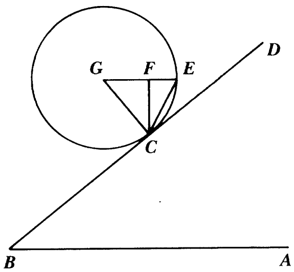


*fig. 54.*

would be suspended at the extremity E of the horizontal radius GE. Pappus contends (and by so doing, seems to have understood the law of equilibrium for the bent lever)[39] that the value of weight H will be (GF/EF)W. Since the weight W is being pulled on a horizontal plane by the force F, a force J = (GF/EF)F would be needed to pull weight W on the same plane. Thus Pappus contends that the force P, capable of lifting weight W on the inclined plane, will be the sum total of the forces F and J, so that we have P/F = GE/EF.

Some geometers at the end of the 16th century, like Guido Ubaldo, for example, are still satisfied with this very inaccurate solution. It is to counter his solution that Galileo must make frequent use of all the resources of his mind.

Leonardo da Vinci certainly knew the two solutions to the problem of the inclined plane which had been proposed before his time. Indeed, one of the solutions is in the *De ponderibus* written by the 13th century geometer to whom Leonardo owes so much that we have called him his Precursor. The other solution, proposed by Pappus, obviously inspired some of Leonardo's thinking in his attempt to solve this problem.

Leonardo considered the determination of the apparent weight of a heavy body on an inclined plane as far from being resolved, and he never ceased to be occupied with its solution. We discussed in Chapter II the types of answers he formulated, some correct, some erroneous.

There are instances in which the influence of the treatise by his Precursor is quite obvious. Here is, for example, a passage[40] where we recognize the proposition which immediately precedes the solution to the problem of the inclined plane in this treatise:

If weights A and B (Fig. 55) do not tend toward the center of the earth because they are separated, their junction nevertheless tends towards the center of the earth, as the central line MN shows us. This line intersects at the proportion of the weights two and

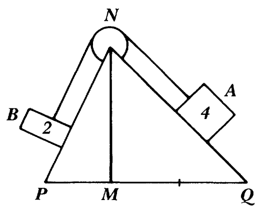


*fig. 55.*

four. But the positions of the weights are not proportionally spaced, because given the same inclinations, one weight could remain elevated and the other lower without their positions, different in elevation, changing the double ratio between the weights.

There can be no doubt that Leonardo has in mind here his Precursor's treatise about this invariability of the apparent weight at different points along an inclined plane. Two pages further on, we find the passage[41] in which Leonardo so cleverly criticizes the demonstration which had led Jordanus to an erroneous proposition on the stability of the balance. In this passage, Leonardo counters the above proposition with the Method of Virtual Displacements, precisely as his Precursor had applied it to the problem of the inclined plane. It seems that circumstances were pressing Leonardo to accept the solution proposed by the 13th century geometer. However, he does not do so. It is enough to look at Figure 55 to realize that Leonardo considers the two weights A and B as proportional to PM and QM respectively, in accordance with an erroneous rule often adapted by him.

The solution proposed by the Precursor of Leonardo to the problem of the inclined plane, was based on a postulate which Jordanus had already implicitly introduced in the demonstration of the law of the lever. This postulate can be formulated in the following way: The motor force used to lift a weight is equal to the product of the weight times the distance it has been lifted. When these two elements change in an inverse ratio, the motor force does not change. Leonardo, more than anyone else, understood the accuracy and the consequence of this postulate. No one else was able to formulate it more clearly or to pursue more persistently its application to different machines. And yet Leonardo does not seem to have understood to what degree this principle was suited for determining the apparent weight of a heavy body placed on an inclined plane. He never made use of it when considering this problem. When the great painter, obviously guided by his knowledge of the treatise of his Precursor, undertakes to solve this problem for the first time, he accepts the accuracy of the solution devised by the great 13th century mechanician, but he foregoes the latter's demonstration in favor of an argument which he seems to have derived from Pappus.[42]

We can find signs of these first attempts in Notebook A, kept in the Bibliothèque de l'Institut. This book, folio 5, recto, presents arguments very similar to those which are used to demonstrate the last proposition of the original treatise of Jordanus. It is in this notebook, from which Libri had torn out pages, that we find Leonardo meditating on the equilibrium of the lever, criticizing the principles of Blasius and writing down his first thoughts on the concept of moment, which were obviously taken from the treatise of his Precursor. His meditations on the inclined plane are thus contemporary to those which we just quoted.

In order to determine the velocity at which a body descends on an inclined plane or to determine the apparent weight of the body placed on the plane (these are proportional for anyone relying on Peripatetic dynamics), Leonardo applies, as we have mentioned, an argument in which there are distant, yet distinct, echoes of the argumentation of Pappus.

In Chapter II, we quoted a text borrowed from manuscript A where this argument is propounded. The same manuscript[43] has preserved for us another version which we shall quote here:

*On movement and weight,* Any heavy body tends to fall to the center and the most oblique trajectory offers the least resistance. If the weight is at A (Fig. 56), its true and direct trajectory would be AB, and whatever part of the wheel touches the ground, locates its pole: and when the larger part of the sphere is to the left of the pole, it falls. Since SX is the pole, it is evident that ST will weigh more than SR; thus, the portion ST will fall, and overcome SR and lift it, since ST moves along the incline vigorously. And if the pole were at N, the more NC enters into BC, the faster the wheel would descend on the incline, compared to the pole being at X.

However strange this reasoning may be, nevertheless, it led Leonardo, as we have seen in Chapter II, to an accurate evaluation of the apparent weight of a heavy body on the surface of an inclined plane.

We can find this evaluation several times in his notes, provided we acknowledge with him a proportionality between this apparent weight

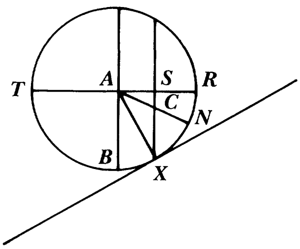


*fig. 56.*

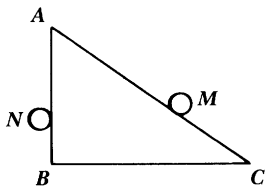


*fig. 57.*

and the velocity of fall. We can read, for instance, passages such as the following:[44]

Weight N will fall faster than weight M (Fig. 57), the more line AB enters into line AC.

We can also read the following statement:[45]

The body will descend more slowly on line BC (Fig. 58) than on line BD, the longer line BC is to line BD, everything else of equal weight and shape (. . . furthermore, it will descend more slowly, the closer the point of contact is to the center of gravity which is moving.)

There is a curious remark[46] which implies an accurate knowledge of the law of the inclined plane:

Weight AB and weight CD are in equilibrium on the balance (Fig. 59).

One does not have to belabor this observation to make it yield the ingenious demonstration which Simon Stevin will provide.

We have seen in Chapter II how Leonardo da Vinci, using the rule of the inclined plane, had sketched a demonstration which implied an

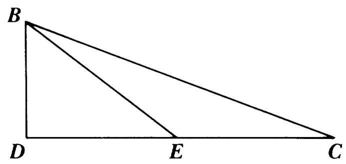


*fig. 58.*

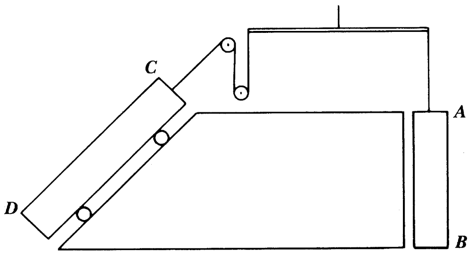


*fig. 59.*

accurate knowledge of the law of the composition of forces. It seems, however, that neither this demonstration, nor the argumentation borrowed from Pappus, sufficed to persuade him. Quite often he applies erroneous approaches to the problem of the inclined plane as well as to the problem of the law of the composition of forces.

There is no essential notion in Leonardo da Vinci's work on mechanics which does not derive from the work of the medieval geometers especially from the treatise of the great mechanician whom we have called the Precursor of Leonardo. The notion of the moment of a force, the distinction between states of stable and unstable equilibrium of a balance, the determination of the component of a force in a given direction, the evaluation of the motor force as the product of the weight lifted and the height to which it has been lifted, the theory of the inclined plane, all of these ideas were discussed in one fashion or another as early as the 13th century. Some of these ideas were only tiny seeds or primitive sketches which developed into concepts of prodigious breadth in the notes of Leonardo. On the other hand, Leonardo did not fully recognize the value of other ideas which had already achieved perfection as early as the Middle Ages, such as the theory of the inclined plane.

Thanks to the meditations of Leonardo da Vinci and the works produced in the School of Jordanus, we can say that there was hardly an essential idea in statics which had not been clearly understood and formulated by the beginning of the 16th century. But much more is still needed before this science can be definitively established. The truths which must coalesce within a coherent doctrine in order to constitute a science are still sparse and scattered and still contain many errors. The mind's eye is still too accustomed to false proofs to be able to accept the clarity of those accurate postulates which are necessary to these truths.

The entire 16th century and the beginning of the 17th century hardly provide enough time to sort out the true theorems from the inaccurate propositions, to dissipate the misunderstandings born of a language as yet imprecise, to eliminate the false proofs, to show the concordance between apparently contradictory truths and to rediscover, in short, what had already been invented in the 13th century.

CHAPTER IX

# THE SCHOOL OF JORDANUS IN THE 16TH CENTURY NICOLO TARTAGLIA

## 1. NICOLO TARTAGLIA OR TARTALEA

In 1546, one of the great geometers of the 16th century, Nicolo Tartaglia or Tartalea of Brescia, published the most important of his works under the title of *Quesiti et Inventioni diverse.*[1] This curious work consists of nine books and is written in the form of a dialogue in which Tartaglia converses with different personalities of his time. We can read learned dissertations delivered by Tartaglia to such renowned noblemen as François Marie, the Duke of Urbino, Richard Ventuorth, subject of his Majesty the King of England, Gabriel Tadino di Martinengo, Knight of Rhodes and Prior of Barletta, and Don Diego Hurtado di Mendozza, Ambassador of the Empire to the Republic of Venice. We see Tartaglia conversing with persons of all conditions, such as Brother Beretino, Master Zuanne di Tonini da Coi, who has a school at Brescia, the excellent physician and philosopher, Marc Antonio Morosini, and mathematicians such as Antonio Maria Fior or his Excellency, Hieronimo Cardano. Sometimes, Tartaglia's interlocutors are persons of lesser social stature whose names have not come down to us, such as an artilleryman, a mortar man or a fusilier.

These conversations touch upon the various branches of pure and, especially, applied mathematics. Algebra and, in particular, the solution of equations of the third degree are the main topics of Book Nine. The other books deal with statics, ballistics, the manufacture and properties of explosives, the art of cartography by use of the compass, the principles needed to lay out fortifications, and tactics. Since war was ubiquitous in those times, the geometer almost always had a second function as a military engineer.

At the end of Book Six of the *Quesiti et Inventioni*, Tartaglia, trying to satisfy the curiosity of the Prior of Barletta, tells us what little we know about his life.

Tartaglia was born in Brescia at the beginning of the 16th century, but the exact date is unknown. All he knows of his father is his first name, Michèle and the nickname Micheletto Cavallero; Micheletto because of his small stature and Cavallero from his occupation.

To earn a living, Micheletto carries mail on horseback and delivers letters to Bergamo, to Cremona and to Verona. When he dies at a young age, he leaves two sons and a daughter to his widow. The girl is the youngest of the children and Nicolo, destined to become the great geometer, is only six years old and younger than his brother.

The family is destitute even before war breaks out with all its horrors. The French take Brescia and sack the town. In order to escape death, many inhabitants of Brescia flee from their insecure houses and seek an inviolable asylum in the cathedral. The asylum is violated, and raging and blood-thirsty hordes invade the church and pitilessly massacre men, women and children. Nicolo is struck three times by a soldier's sabre and has his head split open in three places so that his brain is exposed. Two blows split his palate and both sides of his jaw.

The unfortunate child fell into his mother's arms. These horrible wounds prevented him from talking and from eating any solid food. He could barely swallow liquid. Since Nicolo's mother did not know the art of making ointments and was too poor to pay for a physician, "she was reduced to caring for me not with medications, but by tenderly cleansing these awful wounds, as wounded dogs heal themselves by simply licking and cleansing their wounds." Thanks to this maternal care, Nicolo was able to recover, but due to his wound he stuttered severely throughout his life. Hence, the nickname Tartaglia, the Stammerer, which was to take the place of his unknown patronymic.

Nicolo Tartaglia was about twelve years old when the sack of Brescia occurred. Before his father's death, that is to say, when he was about five or six years old, he had gone to school to learn to read, but after that he did not receive any formal schooling. At the age of fifteen he wanted to learn to write and went to see a certain Francesco. Master Francesco agreed to teach him the art of writing for payment. The first payment was to be made in advance while a second payment was due after Nicolo had reached the letter *k*, with a third payment to be made once Nicolo had come to the end of the alphabet. But when the pupil had learned to write the letter *k*, he ran out of money and was forced to leave school without having learned to write the remaining letters. Tartaglia found a means to procure a complete alphabet written by Master Francesco and to teach himself the remaining letters.

After the lessons with Master Francesco, Tartaglia had no further instruction. He lived "with only the companionship of Lady Poverty, surnamed Industry." This was the route of one of the greatest geometers of the 16th century, indeed, of all times.

The date of Tartaglia's death is unknown to us, as is the date of his birth. In 1556, in Venice, Tartaglia began the publication of his general *Trattato di numeri et misure*. The third part, published in 1560, begins with a dedication by the printer and is dated January 1st, 1560. In the dedication Tartaglia is referred to as if he were already dead.

Book One of the *Quesiti et Inventioni diverse* is devoted to the study of the motion of artillery projectiles. It would be necessary to give a detailed analysis of this work, which was to have a great influence on the development of mechanics in the 16th century, if we were intending to write here a history of dynamics. In particular, we would be forced to identify in this work the different and obvious influences of Leonardo da Vinci on the geometer of Brescia. However, this first book is not without interest for the history of statics because we see for the first time how Tartaglia applies principles developed by Jordanus.

Leonardo da Vinci had written:[2]

> Any heavy body moving to a position of equality only weighs along the line of its motion. This is shown by the initial path described by a mortar shell, a motion which is towards the position of equality.

This idea immediately followed several passages inspired by the principles of Jordanus, known to Leonardo through Master Blasius of Parma and through the treatise of his Precursor.

Tartaglia developed this idea during conversations he had with the Duke of Urbino in Venice in 1538, which inspired him to begin the *Quesiti et Inventioni diverse*. An artillery piece, aimed horizontally, propels a projectile which initially follows a horizontal line. As long as it keeps to this trajectory, its natural weight, directed along a vertical line, is zero. As the projectile begins to fall, its natural weight begins to assert itself and increases as its trajectory approaches the vertical.

In support of this theory, derived from the thought of Leonardo da Vinci, acknowledged by Cardan and professed by Galileo in his youth, Tartaglia invokes the following principle:[3]

> It is necessary to note that a heavy body is considered heavier at the point where its descent is less oblique or less curved, assuming that the body is in the same situation or at the same point. And the descent of a heavy body is considered all the more oblique,

the less it projects on the vertical for the same quantity. Stated differently: it takes less of a quantity of the vertical or of a parallel to the vertical while it describes the same portion of the circumference on which it revolves.

Tartaglia immediately deduces the following application from this principle:

Any weight suspended from a beam of a balance displaced from its position of equilibrium becomes lighter, and lighter to the degree that the distance between the beam and the horizontal position increases.

The appeal to the principles of Jordanus is quite evident here, but it is only parenthetical. We shall see Tartaglia give dogmatic expression to these principles.

Book Seven of the *Quesiti et Inventioni diverse* bears this subtitle: *On the Principles of the Mechanical Problems of Aristotle.* The conversations between Tartaglia and the Ambassador Don Diego Hurtado di Mendozza aim, above all, at proving as insufficient the Peripatetic theory on the balance on the grounds that it was formulated without taking into account the true principles of the Science of Weights.

Aristotle is certainly right in asserting that the same virtue or force applied to the extremity of an arm of a lever moves the arm faster, the longer it is. However, he incorrectly applies this correct principle when he claims that larger balances are more sensitive than smaller ones. His error stems from his inability to distinguish clearly the properties of mathematical, abstract balances, where arms have no thickness nor weight, from the properties of physical balances, made up of material and heavy parts. Thus, contrary to the Philosopher's reasoning, the smaller the balance, the more sensitive.

Tartaglia explains to the ambassador the reasoning by which Aristotle describes the stability of a balance with its point of suspension above the beam. However, his desire to criticize the Stagirite does not prevent him from borrowing his statement and demonstration for the following erroneous proposition: when the point of suspension is below the beam, the equilibrium of the balance is indifferent. To this incorrect proposition he adds another and even takes Aristotle to task for having omitted this erroneous proposition, which he pompously calls:

a beautiful demonstration, even more hidden from our intellect than the two others.

At issue is the following claim: If the center of gravity is located precisely on the upper surface of the beam of the balance, the equilibrium of the balance is stable.

I assure your Lordship, Tartaglia adds, that before attempting to demonstrate the cause of such an effect, I must define and demonstrate several terms and principles of the Science of Weights.

Thus in Book Eight the main topic of conversation between Tartaglia and Don Diego Hurtado di Mendozza is the Science of Weights.

When we hear the Ambassador declare at the start of the conversation that the Science of Weights is not an independent science, but rather a "subaltern" doctrine, and ask Tartaglia from which disciplines it derives, and when we hear Tartaglia immediately answer that it derives, in part, from geometry and in part, from Natural Philosophy, we immediately think of the preamble to the *Peripatetic Commentary* on the treatise of Jordanus. It was through the efforts of Peter Apian that this preamble had gone to press only a few years earlier. It is immediately clear that the description of the Science of Weights which Tartaglia presents to the ambassador of the Empire can be traced to the School of Jordanus.

However, Tartaglia does not blindly follow one particular author of that School, but chooses among the different treatises. We have seen, for example, that he borrows his opening statements from the *Peripatetic Commentary* printed and published by Peter Apian, but that is all he takes from this obscure treatise. Among the given postulates, we find the following,[4] which finds no application in Book Eight of the *Quesiti*:

No body is heavy in its own element; like water within water, wine within wine, oil within oil, air within air, none have gravity.

We recognize here a proposition borrowed from the *Treatise on Weights*, falsely attributed to Archimedes, and reproduced in the works of Blasius of Parma. There are two main sources for most of the content of Book Eight of the *Quesiti*. These are the fragment, *De ponderoso et levi* attributed to Euclid, and the first book of the treatise by the Precursor of Leonardo da Vinci.

The modifications made by Tartaglia in his editions of these various works are minimal. They consist mainly of laudatory comments made by the ambassador:

That is a very elegant proposition! This is a problem to my liking. I follow you very well, you may continue.

The first book of the treatise of the Precursor of Leonardo da Vinci is not reproduced entirely by Tartaglia. He does not mention at all, for example, the elegant proposition concerning the bent lever. On the other hand, he expounds[5] in great detail and with great care the theory of the inclined plane contained in the same work.

When Don Diego Hurtado Mendozza hears this exposition, he quite justly exclaims:

Now there is a grand speculation to my liking.

Indeed, it would be impossible to ask for a clearer and simpler solution to the problem of the inclined plane. Yet Tartaglia shamelessly pretends that such praise refers to him and carefully avoids mentioning the work which contains this theorem. Not once did Tartaglia mention Jordanus, to whose School he is intellectually indebted. This gross injustice did not go unnoticed for long.

A dispute over the proper method for solving the equation of the third degree as well as a question about the priority of invention of these methods brought Cardan and Tartaglia into conflict. This quarrel provoked a fierce mathematical duel between Tartaglia and a supporter of Cardan, Ludovico Ferrari. This student of the Milanese physician declared: "che sono creato suo."[6] The controversey began on February 10th, 1547 with a challenge[7] issued by Ferrari to Tartaglia. It was followed by five other challenges, issued on April 1st, June 1st, August 10th, 1547, during the month of October 1547 and on July 14th, 1548. To each of these provocations. Tartaglia countered with his own challenges dated February 19th, April 21st, July 9th, August 30th, 1547, June 16th, and July 24th, 1548.

In his very first challenge letter, Ferrari vehemently attacks Tartaglia's scientific credibility:

Besides having committed a thousand mistakes in the first books of your work, Ferrari writes,[8] you have set forth in Book Eight the propositions of Jordanus as if they were yours and without mentioning their true author; this is flagrant theft. You give demonstrations of your own making, which are inconclusive most of the time. And to your great shame, you put words in the mouth of his illustrious Lordship Don Diego di Mendozza which he would not say for all the gold in the world. I am sure of this because I know to some extent the breadth of his knowledge. This is proof of your presumptuousness as well as of your ignorance.

Tartaglia did not respond to this barb in his counterchallenge. Ferrari attacked again:

Have you already forgotten, he shouts in his second letter,[9] your thefts and errors which I reminded you of in my letter. Outraged by your injustice I told you that you stole the propositions from Jordanus, and that you took credit for them without mentioning the real author at all, that you were fool enough to claim that your futile arguments were conclusive. And finally, to culminate your disgrace, you put on stage and take for your interlocutor, a very dignified man, the ambassador of the Empire, and have him say that your demonstrations are true and convincing. Through a strange lethargy and to the astonishment of any intelligent mind you often assume what must be proven. Finally, you falsely and unjustly reproach the divine Aristotle.

This time, Nicolo Tartaglia countered[10] and asserted that by Ferrari's own admission the demonstrations he used to prove the propositions of Jordanus were Tartaglia's own.

A demonstration, as you should well know, is a much more important matter, it requires much more knowledge. it is more scientific and more difficult than a simple proposition. Any mathematical proposition without a demonstration is considered by mathematicians to have no value. To compose a proposition is an easy matter and any fool can formulate a proposition without being able to demonstrate it . . .

. . . Thus, it is reasonable for me to consider the Eighth Book on weights as my own for three sound reasons.

First, the order of my presentation is completely different. It is simpler, more intelligible and briefer than the order followed by Jordanus.

Secondly, I expanded considerably on the definitions, the postulates and the propositions and, if death does not interrupt my project, I plan on developing them even further in the future.

Thirdly, as you yourself admit, the demonstration came from me and not from Jordanus. You claim that the modicum I took from Jordanus obliges me to credit him. In response, I can only answer that if I had quoted him, I would also have been forced to point out the great obscurity in his propositions as well as in his demonstrations, something perfectly obvious to any intelligent man. It seemed to me not to be the thing to do.

Tartaglia's riposte to Ferrari does not incite a high opinion of his credibility. It might have duped someone who had formed his judgments of Jordanus on the basis of the cloudy demonstrations of his Peripatetic commentator, as published by Peter Apian. It appears wretched to anyone familiar with the original texts, which are so clear and so accurate that Tartaglia's arguments are often nothing but simple paraphrases in comparison. The demonstrations which Tartaglia claims as his own with such tenacity owe everything to the *Liber de ponderoso* *et levi* attributed to Euclid, and to the treatise of the Precursor of Leonardo da Vinci. Tartaglia made belated amends to his anonymous predecessor.

In his answer to Ferrari he declared that he intended to expand on the demonstrations of Jordanus, unless death interrupted the project. This project was never carried out. Tartaglia bequeathed to his friend Curtius Trojanus, the famous Venetian publisher, a manuscript to which he had added several figures. This manuscript is the treatise which we discussed at length as being the work of the Precursor of Leonardo da Vinci. In accordance with the express wish of the great geometer, Curtius Trojanus published[11] this treatise after appending to it the treatise on specific weights attributed to Archimedes as well as various values of specific weights calculated by Tartaglia himself.

This edition, following the one by Peter Apian, first introduced 17th century geometers not to the original work of Jordanus — which was still unpublished — but to the different commentaries written on the work of Jordanus. We shall see that these commentaries will give rise to serious debate.

## 2. JEROME CARDAN — ALEXANDER PICCOLOMINI

It is impossible to discuss the School of Jordanus during the 16th century without mentioning the name of Jerome Cardan. Cardan, undoubtedly, is primarily a disciple of Leonardo da Vinci. The ideas which he comments upon and develops in his mechanics are those which we read in the notes of the great painter and which Cardan must have known. Yet wherever he adds to Leonardo's discoveries, he borrows from the works of Jordanus and from his School.

We saw in Chapter III the fuss Cardan made about the theory of the Roman balance by taking Archimedes to task for having ignored this problem in favor of less useful research. Yet Cardan was careful not to claim that his own inventiveness played any role in the solution which he gave to this problem because he had found it explained in detail in all of the treatises from the School of Jordanus. The latter had found it in the *De canonio* of the geometer of the School of Alexandria.

Moreover, we can cite at least one of the works from the School of Jordanus from which Cardan drew his information. In one of his earlier works,[12] after having given the theory of the Roman balance, Cardan continues:

From this we can conclude the truth of the following proposition by Blasius:[13] A fly could balance the entire earth if it were placed on the arm of a very long lever. But such figments of an excessive imagination are not useful, because they tend to make science look rather ridiculous.

He thus proceeds to enumerate several problems related to the Roman balance. They are precisely those which make up the *De canonio*, that is to say, those solved by Blasius of Parma, whose *Tractatus de ponderibus* Cardan as well as Leonardo had studied.

We have also seen Cardan examine the reason why a weight suspended from the arm of a balance exerts less weight, the more the arm carrying it approaches the vertical. Although we heard him express very interesting views on this subject dealing with the Principle of Virtual Velocities, those views were permeated with propositions established by Jordanus on the subject of the variation of the positional gravity of a moving weight on a circle.

Nevertheless, Cardan mentions Jordanus only in order to take him to task for the inaccurate proposition on the stability of the balance. Just as Leonardo, and before him his Precursor, Cardan also corrects in this passage the error committed by Aristotle, who had declared indifferent what was in reality an unstable state of equilibrium.

And from this, Cardan asserts,[14] what the Philosopher says is demonstrated. If the weights are equal at F and R (Fig. 60), the balance, nonetheless, returns on its own to the horizontal position, with its pointer along AB. Jordanus neither demonstrates this nor did he understand it. Similarly, he fails to demonstrate that if the pointer is located along QB and below the beam, as it happens when the beam is turned over so that you hold the pointer in your hand with the beam on top, and with a weight which would have normally been pulled down to R and with another equal weight placed at F or with the arms of the balance totally empty, why these arms will not only not return to

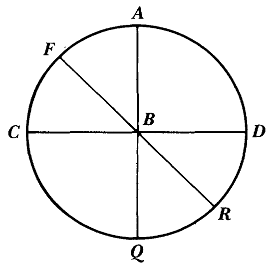


*fig. 60.*

the horizontal position CB, but rather why R will descend toward Q and F will ascend towards A, as is clear through experiments.

These rather confusing arguments which Cardan develops on the questions of stability are in essence those which can be found in the notes of Leonardo da Vinci.

As we saw in Chapter II, Aristotle's mistake was a simple oversight and slip of the pen, and any careful reader could have straightened it out. But to correct the Philosopher, however slightly, was not something to be taken lightly.

During the period which we are now studying, i.e., the middle of the 16th century, Aristotle's *Mechanical Problems*, which had heretofore attracted very little interest, were finally expounded with great care and accuracy by the scholar Alexander Piccolomini.[15] Piccolomini also noticed the oversight of Aristotle, but he dared to reproach the Stagirite only with extreme caution:[16]

The text of Aristotle's words is very unsound as far as this question is concerned. It cost us much effort to establish the true meaning of this text. We made use of a material instrument to establish by means of our senses what the demonstration had disclosed to our intellect. Such an experimental verification is very important according to Peripatetic doctrine.

## CHAPTER X

# THE REACTION AGAINST JORDANUS
# GUIDO UBALDO – G. B. BENEDETTI

### 1. GUIDO UBALDO, MARQUIS DEL MONTE (1545–1607)

From the very beginnings of statics we were able to discern two types of minds grappling with the difficulties of this science: the intuitive versus the deductive mind. We have seen Aristotle, or rather whoever the author of the *Mechanical Problems* might be, look deeply into the principle governing the science of equilibrium, without, however, consolidating his views into perfect order. On the contrary, we have seen Archimedes striving not to set forth any proposition which could not be rigorously deduced from clear and explicitly stated postulates.

Although the impeccably deductive method of the geometer proves to be of value when we have to arrange and classify acquired truths, it is not a method which allows us to penetrate directly to the heart of an obscure problem, and only intuition can cast its nets to the bottom of unchartered depths and dredge for the truths which nourish science. But when the net has been brought in and the rich catch lies spread out on the shore with precious truths intermingled with dangerous errors, deduction must be used to accomplish minute and patient sifting. It must choose what is good, cleanse it of any imperfection, carefully preserve it and cast away anything false and pernicious.

For two thousand years intuition pursued this bountiful search and could be justly proud of its harvest. Deduction hardly ever came to its aid. Most of the fundamental truths of statics are the result of that harvest, but the moment arrived when geometrical rigor began to sort and sift through them, separating them from the inaccurate ideas attached to them and displaying them in full light. When confronted with that confused mass of truth and falsehood, even the best minds hesitate and are uncertain as to what to keep and what to reject.

The selection entrusted to the deductive method is a necessary operation, which must be conducted with judgment and prudence. The most precious conquests achieved by induction are not always precise and clear because the slag of their origins still clings to them and veils them. The geometer who is hurried and not sufficiently perspicacious runs the risk of mistaking one of these truths for a false or worthless proposition, rejecting it with contempt, while a more careful and more enlightened investigation would have caused him to recognize its value.

Too many times the narrow-mindedness of geometers has rejected fertile truths and impeded the progress of science by branding as false, propositions which intuition has formulated without having had enough time to develop the demonstrations. We shall find blatant examples of this narrow-mindedness when we study the reaction against the School of Jordanus, led by Guido Ubaldo[1] and Benedetti. This reaction, undertaken under the banner of deductive rigor, will cast doubt on almost everything discovered by the intuitive method from Aristotle to Leonardo da Vinci.

In the middle of the 16th century, the geometrical approach is particularly glorified. The masterpieces of Greek science are finally studied in the original. Their rigor and elegance, which were only hinted at in Arabic versions, now appear in full light. Pappus, and especially Archimedes, whose works had remained unknown for a long time, are proof that the deductive method can be applied with as much rigor to the field of mechanics as to the demonstrations of geometry.

Assuredly, not everyone was entranced by the subtle analysis applied by the great geometer of Syracuse to determine various centers of gravity and centers of roll. Those who were more inclined towards the practical side of mechanics thought that the results were certainly not worth the effort.

Archimedes, Cardan wrote,[2] discovered two justifications for the center of gravity. The first one concerns suspended weights and the second one bodies floating on water. In each one of these apparati, including the one on the screw, one finds the subtlety which can be expected of such a famous author. However, the rewards are not proportional to the effort, because no one, from Archimedes to the present day, has been able to demonstrate what utility derives from these contemplations.

Those, however, who appreciated the beauty of geometry did not at all share the somewhat vulgar utilitarianism professed by Cardan. Francesco Maurolico of Messina (1494—1575) and Frederico Commandino of Urbino (1509—1575) translated and commented upon the works of Archimedes. More precisely, they found new applications for the methods invented by the illustrious Syracusan and used them to determine centers of gravity unknown till then. The research which had been undertaken by Maurolico in the above described spirit was finished by 1548 but not published until a century after the author's death.[3] Commandino saw his own work published during his lifetime.[4] Other geometers followed the same path. Among them were Luca Valerio,[5] whom Galileo called[6] a "second Archimedes of his time," and Guido Ubaldo.

Such minds as theirs, used to the detailed requirements of the Euclidean method, must have been highly scandalized when faced with the often profound, but almost always murky and confusing intuitions of the great mechanicians of the 13th century. Had these geometers been more perspicacious, they would have found a rich and fertile use for the works left by those mechanicians. They could have undertaken to separate truth from error, to reject the latter and confirm the former. But blinded by the all too evident errors contained in these works, the admirers of Archimedes refused to recognize the very great and valuable measure of truth contained in medieval statics. They used their critical faculties solely to disparage and to reject indiscriminately everything produced by the School of Jordanus.

Thus the admiration professed by the mid-16th century mathematicians for the finished and polished works of the Greek geometers had at first a unique consequence. It negated progress made in statics by abandoning valid truths which would not be reestablished until the middle of the 17th century, after prolonged and laborious effort. In particular, this reaction is the work of two equally subtle disciples of Euclid and Archimedes: Guido Ubaldo del Monte and Giovanbattista Benedetti.

Guido Ubaldo, Marquis del Monte, was a very skillful geometer, who elegantly used the procedures dear to the masters of Greek geometry. His treatise *On the Screw*[7] as well as his research on centers of gravity[8] are proof of his skill. These efforts are also of interest to mechanics and we shall have to mention several passages contained in this research when we study the Principle of Torricelli in a later chapter (Chapter XV, Section 8).

His treatise on mechanics,[9] which was very popular at the end of the 16th and at the beginning of the 17th century, not only bears the mark of Archimedes' influence, but Guido Ubaldo also knows Pappus, whom he quotes in his *Commentary* to the books of Archimedes. And through Pappus he knows the *Mechanics* of Hero of Alexandria. A summary of the latter can be found at the end of the *Mathematical Collections.* Ubaldo undoubtedly borrows from this summary the enumeration of simple machines with which his own study deals, an enumeration which will be preserved religiously up to the present day.

The work accomplished by Guido Ubaldo in mechanics contributes less to his fame than his work in geometry. No work better illustrates how an exaggerated regard for deductive rigor can blind the mind and cause it to ignore the most precious truths. Because of his punctilious mind, Guido Ubaldo takes trifles for grave errors. He severely takes his predecessors to task for always having treated the verticals extending from different points on a lever or a balance as lines parallel to each other. However, he is himself guilty of inaccuracies when he attempted to account for their mutual inclinations and in most cases he, too, ignores these inclinations.

His severity is primarily directed against the School of Jordanus. At the very beginning of his work, he proposes to review the theory of the balance,

> Because, he says,[10] it is astonishing to see how much trivia Jordanus accumulated regarding this question, although he enjoys great authority among modern geometers as well as among other authors intent on discussing this problem.

Jordanus erroneously stated that a lever with equal arms and with equal weights suspended at their extremities is in stable equilibrium. Guido Ubaldo correctly states that such an equilibrium is indifferent.

> But, he adds,[11] certain geometers profess a different opinion concerning this assertion and raise several objections. It will, therefore, be necessary to stay on this subject for a while and I shall attempt to defend, as much as I am able, not only my own opinion, but the opinion of Archimedes, which seems to coincide with my own.

Facing this statement is a note bearing the following entry:

> Jordanus, *De ponderibus*, Hieronymus Cardanus, *De subtilitate*, Nicolaus Tartalea, *De quaesitis ac inventionibus.*

The proposition impugned by Guido Ubaldo is certainly incorrect, but it is, nonetheless, merely an illegitimate application of a very correct and fertile lemma: namely, when the arm of a lever carrying a load at its extremity rotates about its point of support, the positional gravity of the load is smaller, the closer the lever is to the vertical. Guido Ubaldo does not dare question this fundamental proposition, but he assembles the criticism of his predecessors against the demonstration.

Some of this criticism is quite ridiculous, such as the reproach for not having taken into account the mutual inclination of the verticals in this problem. Other objections, although apparently better founded, would not have occupied for long a more perspicacious and less carping mind.

When Jordanus and his successors attempt to determine the ratio between the true gravity of a weight and its positional gravity, they assume that the weight descends along a given arc and compare the length of the arc to its projected length on the vertical. However, the ratio between these two lengths changes depending on the length of the arc.

If this argument were correct, the same weight in the same position would be heavier or lighter depending on how one considers its behavior in the same position: this is impossible.

The contradiction is only apparent. In order to know the ratio between the true gravity and the positional gravity, one needs to take to the limit the ratio between the arc traversed and its projection on the vertical. Jordanus had pointed this out as clearly as one could expect of a 13th-century author. The Precursor of Leonardo da Vinci had more than pointed it out in a passage reproduced in the *Quesiti* of Tartaglia. Instead of rejecting such a consideration, Guido Ubaldo, because of his experience in the computation of limits from his studies of Archimedes, should have been the very person to clarify this matter once and for all.

Quite the contrary, he resolutely rejects any determination of the positional gravity deduced from the inclination of the trajectory:

The human mind will not rest until a cause other than this one is found to explain the variations in this gravity. Indeed, it seems to be more of a designation than a true explanation. This variation derives from a cause other than the one deduced for a motion more or less straight or oblique.

What then is this cause which escaped the School of Jordanus? Let us assume at D (Fig. 61) a weight moving on a circumference in a vertical plane. If the weight were free and without any hindrance, it would fall straight down along the vertical DS.[12] It is prevented from doing so by the radius CD which forces it to move along the circumference:

. . . which pushed it, in a certain sense, and which, by pushing it, partially supports the weight. Thus the radii CD and CL offer resistance to the fall of the weight but not equally.

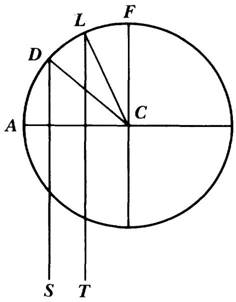


*fig. 61.*

Each one resists more, the more acute the angle it forms with the vertical. At F, where the radius coincides with the vertical, this resistance entirely cancels the gravity of the moving body.

The radius CD offers less resistance to the weight placed at D than radius CL to the weight placed at L. Thus radius CD supports less than CL. The weight is freer at D than at L . . . That is why it is heavier at D than at L . . . Thus the same weight can be heavier or lighter depending on the conditions of its position. this is not because it actually acquires a new gravity or loses its original gravity because of its position — it always keeps the same gravity at any point — but because it weighs more or less on the circumference.

These considerations assuredly contain a nucleus of truth. When a weight is moving on a given trajectory, it is fitting to consider not only the component of the weight tangent to this trajectory, but also the component of the weight normal to the trajectory which the moving body is constrained to follow. This idea had already occurred under various guises to Leonardo da Vinci, and Guido Ubaldo merely reconsiders it.

However, once this is admitted, we cannot see at all why it would be more logical and more natural to determine the normal component rather than the tangential component. We cannot see how the considerations set forth by Guido Ubaldo furnish him with the quantitative determination of the normal component. Finally, we cannot see how, without knowing any law on the composition of forces, our geometer can deduce the tangential component from a knowledge of the normal component.

We can judge by the following passage how vague and erroneous Guido Ubaldo's views on the composition of forces remain:

If the arm of a balance OD (Fig. 62) is longer than the arm OC, a weight placed on O will be heavier if it is suspended from the extremity of the first rather than the second arm, because the descent of the weight will be closer to the natural movement along the circumference OH than along the circumference OG.[13] If the center of the balance is placed at D, the weight will be freer and less bound than with the center placed at C. Therefore, it will be heavier.

Father Mersenne, who reproduced[14] this demonstration in an appendix to Galileo's *Mechanics* adds:

Aristotle believes that the reason for this must be taken from the fact that the center impedes those weights closer to it more than the more distant to the extent that it constrains them more and imparts to them as much as it can of its own immobility . . . . We can easily apply this to the proximity or the distance of created beings from divine perfection, which renders reasonable beings all the more fixed and immobile in its grace and in the firm resolution to goodness, the closer they approach it.

Father Mersenne quite rightly compares Guido Ubaldo's ideas with those of Aristotle. The argumentation which we noted is inspired by certain passages, although perhaps not the best, from the *Mechanical Problems*. The Marquis del Monte was influenced either by the Peripatetic commentator on Jordanus or by Tartaglia's *Quesiti*. However, the maliciousness of his criticism of the School of Jordanus did not prevent him from adopting the gravest mistakes of that School.

Moreover, if certain arguments concerning positional gravity still contained faint traces of the great truth discovered by Jordanus, these traces soon vanished completely in the thoughts of Guido Ubaldo, so

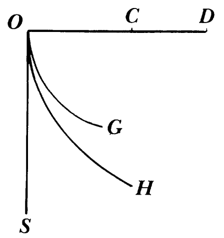


*fig. 62.*

obsessed was he with false rigor. This geometer even fails to recognize[15] the notion of the moment of a weight with respect to the point of support of a lever:

If a force, he says, supports by means of a lever a weight which has its center of gravity on the lever, the force necessary to support the weight will remain the same, in whatever position the lever maintains the weight.

I maintain that the same force, applied at either K or B or G (Fig. 63), will always support the same weight because the weight, with respect to the lever AB, behaves as if it were suspended at E and, with respect to the lever GF, as if it were suspended at L and finally, with respect to the lever HK, as if it were suspended at M.

When the center of gravity of the weight attached to the lever is not on the lever, the magnitude of the force depends on the inclination of the lever and follows a line which changes direction depending on whether the center of gravity is above or below the lever. The weight attached to the lever always behaves as if it were suspended from this lever at the point upon which its center of gravity will project.

If by "force" we understand a weight suspended at the extremity of a lever, Guido Ubaldo's proposition is correct, because the inclination of the lever diminishes in the same proportion as the moment of this weight and the moment of the lifted weight. Ubaldo's proposition is incorrect, however, if the force always remains normal to the arm of the lever, as happens when the hand of a laborer exerts the force. Everything in Guido Ubaldo's work seems to indicate that force is understood by this geometer in this latter sense.

If, indeed, the word 'force' had for him the same meaning as the words 'suspended weight,' we would be unable to explain the following: first of all, how he distinguishes the lever from the balance, the latter being a rectilinear and weightless beam with a weight suspended at each

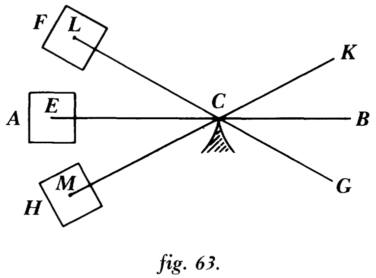


*fig. 63.*

of the extremities. And secondly, we could not explain why, after having set forth in Archimedean fashion the theory for this latter device, he believes that he should at this point establish the laws for the first of these devices. Thirdly, why, in the deduction which extends the laws of the balance to a horizontal lever, he takes care to specify that he is substituting for the force a suspended weight of the same magnitude which under these conditions is certainly equivalent to it. And finally, why the figures which always show a suspended weight when Guido Ubaldo reasons about the effects of such a weight, only indicate the point of application of the force, while the deduction uses this very same notion.

Thus it seems that in the mechanics of Guido Ubaldo, just as later in the works of Descartes, the force applied to the lever is perpendicular to the length of the arm of the lever. It is precisely there that his theory of the lever is seriously flawed.[16]

What a strange remark to make about an author who professed this theory but was considered and is still considered by a great many mechanicians as the discoverer of the concept of moment. He owes this usurped reputation to a passage in Lagrange's *Mécanique analytique*. Here is that passage:[17]

> . . . this is what is called today the principle of moment, by which is meant the product of a force multiplied by the arm of the lever upon which it acts . . . this general principle suffices to solve all of the problems of statics. During the first stages of the development of the theory of simple machines after Archimedes, the study of the windlass brought this principle into prominence, as one can see in the work of Guido Ubaldo entitled the *Mecanicorum liber*, which was published at Pesaro in 1577. However, this author did not know how to apply it to the inclined plane nor to the other machines which depend on it, like the wedge and the screw, about which he gave us only a rather inaccurate theory.

Nothing that Guido Ubaldo says about the windlass justifies this view of Lagrange. Yet, there is another passage[18] by Lagrange which is not less favorable to Guido Ubaldo and hardly any better founded:

> One only needs to examine the conditions of equilibrium in a lever and in other machines to see how easy it is to recognize this law that the weight and the force are always in inverse ratio to the spaces which both traverse in the same time. However, the Ancients did not seem to know this. Guido Ubaldo was perhaps the first to recognize this in the lever or in the block and tackle. Galileo recognized this later.

In fact, the law which Lagrange discusses here had been known since

Aristotle. It was frequently used by the geometers of the School of Alexandria, as well as by the mechanicians of the Middle Ages. Jordanus had shown how it had to be modified in order to be applicable to the straight lever. The Precursor of Leonardo da Vinci had deduced from it the theories of the bent lever and the inclined plane. Leonardo da Vinci and Cardan applied it to the block and tackle, to the screw and to a whole range of machines. It would have been quite surprising if Guido Ubaldo had not known of this law.

Indeed, Guido Ubaldo does know this law, although not in the final form which Jordanus and the Precursor of Leonardo da Vinci had given it, but rather in the form given in Aristotle's *Mechanical Problems*. Guido Ubaldo is so cautious that he does not see in it the principle from which all statics can be deduced. He relegates it to the status of an observation or of a corollary.[19]

For example, Guido Ubaldo establishes the law of equilibrium of the lever by a strategem borrowed from Archimedes. Once this law is established, he deduces from it that the force capable of supporting a weight is to this weight as the paths described by the extremities are to each other when the lever is rotated. He adds the following corollary:

> It is evident by this that the ratio of the path of the moving force to the path of the weight being moved is larger than the ratio of the weight to the force. Indeed, the path of the force is to the path of weight as the weight is to the force which supports it, but the force capable of supporting the weight is less than the force capable of moving it.

The weight always acts along the vertical, while for Guido Ubaldo the force is probably normal to the lever. Although the ratio between these two forces capable of maintaining equilibrium changes according to the inclination of the lever, Guido Ubaldo seems to deny this change, as we have seen. On the other hand, a sound understanding of the principles of mechanics would require the path described by the point of application of the force to be compared not with the path described by the point of application of the weight, but with the projection of this path onto the vertical. One can see how vague and uncertain these ideas are concerning the ratio between the work of the driving forces and the work of the resisting forces which exist in every machine.

The difficulties involved in applying this law to the lever no longer exist when one is dealing with pulleys or the block and tackle, which lift a weight vertically with the help of a force always pulling in the same direction. There the force is to the weight which it balances as the path traversed by the weight is to the path described by the force. Guido Ubaldo can thus apply correctly the law in question[20] to these machines, but yet, as was the case for the lever, he, again, does not use this law as the principle behind their theory. He reduces the study of each arrangement of pulleys to the analysis of a given combination of levers. Then once the condition of equilibrium for a block and tackle has been established, he deduces from this condition the observation that the same ratio which exists between the path traversed by the weight is also the same for the weight and the force supporting it. This corollary routinely reappears after each study of various types of block and tackle. Since, according to Guido Ubaldo, a greater force is needed to move a weight than to balance it, he adds:[21]

> It is evident by the foregoing that the ratio between the path of the moving force and the path of the weight is always greater than the ratio between the weight and the motive force.

In Peripatetic mechanics, it was natural to emphasize the ratio of the velocities between the motor force and the weight being moved. Guido Ubaldo is particularly interested in the ratio of the paths which these two forces traverse in the same time. Although the two ratios are equal to each other, a different interpretation occurs, which deserves to be pointed out because Guido Ubaldo's statics certainly influenced Descartes, who proclaims so clearly the necessity for considering not the virtual velocities but the virtual displacement.

This does not mean that in his study of machines Guido Ubaldo completely fails to consider the velocities or the duration of transport as Descartes will require. But he attaches little importance to this consideration. Accordingly, it is not until the very end of his study on pulleys and the block and tackle, that he states the following proposition:[22]

> The easier it is to move a weight, the more time one needs to move it and the harder it is to move the weight, the faster it can be moved.

The theory of the windlass[23] is constructed in the same fashion as the theory of the combination of pulleys. It ends with the same comparisons between the paths described by the weight being moved, as well as between the ease with which a weight can be moved and the time it takes to move it. In this case, too, these comparisons are given as corollaries to the condition of equilibrium of a windlass and not as principles from which this condition could be deduced.

After having hastily reduced[24] the analysis of the wedge to the theory of the lever, Guido Ubaldo reduces[25] the analysis of the screw to a combination of lever and inclined plane. This reduction is done correctly, but the Marquis del Monte is unable to deduce from it a satisfactory theory of the screw because he clings to the law of the inclined plane as formulated by Pappus. Indeed, one of the worst consequences of the excessive reaction against 13th century statics, caused by an uncritical admiration for the Ancients, was the return to the theory of Pappus. This theory was essentially contrary to the principles of the School of Jordanus because it attributes a positional gravity to a mobile body on a horizontal plane, even though its trajectory does not project on the vertical at all. The doctrine of Pappus found so much favor with geometers that Galileo found it difficult to discredit this doctrine and to establish that the slightest force is sufficient to move a body on a perfectly polished, horizontal plane. The law of the inclined plane as formulated by Pappus can only give an erroneous theory of the screw. That is why Guido Ubaldo states this corollary without any demonstration:

It is evident by the foregoing that the more numerous the turns of a helix, and the longer the cranks or the arms of the windlass, the easier but slower it is to move a weight.

The mechanics of Guido Ubaldo is a work containing errors and it is always mediocre because it often is outdated by the ideas published in the works of Tartaglia and Cardan. We had to study it, nevertheless, because it was very popular towards the end of the 16th and the beginning of the 17th century. We shall have to note often the influence which it had on the works of that time.

## 2. GIOVANBATTISTA BENEDETTI (1530–1590)

Giovanbattista Benedetti was not known for his modesty. He had great confidence in the originality of his own genius. At the beginning of one of his works[26] he addresses the reader by saying:

In these books I have not published anything which I remember having read or heard. If

I occasionally dealt with certain matters which did not originate with me, I either modified the demonstrations in some way or I stated them more clearly. And if, by chance, someone has published anything remotely similar to my work, I either didn't see those publications or I forgot having read them.

Of all the discoveries he prides himself on, he ranks first his research on mechanics as that which will assure the immortality of his name:[27]

Atque vel hoc uno modo me inter humanos vixisse testatum reliquerim.[28]

This pride was, moreover, not totally unfounded, for Benedetti had scarcely reached the age of twenty-three when he furnished proof of his originality.

It was commonly accepted that given two bodies of the same material, but with one having twice the volume of the other, the first body would fall twice as fast as the latter. The proposition was an axiom of Peripatetic physics and was the main subject of the fragment *De ponderoso et levi* attributed to Euclid. Jordanus had used it as the first theorem of his treatise *De ponderibus*. Various commentators including the Peripatetic commentator and the Precursor of Leonardo da Vinci had all scrupulously retained this theorem. Leonardo da Vinci had formulated it on several occasions and in the *Quesiti et Inventioni diverse* Tartaglia had reproduced precisely the description which the *De ponderoso et levi* contained.

It is the validity of this universally accepted proposition, which Benedetti in his very first work[29] dared to dispute. In the dedication of his work to Gabriel de Guzman,[30] he showed by a very simple reasoning that bodies of the same substance but of different volume must fall with the same velocity. This is the reasoning, later resumed in his *Diversarum speculationum*,[31] which Galileo was to reproduce in his earliest research. The proposition to which Benedetti was led by this reasoning was also adopted by Cardan,[32] who in turn justified it with peculiar reasons. Plagiarized by Jean Taisnier,[33] the ideas which Benedetti had developed in his first work came to the attention of Stevin, who, in collaboration with John Grotius, submitted them to the test of experiment.[34] He found them to be in no better accord with the truth than the principles taught by Aristotle.

Benedetti could with good reason insist on the originality of this idea and his dedication to Gabriel de Guzman ended with these words:

This truth did not originate in the mind of Aristotle nor did it originate in the mind of any of his commentators whose work is known to me, nor from anyone with whom I have conversed and who professes the opinion of that philosopher.

His new doctrine was in direct opposition to all of Peripatetic mechanics.

We should not be surprised to see him reject the theory of the balance founded on the Principle of Virtual Velocities. The laws of this instrument:

. . . do not depend[35] at all on the swiftness or the slowness of the motion.

But Benedetti is not content to merely reject Aristotle's statics. He condemns with equal severity the doctrine of the School of Jordanus. Two chapters[36] of his work are devoted to examining:

. . . various errors professed by Tartaglia on the weight of bodies and their motions, some of which were borrowed from an ancient author called Jordanus.

Benedetti does not simply reject the erroneous conclusions of Jordanus repeated by Tartaglia. Not a single proposition formulated by these authors finds any favor in his eyes.

Jordanus, for example, very accurately demonstrated how the positional gravity of a weight moving along the circumference of a circle in a vertical plane diminishes as the weight is displaced from the horizontal diameter.

What he writes is true, Benedetti declares, but the cause which first Jordanus and then Tartaglia ascribe to this effect has no basis in nature.

The eighth proposition of Tartaglia, which is the sixth question of Jordanus, is much better demonstrated by Archimedes because neither Jordanus nor Tartaglia have proven its correctness.

The reasoning which Benedetti treats with such disdain is none other than the elegant and fertile demonstration by which Jordanus de Nemore justified the law of the equilibrium of the lever.

The Precursor of Leonardo demonstrated that the positional gravity of a weight placed on an inclined plane was the same at every point. This proposition could rightly be considered obvious. But according to Benedetti, "it is false for two reasons."

This proposition is, moreover, in the work of the Precursor of Leonardo da Vinci only a kind of lemma which he uses to prepare his elegant solution to the problem of the inclined plane. However, it is far from meeting with our punctilious geometer's approval:

The fifteenth question of Tartaglia is the eleventh question of Jordanus, whose work was retrieved from the darkenss of oblivion and published by Trojanus, the Venetian editor. It is absolutely worthless.

What then remains of statics for Benedetti after he has rejected with such severity everything which depends on the idea of virtual displacements? All that is left is the rule of the lever and the notion of the moment of a weight with respect to a point.

The weight suspended at the extremity of a beam of a balance, he maintains,[37] has a gravity more or less large depending on the position it occupies . . . The weight can not descend on the vertical FuM (Fig. 64) unless the arm FB becomes shorter. It is clear that the weight F exerts a given effort on the center B by means of arm FB . . . Now we have to presuppose that the weight carried by the arm exerts at the center B an effort which is all the greater the closer its vertical (FuM) is to the center . . . , so that the closer the weight F is to A, the more it will lie on the center and the lighter it will be.

Despite Benedetti's pretensions to absolute originality, we can easily recognize the preamble from which we just quoted several sentences and which is borrowed from Guido Ubaldo. We also recognize what follows,[38] because the Precursor of Leonardo could claim it for himself:

The ratio between the gravity of a weight placed at C and the gravity of the same weight placed at F is equal to the ratio between the length of the arm BC and the segment Bu. . . . It is all the same whether the weight F, equal to C, is at F at the extremity of the arm BF, or at u at the extremity of the horizontal arm Bu. This will be evident to us if

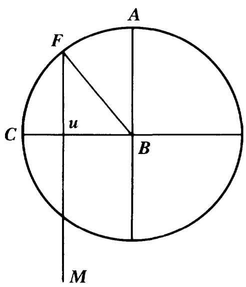


*fig. 64.*

we imagine a vertical rope Fu with the extremity u to which is suspended the weight which was at F. It is clear that the weight suspended in such a way will produce the same effect as if it were at F.

This notion of the moment of a weight with respect to a point is at once generalized by Benedetti to apply to any arbitrary force:

If one wishes to compare the magnitudes by which one can measure the effects of weight or motor forces, one can determine each one of them by means of a perpendicular dropped from the midline of the lever in the direction of the force.

This statement too is not a newly discovered law at all because it can be found almost word for word in the notes of Leonardo da Vinci.[40] And the figures drawn by the great painter closely resemble those which Benedetti uses to support his demonstrations.

Moreover, from this point on, there is nothing in the statics of Benedetti which does not very faithfully reproduce the ideas of Leonardo da Vinci. Thus our geometer undertakes to show:

... how all the properties of balances and levers depend on the principles mentioned below. ... We could imagine, he adds, that weights are suspended at points *u* and *n* (Fig. 65), although they are attached at *s* and *x* in reality, because point *u* is related in such a way to point *s* and point *n* to point *x* so that any force that moves one, moves the other.[41]

Had not Leonardo already taken this rule which justifies these considerations from the manuscript of his Precursor?[42]

The theory of the equilibrium of the bent lever leads Benedetti to state[43] very accurately the theory of the stability of the balance:

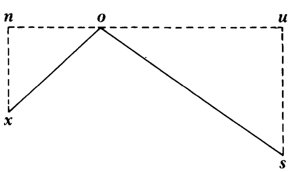


*fig. 65.*

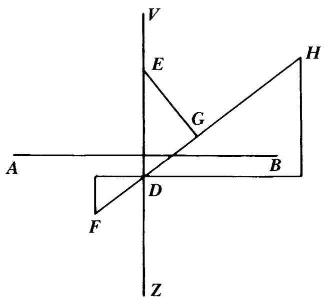


*fig. 66.*

Let AB (Fig. 66) be the balance in its horizontal position. Let E be the point of suspension above the balance. Let us drop the extremity A to F so that the balance is now in position FH. Its midpoint will be at G on the same side of the vertical VZ as point B. VZ will cut the arm FG at point D. DH will thus be longer than FD. Let us suppose, as is correct, that it is all the same whether the balance is supported at either point E or point D when placed in position FH. It follows that the weight suspended at H will exceed in gravity the weight suspended at F by the same ratio that DH exceeds DF.[44] Even if one were to suppose that the material beam FH had no gravity at all, the excess force of the weight placed at H — a force which is much greater than that of the weight at F — would be sufficent to explain why the balance returns to the horizontal position.

Benedetti is correct in considering this theory on the stability of the balance as an improvement over the one proposed by Aristotle, but he did not invent it. Leonardo formulated it[45] quite clearly and it had not escaped Cardan's attention.[46]

These passages are far from being the only ones in which the influence of Leonardo da Vinci on Benedetti is manifest. Benedetti does not treat the block and tackle by utilizing the Principle of Virtual Displacements. Just as Guido Ubaldo had done, he compares these devices to combinations of levers.[47] However, while the Marquis del Monte works from figures which reproduce configurations usually used for pulley blocks, Benedetti, on the other hand, uses very simplified diagrams. It is enough to glance at these diagrams to recognize the drawings devised by Leonardo da Vinci.[48] In Figure 67, drawings A and B are those of Leonardo, drawings C and D those of Benedetti.

Benedetti even reproduces Leonardo's errors. One whole chapter,[49] for example, does nothing but repeat the erroneous law of the composition of forces which had caused the great painter to hesitate after having seen clearly the actual law.

In this chapter, Benedetti considers a weight *n* (Fig. 68) supported by two members *no* and *nu*. If these two supports have the same inclination, it is clear from common sense that the capability of the weight *n* will divide into two equal parts, with one half resting at *o* and the other half at *u* along the two lines *no* and *nu*. In this general case,

It is clear that if the vertical *ni* is further away from the point of support *u* than from the point of support *o*, a greater portion of the weight will be supported at *o* than at *u*. The ratio between the portion of the weight *n* exerting pressure at *o* and the portion of weight *n* exerting pressure at *u*, will not be the same as the ratio between the angles *uni* and *oni*, but the same as the ratio between the lengths *ui* and *oi*.

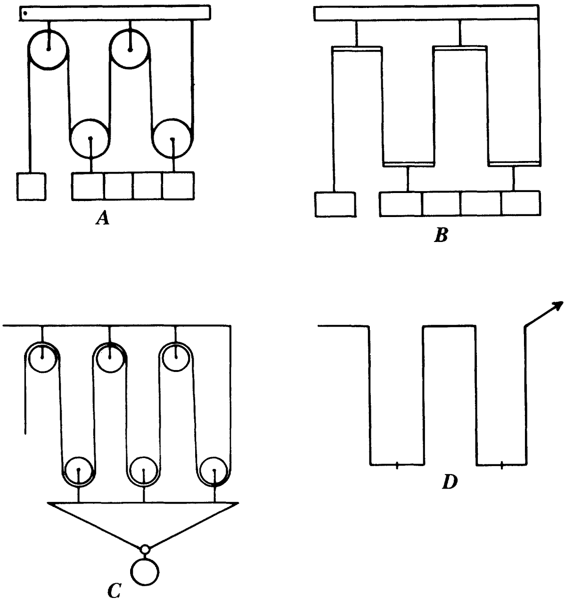


*fig. 67.*

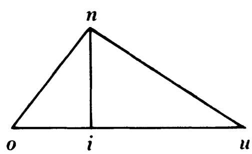


*fig. 68.*

Benedetti thus piously preserves for us certain false ideas of Leonardo da Vinci. What remains in the statics of Guido Ubaldo and Benedetti of all the fertile ideas which inspired the great genius and of all the splended discoveries made by his Precursor? Hardly anything, and what is more, all of these truths will have to be rediscovered. That will be the task of Galileo, Simon Stevin, Roberval, Descartes and Torricelli.

CHAPTER XI

# GALILEO GALILEI (1564–1642)

For a long time Galileo was a poor, luckless wight. In 1589, at the age of twenty-five, he was in dire straits. Then his friends procured for him the chair in mathematics at the University of Pisa with an annual salary of sixty florins. With such meager resources he had to provide for a large family because he had become its sole support after the death of his father. Furthermore, after three years, he lost this meager income as well by having offended Giovanni de Medici's self-esteem as an inventor.[1] During the summer of 1592, Galileo went to Venice. He left Florence with a trunk weighing less than a hundred pounds, yet containing everything he owned.[2]

Because of this abject penury, it was difficult for Galileo to find a publisher for his works. When he had his first book published in 1606 on the subject of the compass of proportion,[3] he had already been teaching for seventeen years and had made numerous discoveries.

For lack of a publisher, he copied or had copies made from his works, which he sent to friends in and outside of Italy. He retained this habit for the rest of his life. When in 1636, for example, he had finished his famous *Discorsi*,[4] he did not print them, but had them circulate as manuscript copies throughout the scientific community of Europe.

Occasionally, those who received a copy from the great geometer did not wish to keep to themselves a discovery which they admired so much and thus gave Galileo's manuscript to a printer. Thus it was in 1634 that Mersenne published a French translation of the *Mechanics*, although the Italian version was not to be printed until after Galileo's death. The same thing happened in 1636, when a manuscript copy of the *Discorsi* reached Conte in Paris. Conte did not wish to deprive the world of such a treasure and sent a copy to the Elseviers, the great printers of Leyden, who published it in 1638.[5]

But most of the work that Galileo circulated in manuscript form did not have such good fortune. Its only publishers were unscrupulous readers who gleaned more than one novel idea from them and then shamelessly appropriated them. Several of these manuscripts were published after a century of oblivion, while others have been lost.

Because of these diverse circumstances, it is sometimes difficult to follow the evolution of Galileo's thought and to trace the influence of his ideas on the scientists of his time as they spread throughout Europe. A careful study of the numerous documents we have today will, nevertheless, allow us to retrace the main steps by which Galileo made his discoveries in mechanics and in statics.

Let us review the sources we shall use to understand what the development of statics owes to Galileo.

(1) The oldest fragment in our possession is surely a commentary on Aristotle's *De Caelo,* which was preserved in manuscript form at Florence and remained unpublished until the publication of the *Edizione Nazionale* of Galileo's works, which in 1888 brought this short treatise to light.[6] This work, written in Latin, is of interest to our purpose in only one respect. It shows us that at the time Galileo wrote it, he was still a faithful Peripatetic, although he had read and quoted from the *De Subtilitate*[7] of Cardan, as well as from the *Exercitationes* of Scaliger.[8] The thoughts of these authors are only accepted by Galileo to the extent that they are in accord with the tradition of the School. It is from this tradition and not from Cardan's argumentation against perpetual motion that he borrows the following two maxims:[9] "Motus simplex terminatur ad quietem." "Nullum violentum potest esse perpetuum."[10]

(2) The *Edizione Nazionale* of Galileo's works also brought to light two drafts of a Latin treatise *De Motu,*[11] preserved in manuscript form at Florence. The study of this treatise is of great importance because Galileo's first ideas on statics, hydrostatics and dynamics appear there.

(3) One segment of this Latin treatise *De Motu* was taken up again later by Galileo, who rewrote it in Latin in the form of a dialogue, a form he was fond of all his life. This dialogue was published for the first time in Volume XI of the sixteen volume edition of Galileo's works published by Eugenio Alberti in 1856 in Florence.[12]

(4) In all the versions of *De Motu,* Galileo discussed at length solid bodies floating on a liquid. The ideas he expressed are developed in a work entitled: *Discorso al Serenissimo Don Cosimo II, Gran Duca di Toscana, intorno alle cose che stanno in su l'acqua, o che in quella si muovono, di Galileo Galilei, filosofo e matematico della medesima Altessa Serenissima* (Discourse to his most Serene High-

ness Don Cosimo II, Grand Duke of Tuscany, on bodies floating on water or moving in water, by Galileo Galilei, philosopher and mathematician to his most Serene Highness). This work, printed in Florence in 1612, contains not only an account of Galileo's hydrostatic theories, but also an initial definition of a concept which will be of great importance for the statics of the illustrious geometer, a concept he refers to as 'momento.'

(5) By mechanics, Galileo means the study of simple machines. His views on mechanics have come down to us in three different formulations.

The first formulation can be found in a manuscript in Galileo's own hand. This manuscript belonged to Prince Hermann of Fürstenberg, who was a disciple of Father Kircher in 1646 in Rome. Brought by the Prince to Germany, the manuscript is kept today in Regensburg in the archives of the Turn-und-Taxis family. This manuscript is a summary of the lectures given by Galileo in 1594 at the University of Padua, as the title suggests, *Della Meccaniche lette in Padova dal Sr. Galileo Galilei l'anno 1594*,[13] It was published in 1899 by Favaro.[14]

(6) In 1634, Father Mersenne of the Order of the Minims had Henry Guenon of Paris publish a small volume containing three works. Two of these works were the *Préludes de l'Harmonie universelle* and the *Questions théologiques, physiques, morales et mathématiques*,[15] These were original works of the hard-working and prolific churchman. The third book was composed of Galileo's various versions on mechanics translated from an Italian manuscript.[16] This work is much more developed than the manuscript published by Favaro.

(7) No Italian edition of the *Mechanics* was ever printed during Galileo's lifetime. It was not until 1649 that the Cavaliere Luca Danesi had the following work printed in Ravenna: *Della Scienza Meccanica e della utilità che si traggano dagl' instrumenti di quella*; opera del Signor Galileo Galilei con uno frammento sopra la forza della percossa. (On the science of *Mechanics* and the utility which can be derived from its instruments; A Work of Galileo Galilei with a Fragment on the Force of Percussion.) This work contained in a more developed form everything which was in the *Mechanics* translated by Mersenne. All of the editions of Galileo's works contain this treatise *Della Scienza Meccanica*.

(8) In 1632, the famous *Dialogo di Galileo Galilei della due massimi Sistemi del Mondo, il Ptolemaico e il Copernicano* (Dialogue of Galileo Galiei Concerning the Two Chief World Systems; the Ptolemaic and Copernican) was published in Florence. This publication brought down on its author on June 22, 1633 a condemnation by the Holy Office. On the Second Day in this Dialogue, Galileo is led to treat parenthetically the principles of statics and, in particular, the equilibrium of the lever.

(9) We have indicated before how Conte rather unexpectedly had the Elseviers publish the work under the following title: *Discorsi e dimostrazioni matematiche intorno a due nuove scienze attenti alla Meccanica, ed ai movimenti locali; di Galileo Galilei, Linceo, filosofo e mathematico primario del serenissimo Gran Duca di Toscana*; Leida, Elsevirii, 1638 (Mathematical Discourses and Demonstrations on the Two New Sciences concerning Mechanics and local motion by Galileo Galilei, member of the Accademia della Lincei, principal philosopher and mathematician to His Most Serene Highness the Grand Duke of Tuscany; Leyden, the Elseviers, 1638.)

This edition contained hardly anything of interest to statics. The subsequent editions of the *Discorsi* — the first one published in 1655 in Bologna — contain, on the contrary, two passages which pertain to this science.

The first of these passages is the *Scholium* added to Theorem II, Proposition II of the Third Day in which the interlocutor Salviati gives the theory of the inclined plane. This *Scholium*, which will be discussed in detail in Chapter XV, was written by Galileo towards the end of his life and sent by him to Father Castelli on December 3rd, 1639 so that it could be added to The Third Day of the *Discorsi* in any new edition.

The second of these passages can be found in the *Giornata Sesta, della forza percossa* (the Sixth Day, on the force of percussion). The first edition of the *Discorsi* contained only the first three days. Everything following these three days was written by Galileo after 1636 and first printed in 1655 in the first edition of the *Opere di Galileo Galilei*, thanks to the good offices of Viviani.

Let us now follow the evolution of the doctrines on statics as professed by Galileo in the works we have enumerated. From his first work on *De Motu*, we see Galileo invoke as an axiom of statics the impossibility of perpetual motion. He uses this axiom to prove that a solid body with the same density as water will remain suspended in equilibrium in the water:

I conclude, furthermore,[17] that it will neither rise nor descend, but that it will remain wherever one chooses to place it. There is, indeed, no reason why it should ascend. Since we assume that it has the same weight as the water, to say that it will descend in the medium of the water would be to say that water within water descends below water and that the water which rises above it will be able to descend too. The water would thus rise and descend alternatively, 'quod inconveniens est'.[18]

This belief in the impossibility of perpetual motion could have come from reading the *De Subtilitate* of Cardan, but it could have also been taken from the scholastic axioms according to which natural motion tends towards a state of rest and violent motion dissipates. In his commentary on the *De Caelo*, Galileo formulated these axioms, which Leonardo had merely developed and brought up to date.

The influence of Cardan and through him that of Leonardo da Vinci can be seen more clearly in the interpretation of the principles of Archimedes, which Galileo repeats with minor modifications in the two drafts of his *De Motu* and in his dialogue on the same subject. He will return to this subject in more detail in his *Discorso intorno alle cose che stanno in su l'acqua*. This would be the place to discuss this interpretation, which is a kind of unpolished and erroneous application of the Principle of Virtual Displacements, if it were not for the fact that we want to postpone the study of hydrostatics until later.

When Galileo begins the study of the inclined plane, the influence of Cardan is incontestable. Although Galileo does not mention any works by the Milanese physician other than his *De Subtilitate*, in his *De Motu* it would be hard to deny that he had not read the *Opus novum*. How can we not think of the fragment in the *Opus novum* which we quoted at the end of Chapter III, when we read the following passage:[19]

Let us assume *ab* (Fig. 69) as a line directed towards the center of the earth and perpendicular to the horizontal plane. From point *b* let us draw any given number of lines *bd* and *bc* which form acute angles with line *be*. It is necessary to ask why a moving body which descends along the line *ab* has the fastest fall; why the fall is faster along *bd* than along *bc*, but slower along *be* and why the fall is slower along *bc* than *bd*? Furthermore, we ask by how much the fall is faster along *ba* than along *bd* and how much faster the latter is than along *bc*? In order to resolve these questions, we first have to make the observation indicated above. It is obvious that a heavy body is drawn

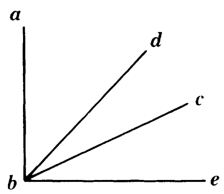


*fig. 69.*

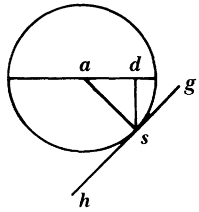


*fig. 70.*

downwards by a force equal to the one needed to pull it upwards. In other words, the heavy body is drawn downwards by a force equal to the resistance which it exerts against upward motion. If we determine how much smaller the force which would pull the heavy body upwards along the line *bd* is than the force which would pull it along the line *ba*, we will immediately know how much greater the force which causes the same heavy body to fall along the line *ab* is than the force which causes it to fall along the line *bd*.

Even if it is true that Galileo borrowed this introduction to the study of the inclined plane from Cardan, nonetheless, he went much further in the analysis of the problem than his predecessor. Because he was content to use an inductive approach, Cardan had arrived at an inaccurate solution. Galileo, using ingenious intuition, arrives at the correct law.

Let us imagine a weight concentrated at the extremity of a straight line rotating about point *a* (Fig. 70) and let us assume this straight line in position *as*. The weight will follow the circumference determined by the center *a* and the radius *as*. Let us draw the tangent *gh* to the circumference at *s*. At the instant the body reaches point *s*, it begins to descend along the arc[20] of the circle which begins at that point and we will be able to treat it as if it were descending along the tangent *gh*. Therefore, the force which would cause the body to descend along the oblique line *gh* is equal to the force which from point *s* on tends to cause it to descend along this arc.

Quando mobile[21] erit in puncto *s*, in primo puncto suus descensus erit veluti per lineam *gh*; quare mobilis per lineam *gh* motus erit secundum gravitatem quam habet mobile in puncto *s*.[22]

Once this daring and fertile intuition is admitted, the problem of the inclined plane is solved. The theory which Cardan and Benedetti deduced from the notes of Leonardo da Vinci or from the manuscripts of his Precursor as edited by Curtius Trojanus — Cardan and Benedetti developed this theory in their different works — is enough to finalize the solution. This theory, indeed, shows us that the force causing the weight placed at *s* to move along the circumference of the circle is proportional to the projection *ad* of line *as* on the horizontal. Simple considerations show us that the gravity pulling a moving body on an inclined plane is to the gravity acting in free fall as the height of the plane is to the length of the line with greatest obliquity. In other words, the ratio between these two gravities is the sine of the angle which the plane forms with the horizontal.

The problem of the inclined plane which Galileo solved in this way attracted at about the same time the attention of another geometer. In 1586, Simon Stevin of Brugge also published the solution to this problem in his *Elements of Statics*.[23] Had Stevin preceded or followed Galileo?

The different versions of the *De Motu* are not dated. Does the oldest one predate or antedate *De Beghinselen der Weeghconst*?[24] It is hard to answer this question. But it is certain that the two geometers, the one from Brugge and the other from Florence, were unaware of the other's essentially different approach to the same problem.

Does it serve any purpose, in any case, to continue to try to determine who had priority? We know, indeed, that the Precursor of Leonardo da Vinci had preceded both of them by three centuries and that the elegant solution of this great geometer had been published in the five editions of the *Quesiti* of Tartaglia as well as in the *De ponderositate* of Jordanus printed by Curtius Trojanus. However, we have not yet finished extracting all the information which a reading of the *De Motu* can yield.

After having been an avowed Peripatetic while writing his commentary to the *De Caelo*, Galileo now argues incessantly against Aristotle's physics. However, he does not totally reject Aristotelian physics. Specifically, he carefully retains the fundamental axiom upon which the dynamics of the Stagirite rest: the proportionality between the force moving a body and the velocity of that body.

We must observe, he maintains,[25] that velocity does not differ from motion; he who postulates motion also postulates velocity and slowness is nothing but a lesser degree of velocity. Thus whatever produces motion also produces velocity: hence, it is the same whether motion derives from heaviness or from lightness, because it is necessary that slowness or swiftness have the same origin. From a greater degree of gravity there follows a greater rapidity of the motion produced by the gravity of the moving body, that is to say, by the downward movement. From a lesser degree of gravity a greater slowness of the same motion results.

Galileo certainly no longer teaches that a ten pound weight falls ten times faster than a one pound weight, as Aristotle had done. He says that in the same medium, weights, whether large or small but made of the same substance, fall at equal velocities. This proposition is inaccurate and completely inadequate compared to the law which he will propose in his *Discorsi*:

All bodies in a vacuum fall with the same velocity.

Galileo must have read the inaccurate assertion in the *Opus novum* of Cardan, who, like Taisnier, had most likely borrowed it from G. B. Benedetti. Galileo must have also read it in Benedetti, whose argumentation he repeats. Yet this assertion does not contradict the axiom cited above. If ten pounds of lead fall with the same velocity as one pound of lead in air in which both had originally been weighed, it is simply because the tenfold force has to move a body ten times more voluminous. Galileo, who[26] repeats Benedetti's view on this point, says:

From this argument derives the solution to the following question: In the same medium what is the ratio between the velocities of two bodies of the same volume, but of different weight? The ratio of the velocities of these moving bodies will be as the excess of specific weight of the medium.

On differently inclined planes, the same moving body has a weight[27] of a known ratio. From here on, Aristotle's axiom will give us the ratio between the velocities at which this moving body will move along these two planes, because it will be precisely the ratio between these two weights:

It is thus certain[28] that the velocities of the same moving body descending along different inclinations will be in inverse ratio to the lengths of these oblique descents corresponding to a given vertical descent.

Galileo adheres to these principles when he later writes the dialogue *De Motu*. In this work, he asserts that in the same medium, two moving bodies of equal volume fall with velocities which are to each other as the excess of specific weight of the moving bodies is to the specific weight of the medium,[29] so that, in a vacuum, the velocities of these moving bodies are to each other as their specific weights are to each other.[30]

Aristotle's axiom, which influenced Galileo so much in his first works on motion, will continue to inform all of his research on statics and make him the defender of the ideas formulated by the Philosopher of Stagira and later developed by Leonardo da Vinci and Cardan. In particular, this axiom will inspire in him a notion which will play an essential role in all of his mechanics: the notion of *momento*.[31]

The same force capable of moving a heavy body at a given velocity is also capable, according to Aristotle's axiom, of moving a body twice that weight, but at half that velocity. The characteristic property of the force is not to be found in the size of the heavy body being moved or in the velocity which it imparts to it, but in the product of these two factors. For the same force, each factor can vary; however, the product is fixed. It is this product which constitutes the *momento* of this force.

It is at the beginning of the *Discorso intorno alle cose che stanno in su l'acqua, o che in quella si muovono*, which still has so many affinities with the different versions of the *De Motu*, that Galileo, in 1612, defines the *momento* for the first time. However, he is careful to show how this notion is related to Peripatetic statics.

I borrow, he says, two principles from the science of mechanics. The first is the following: Two exactly equal weights moving at equal velocities have the same power or the same "momento" in all of their activities. To the mechanicians, "momento" means this capability, this action, this efficient power, by which the motor moves while the moving body resists. This capacity does not only depend on the simple weight, but on the velocity of the motion, and on the various inclinations of the spaces in which the motion is produced. Indeed, a heavy body produces an *impeto*[32] which is larger when it descends on a sharply inclined surface than when it descends on a less inclined surface. Whatever the reason for such a capacity, it always bears the name "momento." and it does not seem to me that this meaning for the word "momento" is new to our language. If I am not mistaken, we often say: This matter is serious, but the other one is of little moment;[33] or while we attend to an insignificant matter, we neglect those of moment. These are metaphors borrowed from the language of mechanics.

The second principle is that the power of gravity increases with the velocity of the body moved, so that absolutely equal weights, but with different velocities, have unequal powers or "momenti," or capabilities. The more powerful of the two is the more rapid by as much as its velocity exceeds the velocity impelling the other body. We find a very appropriate example of this principle in the balance, or the Roman balance, where the arms of the beam are unequal. Exactly equal weights suspended from these arms neither exert equal pressure nor produce equal actions. The weight which is at a greater distance from the fulcrum of the balance lifts the other weight and its motion is faster than the motion of the latter. The velocity of the motion gives to the moving body such a power and capability that they can be compensated for with precision by increasing the slower moving body by an equivalent weight . . .

Such compensation between gravity and velocity can be found in all mechanical devices. Aristotle used it as a principle in his *Mechanical Problems*. Whence, we can consider as absolutely certain the assertion that two weights of unequal sizes will be in equilibrium and will have equal "momenti" every time their gravities are in inverse proportion to the velocities of their motion: or in other terms, every time the lighter weight is positioned in such a way that its velocity is to the velocity of the heavier weight as the weight of the latter is to the former.

The Second Day[34] of the *Dialogue Concerning the Two Chief World Systems* contains references to statics. There Galileo deals with the Aristotelian principle, which the interlocutor Salviati formulates in the folling way:

La velocità del mobile meno grave compensa la gravità del mobile piu grave, e meno veloce. (The velocity of a lighter moving body compensates for the weight of the heavier, but slower moving body.)

The Roman balance serves as the example for this principle which is developed in the same terms as in the *Discorso intorno alle cose che stanno in su l'acqua*.

The *Mechanics* of Galileo was known to the majority of geometers through the translation of Mersenne, which was printed in 1634. But the work must have been written much earlier. We know this from Galileo's own testimony. In 1639, he wrote a passage in dialogue form which was to be included in the *Discorsi* and which was, in fact, included in the *Discorsi* when the first edition of Galileo's works was published in 1655. In this passage,[35] the interlocutor Salviati refers to the treatise *Della Scienza Meccanica* as:

An old treatise on mechanics written some time ago in Padua by our Academician for the exclusive use of his students.

Thanks to Favaro we know today the text of the lessons *On Mechanics*,[36] which were taught by Galileo in 1594 in Padua. This concise text contains considerably fewer reflections on the principles of statics than the works which we have already discussed. When dealing with the lever, Galileo remarks very briefly[37] that by means of this instrument:

what one gains in capacity, one loses in space, time and velocity, and that the same is true for all other existing or imaginary devices.

The capstan[38] and the windlass[39] provide him with another opportunity to repeat the same observation, which he makes again when he discusses the block and tackle[40] and gear trains.[41]

The notion of "momento" is not defined in the *Della Meccaniche.* However, the word is used there. Galileo remarks that the force supporting a weight by means of a lever is not enough to lift it. But he adds,[42]

> since any quantity of momento, however small, added to the force functioning as a counterweight, is sufficient to put the weight into motion, we shall not take into account this negligible quantity of momento . . .

When Galileo gives his theory on the screw in his *Della Meccaniche,* he uses the theory of the inclined plane,[43] without, however, emphasizing this fact:

> Everything we said, he writes, is obvious to native intelligence and through experience. But if we wanted to determine in a demonstrative fashion the relation of the force to the weight which it can move on variously inclined planes, we would be faced with a somewhat more difficult speculation. We shall thus omit it here and be content to take cognizance of the conclusion . . .

However, at the end of his study on the screw, the great geometer points out[44] that if a heavy weight can be lifted with little effort with the use of such a device, it is because the force traverses the long path represented by the helix while the weight only reaches the height of the cylinder. Cardan and Guido Ubaldo had already made a similar observation and it would have been easy to deduce from it the theory of the inclined plane which Galileo will formulate later by repeating almost verbatim the arguments of the Precursor of Leonardo da Vinci.

Salviati was certainly not alluding to the *Della Meccaniche* brought to light by Favaro. Salviati cited, however, these lessons in reference to the theory of the inclined plane, which, he said:

> . . . was demonstrated in a detailed and conclusive fashion with a view to considering the origin and the nature of this marvellous device, the screw.

These words could not apply to the *Della Meccaniche,* which we have analyzed and which is so concise. These words undoubtedly allude to a more complete version which Galileo wrote later.

Father Mersenne used such a version to publish the French translation in 1634. Another even further developed version was printed in

1649 in Ravenna by Luca Danesi. Salviati's remark applies to both of these versions because both treat in detail the inclined plane.

We can assume that these versions closely followed the *Discorso intorno alle cose che stanno in su l'acqua* because we find in them the definition of "momento" among all the other definitions. The *Mechanics*, as well as the treatise *Della Scienza Meccanica*, formulated the definition in practically the same terms as does the work on floating bodies, printed in 1612.

The moment is the tendency of a body,[45] when that tendency is not considered solely in that body but jointly with the position which the body occupies on the arm of a lever or on the arm of a balance. And its position often causes it to counterbalance a heavier weight because of its greater distance from the fulcrum of the balance. This length together with the actual weight of the heavy body gives it a greater capacity to descend, so that this tendency is composed of the absolute weight of the body and the distance from the center of the balance or from the fulcrum. Therefore, we shall henceforth refer to this composite capacity as moment, which corresponds to *ῥοπή* in Greek.[46]

In fact, the notion of moment formulated in such a way has more than one analogy to what Aristotle and his commentators call *δύναμις* (power) or *ισχύς*? (force) and what are called "virtus" or "fortitudo" by the Latin translators of the fragments on mechanics, usually attributed to Euclid. The notion of "momento" as conceived by Galileo is obviously an idea very much permeated with Peripatetic physics.

Galileo no longer merely refers to Peripatetic dynamics to justify his introduction of this notion. He develops a direct argument which seems to appeal solely to self-evident affirmations. Behind this argument, however, one can uncover a postulate which is nothing other than the axiom of Aristotle.

Let us consider, he says,[47] an arbitrarily determined resistance, an arbitrarily limited force and an arbitrarily fixed distance. One can, beyond any doubt, transport the given weight over the given distance by means of the given force, even if the given force is extremely small. It suffices to divide the weight into a great many parts so that none of the parts is larger than the force at one's disposal and then to transport the parts one by one. Thus one will ultimately move the entire weight to the designated position. From the preceding, one can correctly conclude at the end of the operation that a heavy weight has been moved and transported by a force smaller than itself. But the force, however, will have repeated its movement several times and will have traversed the space several times, which the weight under consideration, taken in its entirety, will have traversed only once. One can see by this example that the velocity of the force surpasses that of the weight by as many times as the weight surpasses the force. In effect, during the time that it took the moving force to traverse several times back and forth the distance between the two endpoints of the motion, the moving body only crossed this interval once. Consequently, it cannot be said that it is unnatural for a great resistance to be overcome by a small force. Only in the particular case where a small force would transport a great resistance in such a way that force and resistance would travel at the same velocity, could one say that the laws of nature had been transgressed. We assert that it is impossible to accomplish such a transportation with any device available now or conceivable in the future.

It sometimes happens that we have at our disposal a small force but that we need to transport a large weight in one piece, without dividing it up into smaller weights. In this case, our force must not traverse the same space as the weight, but must travel through a path which surpasses the path of the weight by as many times as the weight surpasses the force. At the end of such an operation, we shall find that the only benefit we gained by using the machine is to have been able to transport the weight in one piece with the available force over the required distance. Without a machine and under the sole condition of dividing the weight into several parts, we could have transported the same weight with the same force during the same time and over the same distance. This is where we can expect help from a mechanician, because it often happens that while we lack sufficient force but have ample time, we can still succeed in moving a great weight in one piece. But whoever hopes to produce the same effect with the help of machines without reducing the velocity of the moving body and whoever sets out to do so, will certainly be disappointed. He will prove that he has no understanding of the power of mechanical devices and of the reasons for their effects.

It is needless to say that this argumentation by Galileo is anything but rigorous. It does not follow at all from a precise dynamics, but gives, indeed, this immediate consequence: The force moving a given weight through a given space during a given time, moves a weight ten times greater through the same space during a time ten times longer. This consequence is none other than the old axiom of Aristotle deriving from a dynamics where no distinction is made between weight and mass and where velocity is assumed to be proportional to force.

The *Della Scienze Meccanica* can thus be seen to be closely related to the dynamics of the Ancients which Leonardo da Vinci and Cardan deduced from Aristotle's *Physics*, from the *On the Heavens* or from the *Mechanical Problems.* Galileo, who is regarded as the founder of a new dynamics, does not yet possess the principles which will distinguish this science from Peripatetic mechanics. These principles will, in reality, be discovered by others and he will never know them.

The equilibrium of the lever or of the Roman balance provide Galileo with a basic example of the general considerations by which he begins. He shows that if one moves a lever where the weights are in inverse ratio to the arms, the velocities of these weights are in inverse ratio to their weights, and he adds:

It is neither a miracle nor a contradiction to the laws of nature if the velocity with which the heavy body B moves compensates for the greater resistance of the weight A.

In conclusion, he observes that the role of the lever surely consists in transporting a great resistance very slowly and without dividing it, by means of a small force moving rapidly. The study of the windlass, the capstan, the pulley and the block and tackle provides him the opportunity to return to similar reflections.

As his research progresses, he, like all his predecessors, encounters the notion of moment, taken in the sense that moderns give to this term. Here are the brief terms he uses to introduce it:

The weight suspended at point D (Fig. 71) produces an impetus along the line DF. When the weight was suspended at point B, it produced an impetus along the line BH . . . Finally, one must take care to measure the distance along the line which meets at a right angle the line along which the heavy body is suspended and along which it would fall if it could move freely.

Galileo could have tied this notion of moment to his general principles. All he had to do was to note that the "momento" of a heavy body must be considered proportional to its velocity of descent and not to the total velocity of its motion. He would have discovered that the "momento" is proportional to the moment, but he made no such connection, although Cardan had done so in his *De Subtilitate*. On the other hand, he will use this principle, which had been so clearly formulated by Cardan, in his theory of the inclined plane, although this application had not occurred to the Milanese physician.

When Galileo approaches the theory of the inclined plane in his *Mechanics* and in the treatise *Della Scienza Meccanica*, it is preliminary

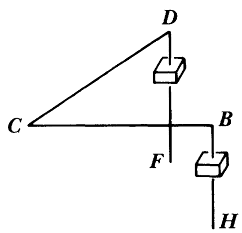


*fig. 71.*

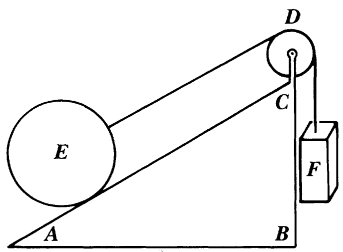


*fig. 72.*

to the study of the screw. He first repeats everything he said about the theory in his earlier treatise *De Motu*, and then in the *Della Scienza Meccanica* he adds the following passage, which is also contained in the *Mechanics*, in a slightly different form.

In conclusion, let us not leave unmentioned the following consideration: from the outset we have stated that it is necessary in every mechanical device that the more force is multipled by means of such a device, the more one loses in time and velocity. This proposition might not seem self-evident or true to someone observing its use in the present case. It might seem to him that the force here is multiplied without the motor force being required to travel over a greater distance than the moving body. Let us thus imagine that in the triangle ABC (Fig. 72) the line AB represents the horizontal plane. The line AC is the inclined plane with its height represented by the perpendicular CB. A mobile body E is placed on the plane AC and is attached to the rope EDF which is supporting at F a force or a weight which is in the same ratio to the gravity of the weight E as line BC is to line CA. If weight F starts to descend by pulling the mobile body E over the inclined plane, the mobile body E will traverse along line AC a distance equal to the one described by F in its descent. But let us note the following: it is true that the mobile body E will have covered the entire line AC in the time that F will require to descend an equal distance. However, during this time, the mobile body E will not be displaced from the common center of heavy bodies by a distance greater than the vertical BC, while the weight F, moving along the vertical, will have descended by a distance equal to the entire length of line AC. Heavy bodies only resist oblique displacements when they move away from the center of the earth. . . .[48] We can thus rightly say that the displacement of the force F has the same relation to the displacement of the force E as the length AC to the length CB, that is as weight E is to weight F.

The first edition of the *Discorsi e dimostrazioni matematiche intorno a due nuove scienze attenenti alla Meccanica ed ai movimenti locali*,[49] which has finished by Galileo in 1636 and printed by the Elseviers in 1638, contained very few innovations of interest to statics. An elegant demonstration of the law of equilibrium of the lever can be found in the First Day, although it closely resembles the demonstration given by Simon Stevin forty years earlier and published afterwards twice in Flemish, once in Latin and once in French. In this demonstration, Galileo, like Stevin, simply made use of the principle so well-known since the Middle Ages and from which the theory of the Roman balance can be deduced. Furthermore, a very similar demonstration had been known since the 13th century, as we shall see in the following chapter.

The Third Day of the *Discorsi*, dealing with local motion, contained a proposition which was essential to the development of modern dynamics. This proposition asserted the equality between velocities acquired by heavy bodies descending from the same height on differently inclined planes. In the first edition of the *Discorsi*, this equality had been assumed, but had not been demonstrated.

Under circumstances which we will discuss in Chapter XV, Galileo attempted to strengthen this proposition with solid arguments. He wrote a demonstration which he called a *Scholium* and which was added to the proposition under consideration when the complete works of the great geometer were assembled for the first time in 1655.

At the beginning of this *Scholium*, the interlocutor Salviati expresses himself in the following terms:

First of all, I shall assume that it is a well-known fact that the "momenti" or the velocities of the same moving body are different on differently inclined planes and that the greatest velocity would correspond to a descent along the vertical and that on an inclined plane, this velocity decreases the further the plane departs from the vertical. . . . In this way the impetuosity, the capability, the energy or what we shall call the "momento" of the descent decreases in this mobile body as the underlying plane on which it rests decreases.

In order to evaluate this variation of impetuosity, Salviati states that he is here referring to:

an old treatise on mechanics which was written long ago in Padua by our Academician for the exclusive use of his students,

an obvious reference to the *Della Scienza Meccanica* of Galileo. Indeed, he states that, according to this treatise, the "momento" of a heavy body descending on an inclined plane is to its "momento" in free fall as the height of the plane is to the length of the line with the greatest inclination. It is from this proposition that he deduces the desired demonstration.

Salviati concludes by asserting that:

In order to assure the equilibrium, that is to say, a state of rest between the two moving bodies under consideration, it is necessary that their "momenti," their velocities, or their propensities to motion, which is to say, the distances which they would traverse in the same time, be in inverse ratio to their gravities, according to the general law which all mechanical motions follow.

In the lengthy additions to the *Discorsi* written by Galileo but made known only after his death, the passage which we have just quoted is not the only one dealing with the inclined plane. There is another passage in the Sixth Day, *Della Forza della Percossa*. This latter passage faithfully reproduces the ideas and the terminology of a fragment from the treatise *Della Scienza Meccanica*, which we referred to above. The various passages in the *Discorsi* which we have just analyzed did not make any new contributions to the progress of statics. Their significance lies elsewhere.

If we are to believe the majority of the historians of mechanics, the *Discorsi* of Galileo completely overturned the bases of Peripatetic dynamics and established modern dynamics on an entirely new foundation. However, in these same *Discorsi*, Galileo borrows a lemma from a statics which has as its basis the axiom of Aristotle. This same lemma does not have as its goal the demonstration of some subordinate and insignificant theorem. Its object is the demonstration of a proposition which Galileo considers the "Quintessential Theorem"[50] for establishing the science of motion which he was propounding. Although the axiom of Aristotle is not explicitly stated in the reflections on the inclined plane contained in the *Discorsi*, there is nothing there to indicate that it ought to be rejected. The demonstrations in the treatise *Della Scienza Meccanica* are viewed as detailed and conclusive demonstrations:

> . . . che in un antico trattato di meccaniche scritto già in Padova dal nostro Accademico sol per uso dè suoi discepoli fu diffusamente, e concludentemente dimostrato . . .[51]

Those demonstrations are deduced from a principle which is equivalent to the axiom of Aristotle. And finally, Galileo repeats several times that the same heavy body, under different circumstances, has "momenti" proportional to the velocities at which it is moving under those same circumstances. The obvious conclusions implied in these remarks is that Galileo, considered by many historians to be in the process of creating a new dynamics, continued to base his deductions on Ancient dynamics, the dynamics professed by Aristotle and commented upon by the School and from which Leonardo da Vinci and Cardan had drawn so many important conclusions. Galileo never ceased to believe in the Peripatetic axiom which proclaimed the proportionality between force and velocity. The view which makes Galileo the founder of modern dynamics is nothing but an unfounded legend.

Furthermore, the statics of Galileo perhaps might not deserve all the praise which historians traditionally have given because much of this praise should legitimately go to the geometers preceding Galileo. There is little in his statics which is not already contained in the works of Cardan, who was influenced, in turn, by the unpublished ideas of Leonardo da Vinci. Indeed, if one tries to argue that the statics of Galileo excels that of Cardan, only one essential advance can be found: the solution to the inclined plane. However, the solution to this problem had existed since the 13th century. In considering the two demonstrations which Galileo uses to justify the solution, one is an almost direct application of the concept of positional gravity of Jordanus and of the connection established by Cardan between this notion and that of moment. The second demonstration is the most satisfying, but it is a pure and simple reproduction of the argumentation produced in the Middle Ages by the Precursor of Leonardo da Vinci.

How can we argue that Galileo did not know the work of this great but anonymous geometer? The five editions of the *Quesiti et Inventioni diverse* by Tartaglia, the collected works of the same author, the *Jordani opusculum de ponderositate* printed by Curtius Trojanus, all made it public on seven different occasions. Cardan, Guido Ubaldo and Benedetti criticized this work, which they had attributed to Jordanus. Ultimately, this work was read by the followers of Galileo. One of these followers, Bardi, writes[52] with regard to specific weight:

Gravitas de qua hic agitue ea est quam nonnulli a pondere distinguunt, Galileus vero cum Jordano gravitatem in specie appellat.[53]

This allusion to Jordanus, as Thurot[54] has correctly observed, actually refers to the short treatise on specific weights[55] erroneously attributed to Archimedes and which Curtius Trojanus appended to the *Jordani opusculum de ponderositate*, without giving the author's name. Thus this very ancient treatise was clearly known in the circle around Galileo and in certain points its statics goes beyond everything written on the same subject by the Florentine geometer.

CHAPTER XII

# SIMON STEVIN (1548–1620)

Beginning in Antiquity, the physicists who worked on problems of equilibrium approached them using two clearly distinct approaches. Aristotle, more a philosopher than a geometer, considers equilibrium as only a special case of motion. Thus for him statics is not at all an autonomous science with independent principles. It is only a branch of dynamics, and its propositions must be deduced from the general laws which determine local motion. Archimedes, more a geometer than a philosopher, applies his great genius more to the development of a rigorous sequence of propositions drawn from clear and unquestionable axioms than to a profound penetration into the nature of things.

At the time of Archimedes and perhaps even today, the study of motion is not far enough advanced to establish those propositions which everyday experience insists on so clearly that contradiction is impossible. On the other hand, one can find such propositions in the study of the science of equilibrium. And it is just such propositions which Archimedes postulates and from which he develops the hypotheses upon which he founds statics as an autonomous science.

These two distinct currents can be traced back throughout the development of statics. Sometimes progress in this science is made using the method of Aristotle, sometimes using the method of Archimedes.

The history of the evolution which we have recounted in this book is almost exclusively tied to the doctrine of the Philosopher of Stagira. The laws of statics, which were to become more precise and general through the works of Jordanus de Nemore, the Precursor of Leonardo da Vinci, Leonardo da Vinci himself, Cardan and Galileo, grew out of the ideas contained in the *Mechanical Problems*. On the other hand, one needs only to leaf through the statics[1] of Simon Stevin to recognize in the geometer from Brugge a faithful disciple of the geometer from Syracuse.

Simon Stevin was born in Brugge in 1548. For some time he was a cashier and bookkeeper in Antwerp. Later he received a position with the financial administration in the "Vrije van Brugge"[2] When he was refused an exemption from the duties on beer, he left his homeland. We know that he had already left by 1571. He visited Prussia. Poland, Sweden, and Norway and then returned to settle in the Northern Netherlands for the rest of his life. From 1581 on, he lived in Leyden, where he published his first work in 1582. On February 16, 1583 he registered as a student of letters at the University of Leyden. In 1590, he left this city for Delft and then for The Hague. His scientific reputation was considerable and he became Professor of Mathematics and later Administrator of Finance for Prince Maurice of Nassau and Inspector of Dikes and Quarter-Master General of the Army of the Estates. He died in 1620.

We have stated that the works on mechanics by Stevin are not those of a philosopher, but essentially the works of a geometer. Stevin showed a great fondness for the approach used by Archimedes, who had learned it in turn from his master, Euclid, and which was so elegant in its rigor and precision. This method is evident at the outset in the erudite arrangement of the presentation of statics by this illustrious Fleming. The definitions, the axioms, the postulates, the propositions and the examples are all arranged with minute regularity according to the place which the laws of deductive logic would assign them. Even more than Euclid and Archimedes, Stevin attempts to lay bare the framework of his reasoning so that the reader may see all the parts and their interrelations. Not only does the form of his writings show us that Stevin was a fervent disciple of Archimedes. By his own admission he assures us that he rejected completely the methods used by Aristotle and Cardan in statics.

This rejection is already apparent in the first edition of his *Statics* in the preface to the reader at the beginning of the section which deals with the application of statics.[3] It is also quite apparent in an Appendix[4] which Stevin wrote for the second edition of his *Statics*.

In confronting the numerous erroneous concepts prevailing in the statics of his time, Stevin feels within himself both the need and the ability to gain a Marathonian victory[5] over this vast army of enemies of the truth. But he prefers to condense into two propositions the very essence of all of these heresies and to refute them in two chapters.[6]

The first of these chapters is directed against the fundamental idea of the *Mechanical Problems*;

The cause of the equilibrium of the lever, as the chapter heading states, does not reside in the arcs of a circle described by its extremities.[7]

Common sense suffices to prove to us that equal weights suspended from equal arms of a lever are in equilibrium. But to say that unequal weights suspended from unequal arms of a lever are in equilibrium when these weights are in inverse ratio to the arms from which they are suspended, might not seem so evident. The Ancients believed that the reason for this resided in the arcs of the circle described by the extremities of the lever. These views can be found in the *Mechanical Problems* of Aristotle and in the works of his followers.

We shall prove the inaccuracy of this view in the following manner:

That which is motionless does not describe a circle; two weights in equilibrium are motionless: thus two weights in equilibrium do not describe a circle.[8] Therefore, there is no circle; once the circle is discarded, the cause which could reside in it disappears. The cause of equilibrium of a lever is thus not to be found in the arcs of the circle.

Let us insist upon this point so that the minor premise of our syllogism is beyond any doubt. This circular motion which we are considering here is in no way a property of the weights which are in equilibrium, but it is rather an effect of chance and is caused by the wind or by some other external impulse. Thus not only do weights in equilibrium describe circles but also any unequilibrated weights. The explanation of equilibrium does not at all reside in the arcs of the circle . . . It is not surprising then to see that those who took such errors as truth did not arrive at a true understanding of causes. And because they were totally unable to find the real foundations of statics, they departed from the truth in every possible direction, and thus were forced to contend with many false propositions.

This judgment is both harsh and completely unjustified. The Method of Virtual Displacements derived by a continuous evolution from this proposition, so haughtily rejected by Stevin. The wide applicability of this method, which seems more astonishing every day, continues to confirm the genius of the author of the *Mechanical Problems.* The disdain which Stevin manifested derives from an exclusively geometrical perspective. The eyes of the pure geometer require torrents of light, because the only truths which he perceives are those which, like glittering butterflies, spread their wings in the brilliant light of clarity. However, the ideas of the future, which will be full-blown tomorrow, are today still embryonic and exist in a semi-clarity, which appears to the bedazzled eye of the geometer as dark night swarming with hideous creatures.

The criticism which Stevin[9] levels against the rather crude dynamics taught by Cardan in the *Opus novum* is far more justified. It is easy for the geometer of Brugge to show that the dynamics of Cardan cannot explain the properties of heavy bodies falling through air or through other homogeneous media. How could dynamics explain the motion of *machines made of wood and iron which have certain parts greased with oil or lard and which are sometimes swollen by the humidity of the air or corroded by rust and when all of these different circumstances, as well* *as many others which I shall omit, sometimes promote and sometimes hinder motion.*

Statics will not consider the motion of machines:

Statics will explain exclusively[10] the circumstances under which the motor force and the weight moved are equivalent and in equilibrium. Yet each moving body always possesses certain inherent impediments to motion. It is only in theory that one can treat these impediments as abstractions. But in order to set the body in motion, it is necessary to overcome these impediments. The calculation of the force necessary to set any given weight in motion will remain outside the domain of statics. The mathematical method is, indeed, unable to determine or explain these excesses of motor power required for motion because the resistances to motion have no fixed relation to the object moved.

Can statics be constructed without taking into consideration these impediments to motion? Simon Stevin says yes. He denies that these obstacles, these resistances, can maintain bodies at rest under conditions other than those which the science of statics has determined.

Moreover, he says,[11] the analysis of the state of equilibrium is sufficient here. Indeed, if you were to place two equal weights in the two pans of a balance, even though the beam is not exempt from certain impediments to motion, the slightest effort on your part would make the balance rock. It is certain that it would be the same in all other cases.

The assertion here is obviously erroneous. Resistances of all kinds, as well as the different types of impediments to motion, determine a great many possible states of equilibrium which statics cannot possibly anticipate since it does not take into account these obstacles. Each one of the *impedimenta* is for statics a source of contradiction or cause of disagreement with the facts of reality. Nonetheless, the geometer will ignore them in his argumentation, because the laws which they obey are not sufficiently clear in his view.

Impedimentorum, inquam,[12] potentia, cum catholica non sit, a Staticae praeceptis rejicienda, quia ejus ad potentiam moventem ratio unica et certa nulla apparet.[13]

What a strange approach, one might say of a method which sacrifices accuracy for simplicity and clarity. However, what a fortunate inconsistency, which frees the human mind from the useless and hopeless contemplation of an intractable problem, bristling with difficulties and complications, so that it may attack a less formidable and more accessible problem. Once this initial problem is solved, the human mind can then by a second effort march forward in its quest for the whole truth, which seemed at first unattainable. When Galileo studied the fall of heavy bodies, he would not have created dynamics if he had not ignored the very real and efficacious resistance offered by the medium. The same thing would have happened in statics if Simon Stevin, who gave such an important boost to statics, had sought to take into account these resistances and impediments.[14]

Stevin, like Archimedes, will construct a statics independent of the science of motion. This statics will base its deductions on axioms upon which common sense confers certainty and clarity.

In any undertaking of this kind, it is easier to connect the diverse propositions in a rigorously logical order which will form the basis for any future theory, than to enumerate without omission or repetition all the axioms one needs for completeness. Euclid has provided us with an unforgettable model of such an enumeration at the beginning of his *Elements*. Educated in the School of Euclid, Archimedes was marvelously capable of disentangling almost all of the postulates which had to be formulated at the beginning of his treatises on mechanics. However, he left out some important axioms. His theory of the lever assumes, without requiring it explicitly, the existence of certain properties related to the center of gravity. Simon Stevin was perhaps less successful than his famous predecessors in a similar undertaking. Despite the great complexity in the logical order he gives to his deductions, we occasionally glimpse a half-hidden postulate which is less evident than the formally stated axioms. Girard himself observed this. In one of his demonstrations (Book I, Theorem II, Proposition VI), Stevin offhandedly writes this sentence:

> One needs to observe this general rule of statics which says that the center of gravity of a suspended body is located on the perpendicular of gravity.

Albert Girard wrote a note on this deduction of Stevin in which, along with other criticisms, one can read the following:

> One can see that Stevin does not at all prove his demonstration, because he uses a concept which he does not demonstrate here . . . Finally, he should have applied sooner the above rule on the *petitio principii*.[15]

The *Principles of Statics*[16] begins with a list of definitions and then presents a list of propositions. Among the latter, some deal with weights which pull vertically, while others deal with weights which pull obliquely. The first are governed by the theory of the lever and the second by the theory of the inclined plane.

The theory of the lever is presented in a very ingenious form. Let us imagine a straight, uniform cylinder ABCD (Fig. 73) generated by horizontal lines. Let us assume that it is suspended at its center M, which is also its center of gravity. This cylinder will obviously be in equilibrium.

At the cross section EF, we can divide this cylinder into two cylindrical segments AECF and EBFD. Their volume and, therefore, their weight are in a ratio which we can determine at will. We can then substitute for each of these weights two weights of any given shape, but equal in magnitude to the substituted weights and suspended at point K and L on the weightless line GH which is also the axis of the entire cylinder. Furthermore, points K and L are the centers of gravity of the cylindrical segments.

Thus we end up with a horizontal lever KL, in equilibrium and carrying at its extremities the weights K and L, which are proportional to GI and IH, that is to say, which are in inverse ratio to the lengths of the arms of the lever KM and ML, respectively.

It is by this very elegant method that Stevin arrives[17] at the law of equilibrium for a horizontal lever. From it he easily deduces the law of equilibrium for an oblique lever as well as various propositions concerning centers of gravity.

We shall later examine the originality of this demonstration. For the moment, we shall continue the analysis of the statics elaborated by the great geometer from Brugge and first turn our attention to the problem of the inclined plane.

Stevin arrives at the solution of this famous problem by a method so

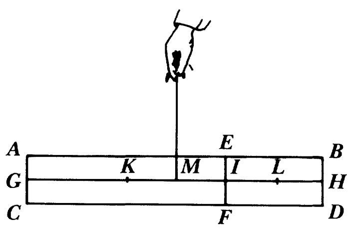


*fig. 73.*

absolutely unique that it owes nothing to the various methods used by Galileo, Descartes and Torricelli to solve it.

Heretofore, he says,[18] we have enumerated the different kinds of weights pulling vertically. Henceforth, we shall describe the properties of weights pulling obliquely. We shall take as the foundation of these properties the truth contained in the following theorem.

Theorem XI, Proposition XIX. Let there be a triangle in a vertical plane with a base parallel to the horizon. On two sides are placed two spheres[19] in equilibrium with each other.[20] The apparent weight (*sacoma*) of the sphere on the left is to the opposing apparent weight (*antisacoma*) of the sphere on the right as the length of the right side of the triangle is to the left side of the triangle. Let ABC (Fig. 74) be the triangle, Stevin adds, where side AB is twice side BC. Since the two spheres D and E have equal size and equal weight, we need to prove that the apparent weight of sphere E is double the apparent weight of sphere D.

In order to do so, let us add to these spheres twelve more identical spheres, F, G, H, I, K, L, M, N, O, P, Q, and R. Let us connect them with threads of equal length in such a way that we form a wreath on which the fourteen spheres are strung at equal distances. Let us place this wreath on our triangle in such a way that four spheres rest on the side AB and only two spheres on side BC.

If the apparent weight of the aggregate of the four spheres, D, R, Q and P, were not equal to the apparent weight of the aggregate of two spheres E and F taken together, one of these two aggregates would weigh more than the other. Let us assume that the heavier aggregate is made up of the four spheres D, R, Q and P. On the other hand, the four spheres O, M, N and L have the same weight as the four spheres G, H, I and K. Thus the segment of the wreath formed by the eight spheres D, R, Q, P, O, N, M and L

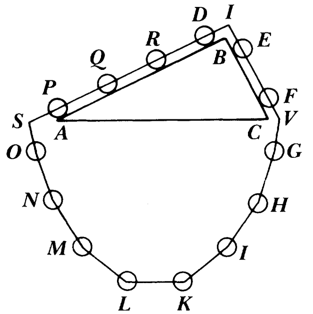


*fig. 74.*

would be heavier than the segment of the wreath formed by the six spheres E, F, G, H, I and K. Since a heavier segment will draw a lighter one, the eight spheres will descend and the six spheres will rise. Let us imagine that sphere D has descended and now occupies the former place of O, that E, F, G and H are now where P, Q, R and D used to be and that I and K are where E and F used to be. The wreath or the necklace of spheres will be in the very position it was originally, and for the same reason the eight spheres on the left will weigh more than the six spheres on the right. These eight spheres will descend again and the six on the right will rise. Thus these spheres by themselves would assume a continuous and perpetual motion, which is impossible.

The apparent weight (*sacoma*)[21] of the four spheres on the left is thus equal to the opposing apparent weight (*antisacoma*) of the two spheres on the right. And just as was previously stated, the *sacoma* of one of the spheres on the left is half of the *antisacoma* of one of the spheres on the right.

Let us assume that a body M (Fig. 75), resting on an inclined plane AB, is pulled by a rope MN, stretched in a line parallel to the inclined plane. Let this rope pass over a pulley N and let the free end carry a weight P. What magnitude must this weight P have in order to hold the body M in equilibrium? It must certainly be equal to the apparent weight, to the *sacoma* of body M. In other words, the weight P will be to the weight of the body M as length BC is to length AB.

This result can be stated in another way. Let us draw a triangle *abc* with sides *ac* and *ab* perpendicular respectively to AC and AB, and

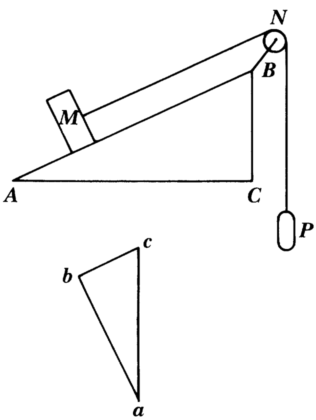


*fig. 75.*

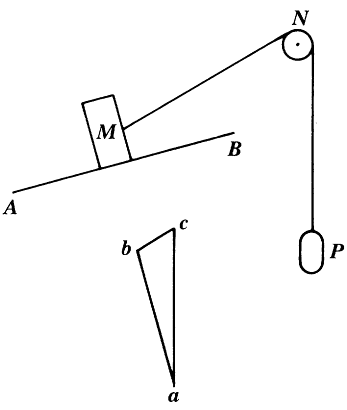


*fig. 76.*

with the third side parallel to AB. The weight P will be to weight M as the side *bc* is to the side *ac*. The geometrical construction which Stevin[22] gives here is obviously the same as we would use today to express the fact that the tension in the rope MN and the weight of the body M have a resultant perpendicular to the plane AB.

How can one determine the weight P (Fig. 76) if the rope pulling the moving body M is stretched along a line MN which is no longer parallel to the inclined plane AB? Stevin preserves the form of the preceding derivation. If one draws a triangle *abc* with the vertical *ac* and with *ab* perpendicular to AB and with the third side *bc* parallel to MN, weight P will be to weight M as side *bc* is to side *ac*. The rule which is formulated is the same as the one which we use today to indicate that the weight of the moving body and the tension of the rope have a resultant which is perpendicular to the inclined plane.

But we must admit that this generalization of the first derivation in the work of Stevin is nothing but a *petitio principii*. It does not seem to us that the discussion[23] which accompanies the statement can in any way be taken as an argument.

The geometrical derivations which we have just given are equivalent, in short, to the composition of forces. Indeed, Stevin will deduce from them the general rule for the composition of two concurrent forces: the famous law of the parallelogram of forces.

Stevin will assume that two ropes are supporting a heavy body. Then he will first demonstrate that the directions of these two ropes, situated in the same vertical plane, will converge at a point on the vertical through the center of gravity of the suspended body. He will then try to determine the tension in each of the ropes. He will construct a parallelogram with sides parallel to the two ropes and with a vertical diagonal. He will then demonstrate that the tension in each of the ropes is to the total weight of the body as the length of the corresponding side of the parallelogram is to the length of the diagonal. In this way, he will demonstrate the now famous law which gives us the resultant of two concurrent forces. From this law one can also easily deduce various consequences still considered classical today.

What intermediate steps did Stevin take to get from the theorems on the inclined plane just mentioned to the law of the parallelogram of forces? It is not possible for us to retrace those steps here because the geometrical method of the Ancients, which Stevin used exclusively, progresses by a long series of propositions connected by complex geometrical constructions. These laborious and complicated steps seem onerous to us today because we have grown accustomed to the conciseness and simplicity of modern analysis.

Stevin did not follow this laborious path of deduction from its point of departure to its conclusion. He needed two attempts in order to arrive at the law of the parallelogram of forces. When he published the first Flemish edition of his statics, he had already fully understood the first part of the statement.[24] The second part, however, was published in an appendix on the equilibrium of ropes (Spartostatica),[25] which was included in the *Hypomnemata mathematica.*

Furthermore, the detailed intricacy of the logical apparatus brought into play by Stevin does not function smoothly and we have already pointed out one source of friction. It is not the only one, and the development[26] from the equilibrium of a body on an inclined plane to the equilibrium of a body which has a fixed point seems to us rather crude.

Whatever the objections one might have to more than one of the arguments of Stevin, the laws which he set forth are accurate and answer questions which from Antiquity on had challenged mechanicians, without, however, having yielded any solution. The laws will continue to be of use to later geometers working in statics. Stevin was thus justified to look proudly on the monument he had constructed.

He was particularly proud of having solved the problem of the inclined plane, which was a kind of a crown to his entire statics. On the title page[27] of the first edition of his work, there is a figure placed in the center of a coat of arms displaying a triangle encircled by a wreath of fourteen pearls bearing in Flemish the motto:[28] "Wonder en is gheen Wonder." It was put there to remind the reader of the ingenious way by which the geometer of Brugge had so easily untied this Gordian knot.

This demonstration draws all of its strength from one principle: the impossibility of perpetual motion. Stevin uses this principle without having previously asked his reader to accept it and without having listed it among the postulates[29] at the beginning of his statics. Was this principle then so completely self-evident that this logical step was thought by Stevin to be unnecessary? At any rate, among the explicitly formulated postulates, Stevin lists the following first: Equal weights suspended from equal arms of a lever are in equilibrium. The impossibility of perpetual motion is undeniably a proposition which is much less obvious than the proposition just quoted. The latter has never been questioned, while, on the contrary, every epoch throughout history has seen inquisitive minds grappling with the problem of perpetual motion and they were not all fools by any means.

Where else could Stevin have gained this total confidence in the axiom on the impossibility of perpetual motion, if not in the argumentation of Cardan, who, in turn, had borrowed it from Leonardo da Vinci? It is true that Stevin mentions only one single work by Cardan, the *Opus novum de proportionibus*. But after having carefully read and criticised this work, how could he possibly not have known the *De Subtilitate*, which was so much in fashion during his time? And if he owes his belief in the impossibility of perpetual motion to his reading of the *De Subtilitate*, does not this belief pay indirect homage to the considerations developed in this work on the force necessary to maintain a machine in motion? Because without these considerations, which Stevin so vehemently faulted, Leonardo da Vinci and Cardan would have been unable to justify their attacks against perpetual motion.

So it happens that the very same people who pretend to understand statics as perfectly autonomous and fully independent of any reference to the laws of motion, find themselves compelled to resort more or less explicitly to the principles of dynamics.

Among the appendices to his original statics which Stevin was to include in his *Hypomnemata mathematica*, we find one[30] which deals with the equilibrium of pulleys and the block and tackle (Trochleostatics). He formulates the following brief observations[31] about these mechanisms: "Notice that in the present case the following axiom of statics can be applied:

Ut spatium agentis, ad spatium patientis; sic potentia patientis ad potentiam agentis."[32]

This is the only passage in which Stevin alludes to the concepts developed by those who had turned their attention to the statics preceding his own. Any consideration of the relation between the velocity of the force and the velocity of the resistance is carefully excluded from this reference. In this, Stevin is consistent with his previous harsh criticism of the Peripatetic formulation of the Principle of Virtual Velocities. Only the paths traversed by the force and by the resistance are taken into consideration, just as they will be systematically considered by Descartes, whose research was undoubtedly influenced by the statics of Stevin. This undeniable influence gives special significance to the brief passage just quoted because this passage marks in a certain sense a turning point in the path taken by the science of equilibrium.

Peripatetic dynamics leads most naturally to the declaration that two weights are in equilibrium when they are in inverse ratio to the virtual velocities of their points of application. This statement dominates not only the *Mechanical Problems* attributed to Aristotle, but also many works from the School of Alexandria, the *Causes* of Charistion, as well as the commentary on the *Causes* by Thâbit ibn Qurra.

A principle similar in effect to the preceding one, yet very distinct in origin, consists in positing equilibrium between two weights when the virtual descent of one is to the virtual ascent of the other as the weight of the second is to the weight of the first. This principle is implicitly admitted by Jordanus and leads him to his theory of the rectilinear lever. From this principle, the Precursor of Leonardo da Vinci cleverly deduces the theory of the bent lever and the law of the inclined plane.

In the works of the 16th-century geometers, the two principles which stem from different concepts, but which are undistinguishable when applied, are continually interchanged. Leonardo da Vinci and Cardan recognize both principles and very often it is difficult to decide if they base their arguments on one or the other. After Tartaglia presents the doctrine of Aristotle, he borrows the method invented by Jordanus and his School. Finally, Guido Ubaldo refuses to base his deductions on either one of these principles. After having made one corollary to the other, he subsequently considers them as equivalent and is always careful to formulate them together.

Galileo, whom a misguided tradition portrays as rejecting Peripatetic dynamics in order to build a new dynamics, adheres in almost all cases to the Principle of Virtual Velocities[33] as formulated by Aristotle. It is only parenthetically and rarely that he gives it the form of the Principle of Virtual Displacements. On the other hand, with his attacks on the Peripatetic principle and with his brief remarks quoted above, Stevin, the practitioner of statics, prepares the way for Descartes, who will bring to fruition the notions deriving from the School of Jordanus. Descartes will show how the Principle of Virtual Displacements saves the fecund method introduced into statics by the Peripatetics, while the dynamics of Aristotle will crumble under his blows and those of Beeckman.

Stevin does not seem to have grasped the significance of the relation between the path traversed by the force and the path traversed by the resistance. Early on, this relation had helped to undermine the claim, attributed to Archimedes, that a machine could be built which would be so powerful as to allow one man to move a weight as heavy as the Earth. Quite correctly it was pointed out that the path traversed by this resistance would be to the path described by the hand of the man as the force exerted by the man would be to this enormous weight. Even a very great displacement by this hand would only produce an infinitely small displacement of the Earth. Stevin mentions this objection which he can not contest because of its intrinsic merit, but he does not seem to have grasped its importance.

Although this displacement, he says,[34] is neither visible nor measurable, nonetheless, the possibility of producing an infinite force has been demonstrated to us and any mind can comprehend it. If its action were to continue through centuries, it would finally produce a visible displacement . . . The exclamation of Archimedes in his joy at having discovered the Charistion: "Give me a place to stand and I will move the earth," cannot be considered a statement about an impossibility or absurdity.

The passage from which we have taken this quote is interesting in more than one respect. According to Jacques Besson, Stevin asserts[35] that the Charistion so admired by Archimedes was a machine designed to haul boats into drydock. The construction of this machine was based on the application of the worm gear and it had been invented to haul a large galley which Hieron of Syracuse had built for Ptolemy, King of Egypt. The designation *Charistion* was supposed to refer to the elegant shape of that ship. In truth, it is hard to understand why this elegant comber could give its name to the galley itself rather than the instrument designed to haul the galley into drydock. In Chapter V we stated how little credit should be given to this legend in our opinion.

For the same task, Stevin preferred to the *Charistion* of Archimedes a machine which he named the *pancration* because it has greater power. This machine is none other than our modern windlass.

Stevin discusses this windlass,[36] its construction and its capabilities in such a way that the reader is led to believe that Stevin is the inventor of this machine, However, we know that its invention dates back to Hero of Alexandria. Not only does Hero describe this windlass at the beginning of his book on the *Elevator*, but Pappus, following the great mechanician, also gives us a description of this machine.[37] Pappus attributes its invention to Archimedes and tells us that the proud exclamation of the great Syracusan geometer was occasioned by this invention. It is certain, however, that Stevin did not know of the work of Hero, because the manuscript of the Arabic translation of Qustâ ibn Lûkâ, recently published by Baron Carra de Vaux, was brought to the library at Leyden by Golius (1596—1667) long after Stevin had published his statics. On the other hand, the great geometer from Brugge must surely have known the *Collections* of Pappus, because he quotes this author[38] when borrowing his definition of the center of gravity which[39] is in the same book (Book VIII) which contains the description of the windlass. Stevin, as we can see, was not particularly concerned to name his predecessors, and to mention his borrowings from them. In this, he merely followed the traditional omissions committed by all of his contemporaries. An author would only bother to quote his predecessors or rivals if it was for the purpose of attacking them. Such a custom makes work very difficult for the historian who is attempting to untangle the influences which might have suggested a novel idea to a particular geometer. Thus the historian is often reduced to conjectures.

Did Stevin know the doctrines professed in statics by the School of Jordanus? It is difficult for me to doubt this. How can we possibly believe that he had not seen the treatise published by Peter Apian as well as that of Jordanus or one of the many editions of the *Quesiti et Inventioni diverse* of Tartaglia? Surely he was familiar through one or the other of these works with the notion of positional gravity, to which his notion, called *sacoma*, corresponds so well. Was Stevin familiar with the *Mechanicorum liber* of Guido Ubaldo? He might have known it and used it in his research on statics. The *Mechanicorum liber* was published in 1577 and the Italian translation by Pigafetta came out in 1581, while the first edition of the statics of Stevin is dated 1586. It is true, nevertheless, that it is one of Stevin's great claims to fame to have accurately solved a problem which Guido Ubaldo merely posed.

Indeed, the School of Jordanus had only given consideration to positional gravity, i.e., to the component of the weight along the trajectory which a moving body follows. Guido Ubaldo insisted on the necessity of taking into consideration the component of the weight along the perpendicular to this trajectory. But he was certainly not aware of the procedure by which to determine these two components.

Stevin states the law by which the weight is divided between these two rectangular components and we cannot deny him the priority of this discovery.[40] Doubtlessly, Leonardo da Vinci had possessed momentarily a correct understanding of this law, according to which a weight is divided into two given directions. However, he rejected this law almost immediately after he had grasped it and no one seems to have found it in his notes or made it public.

If Stevin succeeded in correctly dividing a weight into two rectangular forces, this was due to the solution of the problem of the inclined plane. Despite the unique and ingenious method by which Stevin was able to solve this problem, we cannot forget that the School of Jordanus had solved it just as accurately before him nor can we doubt that Stevin was familiar with the research of his predecessors.

By the time Stevin published the first edition of his statics, the *Questiti et Inventioni diverse* of Nicolo Tartaglia had already gone through five editions and the latest one was already thirty-two years old. Twenty-one years earlier, Curtius Trojanus had printed the *Jordani opusculum de ponderositate*. How could Stevin not have known the splendid theory on the inclined plane contained in the work of the Precursor of Leonardo da Vinci?

There can be no doubt that the admirable work accomplished in statics by the great geometer from Brugge was in several instances favorably influenced by the ideas expressed from the 13th century on by Jordanus de Nemore and the mechanicians of his School.

There is one more discovery which had been made before Stevin, but of which he was perhaps unaware. The geometers of the School of Alexandria had attempted to deduce the law of the lever from the demonstration of the following proposition: A heavy cylinder attached to the arm of a lever in such a way that the generators are parallel to the lever, is equivalent to an equal weight suspended by a rope and attached to the center of the cylinder. This theorem played an essential part in the four propositions attributed to Euclid in the *Liber Charastonis* published by Thâbit ibn Qurra, and in the *De canonio*, and it also formed the conclusion of the *Elementa Jordani de ponderibus*.[41]

By reversing, in a certain sense, the demonstration utilized up to his time, Stevin conceded the accuracy of this proposition and he very elegantly deduced from it the proof for the law of the equilibrium of the lever. After Stevin, Galileo gives a similar deduction in the First Day of his *Discorsi*. Perhaps as early as Antiquity, but certainly from the 13th century on, it was known that the law of the equilibrium of the lever could be justified in this manner.

One of the numerous manuscript collections [42] kept at the Bibliothèque Nationale contains an important fragment written in elegant Gothic script and bearing the undeniable mark of the 13th century. This fragment contains a very accurate version of the treatise on specific weights attributed to Archimedes. This treatise, as we indicated earlier, appears to be related to the *De ponderoso et levi* attributed to Euclid and to derive from the School of Alexandria, just as does the *De ponderoso*.

Following this treatise on specific weights are several disparate propositions which might have had the same origin as the treatise quoted above. The objective of the first of these propositions is to establish geometrically the equality which modern algebra will express in the form:

$$(a - c)b = (a - b)c + (b - c)a$$

Immediately following this proposition is a unique and elegant demonstration of the law of the lever, which we shall briefly summarize. Two equal weights, whatever their form, are in principle assumed to be in equilibrium if they hang from the extremities of lever arms of equal length.

Two equal weights are suspended from the two points $a$ and $b$ equidistant from the point of support $c$ (Fig. 77). One of the weights $f$, suspended at $a$, has no specified form while the second weight is a cylinder $eg$ whose generators are horizontal. The center of this cylinder is situated on the vertical rope extending from point $b$. This cylinder is long enough for its extremity $g$ to pass beyond the vertical of the point of support.

Let us lift this cylinder until it becomes contiguous with the lever and

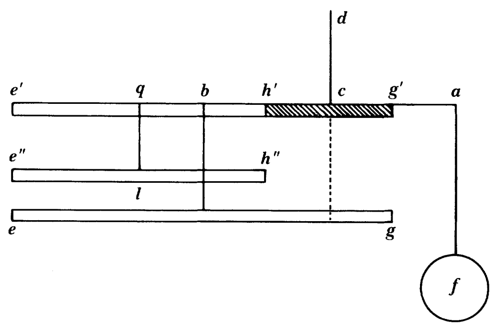


*fig. 77.*

fix it in the position $e'g'$ of this lever. According to the postulate from which we will derive our demonstration, the lever will remain in equilibrium.

But it is obvious that we will not disturb this equilibrium if we remove from our cylinder the segment $cg'$ beyond the vertical line at the point of support as well as an equal segment $ch'$ on the other side of the same vertical. In this way, the cylinder $e'h'$ coinciding with the lever is in equilibrium with the weight $F$, suspended at $a$.

According to our postulate, the cylinder $e'h'$ can, in turn, be replaced by an equal cylinder $e''h''$ suspended from a rope attached to the beam at $q$, i.e., at the midpoint of $e'h'$.

If we designate by $l$ the weight of the cylinder $e'h'$ or of the cylinder $e''h''$, we shall easily demonstrate that we have the following equation:

$$\frac{l}{f} = \frac{ca}{cq}$$

which is the law of the equilibrium of a lever.

Was Stevin acquainted with this demonstration? It is impossible for us to answer this question. Whatever the case may be, several conclusions seem beyond any doubt.

The first conclusion is that Stevin was influenced by his predecessors much more often and more deeply than his very rare references would lead us to believe. The second conclusion is that the ideas planted in him through the works of other geometers germinated and flourished magnificently through his meditations and often far exceeded the seed from which they sprang. In particular, it is the idea of resolving a force into two components, which had only been surmised by the School of Jordanus and by Guido Ubaldo, that furnished Stevin with the theorems which we use today and for which Stevin found so many applications. Before Stevin, only Leonardo da Vinci had had an equally clear understanding of the law of the composition of forces, but he failed to recognize his discovery and no geometer, it seems, was able to exhume it from his notes.

Finally, the third conclusion can be stated as follows: Despite the complexity and apparent rigor of the logic which Stevin puts to use in each of his demonstrations, he is still far from establishing a conclusive proof for the law according to which he combined concurrent forces. After his death, this law still needed the geometer who would establish it in an entirely convincing way. That will be the task of Roberval.

CHAPTER XIII

# THE FRENCH CONTRIBUTION TO STATICS – ROBERVAL

## 1. SALOMON DE CAUS. THE EARLY WORKS OF F. MERSENNE. *THE COURSE ON MATHEMATICS* BY PIERRE HÉRIGONE

Around the turn of the 16th century, the study of statics flourished in the Low Countries with Stevin and in Italy with Galileo. But during the first third of the 17th century no important work concerning this branch of science was printed in French.

French readers wishing to learn about statics and hydrostatics could only consult[1] *Les Livres de Hierome Cardanus, médecin milanois, intitulés de la Subtilité et subtiles inventions, traduis de latin en françois par Richard Le Blanc.* Although slightly dated, this work continued to be useful.

In 1615, the Norman, Salomon de Caux or de Caus (1576–1630) published a work[2] which is of importance to the history of the steam engine, as Arago pointed out. In this book[3] only one modern author is quoted as having written on mechanics and that author is Cardan. The concepts of hydrostatics and statics which precede the description of the machines invented or perfected by Salomon de Caus are all borrowed from Cardan. All Salomon de Caus did was to restate neatly and orderly what appeared in disorder in the bizarre work of the geometer-astrologer.

Since Salomon de Caus was primarily an engineer, he pays almost exclusive attention to the law of equality between the motor work and the resisting work in the statics of Cardan. It is a law for which Cardan himself was probably indebted to Leonardo da Vinci.

In a lever, for instance, the weights which are in equilibrium with each other are in inverse ratio to the arcs described by their points of application in a virtual displacement.

If certain men would consider this demonstration more carefully,[4] they would no longer delude themselves in trying to construct various machines with which they hope to lift heavy burdens with a small force. Although this quite possible, as shall be demonstrated, the small force must, additionally, travel a longer distance, as we have demonstrated above. And I will not demonstrate here that these displacements must occur in the same time.

The same observations are made about the lever and pulley:[5]

Thus, if one pulls twenty feet of rope, the weight will only rise by ten feet. With this machine one man will lift the same weight that two men would lift with a simple machine. However, the two men would lift during the same time double the length, that is to say, twenty feet before one man would have lifted ten. And if there were two pulleys in the block and tackle, the force would be fourfold, but the weight would only rise five feet for every twenty feet of rope pulled in.

Gears[6] exist for the same reason, because by augmenting the force, one augments the time proportionally.

Salomon de Caus then describes a machine where two axles C and E of the same diameter have respectively a cogwheel of six teeth and a gear wheel of forty-eight teeth, all equal in size and meshing with one another.

The above-mentioned cogwheel must do eight revolutions to one for the large wheel so that a pound weight suspended from axle C will be equally balanced by an eight pound weight suspended from axle E, assuming that both axles are of the same size. Thus, if one wishes to lift a four hundred pound weight with axle E, it will only require as much work as a fifty pound weight using axle C . . . so that one man lifting a load using this machine would produce as much work as eight men would produce if each of them had axle C to work with. But the eight men would lift their weight in one hour, while one man alone would lift his weight in eight hours.

This is without a doubt the first time that the word *work* is used in French with the meaning which it will have in contemporary mechanics.

The pinion gear and the wine press give Salomon de Caus occasion to observe the equality which connects the motor work to the resisting work in every machine. This law is borrowed from Cardan as well as the examples used by the famous astrologer.

The year 1634 is a milestone in the history of French statics. It was in that year that three books were published which revealed to the mechanicians of our country the discoveries in mechanics which had been made abroad.

Indeed, it is in 1634 that the Elseviers published in Leyden the *Mathematical Works* of Simon Stevin in an edition translated, amended and enlarged by Albert Girard. Furthermore, it was in 1634 that Mersenne published the *Mechanics* of Galileo with the publisher Henry Guenon is Paris. Finally, Pierre Hérigone had his *Cours Mathématique* published in Paris in 1634.

The simultaneous publication of these different works undoubtedly initiated a powerful movement which directed the attention of the French geometers to the laws governing the equilibrium of weights. Inspired by these problems, these geometers produced remarkable works which perfected and completed the solutions of their predecessors. Thus the French School of Statics was born. The first two eminent practioners of this School — Roberval and Descartes — were also arch rivals.

The books published in 1634 by Girard, Mersenne and Hérigone disclose to us the sources which will nourish this current of French thought. Mersenne added various supplements to his translation of the *Mechanics* by Galileo:

which will be as gratifying as the rest,[7] because they contain novel speculations which can serve to penetrate the secrets of physics and, in particular, everything concerning natural as well as violent motion.

In these supplements, Mersenne borrows most often from the *Mechanicorum liber* of Guido Ubaldo and does not conceal his admiration for this treatise:

Those wishing to study mechanics[8] alone should first read the entire eighth book of Pappus, in which he explains various kinds of instruments, and then read the books of Guido Ubaldo, who discussed the nature of such instruments better than any one else.

The first supplement is devoted to explaining the concept of moment, and the way it is presented reminds us very much of the format used by Giovanbattista Benedetti. It would not be surprising if Mersenne had borrowed it from him, because in a different work[9] Mersenne adds the following remarks to his argumentation after having used the same concept of moment as Benedetti:

As does Jean Benoist in the 3rd Chapter on mechanics.[10]

Pappus, Guido Ubaldo and Benedetti are not the only ones to have influenced Mersenne's supplements to the *Mechanics* of Galileo. In Supplement X, which concludes the treatise, he gives[11] the procedure to determine the pressure exerted by a weight on an inclined plane:

If one wants to determine the force exerted by weight F on the plane BC, one needs to take the base of the triangle AC and compare it with the hypotenuse BC, the more so, since the entire force of weight F is to the weight exerted on the plane BC as BC is to AC.

This theorem is one of the most important propositions demonstrated by Stevin. Mersenne knew the *Hypomnemata mathematica* before Girard had translated them. We shall have to return to Supplement X, when we discuss the work of Roberval.

In one of the first works of this indefatigable member of the Order of the Minims, we can find proof that he knew the work of Simon Stevin before Girard had published it in translation.

The *Mechanics* of Galileo is preceded by a dedicatory letter to Mr. de Reffuge, Counselor of the King to Parliament. This letter begins as follows:

Eight years ago I presented to you the books on mechanics written in Latin. . .

Indeed, in 1626 Mersenne had published a series of short treatises with the title *Synopsis mathematica*,[12] and each of these treatises contained a collection of propositions which had been taken from ancient and modern authors and had been reproduced without any figures or demonstrations.

According to Nicéron,[13] one of the treatises was entitled: *Euclides elementorum libri* and another one: *Theodosi, Menelai et Maurolyci sphaerica et cosmographica*.[14] These two treatises are lacking in the copy of this very rare work, which was sent to us by the Bibliothèque Municipal of Bordeaux. However, that copy contains only three treatises, each having its own special pagination. One of these treatises contains all of the propositions which are to be found in the works of Aristotle. The second has all of the propositions demonstrated by Apollonius on the subject of conics and by Serenus on the subject of conic sections and cylinders. The third treatise, finally, entitled *Mechanicorum libri*, is the same one which Mersenne mentions in his letter addressed to Mr. de Reffuge.

The preface, which bears the mark of Peripatetic influence, asserts that almost all of the theorems of mechanics can be reduced to the following axiom:

Rotunda machina est moventissima, et quo major, eo moventior.[15]

To which Mersenne adds:

Quo ad illam divinam sphaeram spe erigamur, cujus centrum ubique, circumferentia nullibi esse dicitur; et quae tempus ab aevo ire jubet, stabilisque manens dat cuncta moveri.[16]

Pascal,[17] who is no longer a Peripatetic but a Cartesian, will not call God, but rather the universe

. . . this infinite sphere with a center which is everywhere and a circumference which is nowhere.

These three books on mechanics probably offer us a complete summary of what the Frenchman who was the best informed on scientific developments outside of France knew of statics in the year 1626.

The first book is entitled: *De Gravitatis et Universi centro* and is composed of four parts, several of which will be of great interest to our study in Chapter XV. The first part is often inspired by Guido Ubaldo. The second is composed of propositions taken from the book of Commandino on the centers of gravity of solids. The third book reproduces a series of theorems by Luca Valerio on that same topic. Finally, the work of J. B. Villalpand on Jerusalem and its temple – we shall talk about this in Chapter XV – provides the definitions of the fourth part for mechnicians who continue to reproduce these formulations until the end of the 17th century.

The third book, *De hydrostaticis et iis quae ad aquam pertinent*,[18] is borrowed in its entirety from Stevin. We shall now turn our attention to the second book, which is dedicated, as Mersenne informs us in his preface, to a restatement of many propositions which had been demonstrated by Guido Ubaldo and Stevin.

Indeed, Stevin and Guido Ubaldo furnished most of the theorems on the balance and the lever contained in part one, on the laws of the pulley and the block and tackle related in part four, as well as on the theory of other machines stated in part five. The third part, permeated with the most obscure and confused parts of Peripatetic statics, deals with *Des applications utiles et merveilleuses du cercle aux Méchaniques*.[19]

The second part deserves our attention for a moment. It is entitled: *De ponderibus obliquis et de viribus vectis, et librae et aliarum machinarum ad ea reductarum, ubi et de navigatione et de Quaestionibus mechanicis Aristotelis*.[20] The conclusion of the second part reproduces almost in its entirety the *Mechanical Problems* of Aristotle, but everything preceding this is borrowed from Stevin.

We do not have here a complete list of the propositions demonstrated by Stevin on the inclined plane and the composition of forces. The theorems which Stevin included in the *Supplement to Statics* are not mentioned at all either because Mersenne did not yet know about them, or because he considered them to be insufficiently established.

Mersenne, indeed, tells us that the theory of oblique weights was far from being universally accepted by 1626.

Heretofore, he says,[21] hardly anything has been demonstrated concerning weights which rise or descend obliquely. Thus we shall for the moment only state those propositions upon which the great majority of geometers agree.

The first of these propositions deals with apparent weight on an inclined plane. After this proposition, Mersenne makes the following reflections,[22] which show us that the demonstration given by Stevin was far from satisfying all mechanicians:

Stevin proves this proposition by showing that if it were not true, perpetual motion would exist, which he considers to be absurd. But some maintain that he erred in this just like Pappus. . . . They think that the falsity of this proposition as well as the error by Pappus can be clearly demonstrated.

Further on, Mersenne[23] writes the following lines:

But all of this seems to rest on the axiom which I touched upon above. The velocity of descent of one of the weights is to the velocity of descent of the second weight as the length of one of the sides of the triangle[24] is to the length of the other. Indeed, two descents are equal when they correspond to the same diminution in distance to the center. The more oblique the side of the triangle, or, to say it differently, the more oblique the plane, the longer this side and the slower the descent of a heavy body along this side and hence, the slower its approach toward the center of the universe.

It is impossible to misunderstand the intent of this passage. The demonstration of the law of the inclined plane which is sketched out here is the same one which Galilieo gives in his *Della Scienza Meccanica*, which Mersenne was to translate in 1634.

Should we conclude, therefore, that Mersenne was in possession of a manuscript of the treatise by Galileo as early as 1626? There is every indication that we should reject this interpretation. Not only is the name of Galileo not mentioned in the *Synopsis*, but, apart from the passage just quoted, none of the remaining propositions bear any trace of the great Florentine geometer. Finally, at the beginning of the translation of the *Mechanics* of Galileo which he publishes in 1634, Mersenne writes to Mr. de Reffuge:

After having presented to you eight years ago the books on *Mechanics* written in Latin, I now make known this new treatise by Galileo which sheds new light on this science. . .

This sentence seems to indicate that Mersenne did not come to know the *Della Scienza Meccanica* until after the publication of the *Synopsis.*

We must, therefore, conclude that Mersenne through his own reflections developed the theory of the inclined plane which Galileo developed independently. The construction of this demonstration was hardly difficult. It was enough to take the argument of the Precursor of Leonardo da Vinci, which Tartaglia had published in the *Quesiti et Inventione diversi* and which Curtius Trojanus had included in the *Jordani opusculum de ponderositate*, and to substitute for it the velocities of the distances traversed. Such a substitution was familiar to the readers of Guido Ubaldo, and Mersenne was more than capable of discovering on his own the demonstration for which Galileo is so excessively praised.

Thus in the eyes of the French geometers, the argumentation by Simon Stevin had not entirely superceded the older and solid reasoning constructed by the School of Jordanus. We shall have further proof of this when we study the *Cours mathématique* of Pierre Hérigone.

We know very little about this mathematician. Only one episode in the career of this geometer is known. Hérigone was a member of a commission in charge of examining the method proposed by Morin to measure longitudes at sea. On March 30th, 1634, the commission rejected the method of Morin and this decision prompted the publication[25] of the *Lettres escrites au Sr Morin par les plus célèbres astronomes de France approuvans son invention des longitudes, contre la dernière sentence rendue sur ce subject par les sieurs Pascal, Mydorge, Beaugrand, Boulanger et Hérigone, commissaires députez pour en juger.*[26]

In 1634, Pierre Hérigone published a complete course on mathematics in five volumes.[27] This course was written in Latin and French. Furthermore, the demonstrations were set forth by means of abbreviations and symbols thanks to which, according to the author, they could

. . . easily be understood without making use of any language.

The notation used by Hérigone scarcely bears any resemblance to the algebraic notation in use today. Thus, where we use the three symbols $=$, $>$, $<$, Hérigone wrote 2/2, 3/2 and 2/3.

This course, which has fallen into oblivion in our time, enjoyed a certain popularity at that time. On February 26th, 1639, Debeaune wrote to Mersenne:[28]

As far as Mr. de Beaugrand is concerned, I must confess that I have learned much from the geometry of Mr. Descartes and what I knew before, I had learned from the algebra of Hérigone.

After having been augmented by two supplements, the work of Pierre Hérigone had to be published again in Paris in 1644 by Simon Piget.

The part of this work which interests us here is the third volume of the *Cours mathématique*, contenant la construction des tables des sinus, et logarithmes, avec leur usage aux intérests, et en la mesure des triangles rectilignes; la géométrie practique; les fortifications; la milice; et les méchaniques.[29]

Although the name of the author is not mentioned for the part of the course entitled: *Mechanica — Les Méchaniques*, it is easy to recognize the various influences on Pierre Hérigone when he wrote this chapter.

First of all, the influence of Guido Ubaldo is evident. The *Mechanicorum liber* must have been constantly used by Hérigone, since the Latin text of proposition VI of the *Mechanica* reproduces, without changing a single syllable, the text of the eighth proposition concerning the lever by the Marquis del Monte. The various problems of the balance related to propositions III and IV are also borrowed from the treatise by the Marquis del Monte.

Hérigone does not seem to know at all the discoveries of Galileo, because nowhere does the *Cours mathématique* reflect a single thought of the illustrious Florentine. On the other hand, the French geometer borrows heavily from the statics of Stevin, as the following analysis will demonstrate.

This analysis will show us a third source of influence on the science of Hérigone. The statics of the School of Jordanus had not remained unknown to him. The demonstration of the law of the lever devised by Jordanus de Nemore and the demonstration of the law of the inclined plane constructed by the Precursor of Leonardo both came down to Hérigone, who was able to make the most of them. How did he know of these demonstrations? Was it by studying the *Quesiti et Inventioni diverse* of Tartaglia or by reading the *Jordani de ponderositate* edited by Curtius Trojanus? Or could it have been by examining directly an ancient manuscript? We will perhaps be able to respond to these questions later in a more persuasive fashion.

The point of departure for the statics of Pierre Hérigone is the law of the equilibrium of the lever which he obtains through the elegant demonstration proposed by Stevin.[30]

The proposition which sets forth this law is immediately followed by this assertion:[31]

With two weights in equilibrium, the heavier weight is to the lighter as the displacement of the lighter is to the displacement of the heavier and as the perpendicular to the motion of the lighter weight is to the perpendicular to the motion of the heavier weight.

Upon this last remark Jordanus had based the law of the equilibrium of the lever, but Hérigone does not do the same. On the contrary, from the law of the lever established in the manner of Stevin, Hérigone deduces as a consequence what Jordanus had made a principle. The intuition which motivates him to present his ideas in this order is quite evident. He wanted to justify, for a particular case, the proposition which he will later make into a general hypothesis:

With two weights in equilibrium, the heavier one is to the lighter as the perpendicular to the motion of the lighter is to the perpendicular to the motion of the heavier.

Leonardo da Vinci had formulated very precisely this hypothesis several times, but he used it together with the hypothesis of Aristotle, according to which the relation between the virtual velocities discloses the relationship between the weights in equilibrium. Cardan made even less distinction between these two hypotheses, and Guido Ubaldo, who reduced them to the status of corollaries, always considered them of equal value. Stevin, who had rejected the hypothesis of Aristotle, reduced the principle of Jordanus to nothing more than a brief remark found at the end of the theory on the block and tackle, while Galileo returned to the Peripatetic statement concerning velocities almost to the exclusion of everything else. Thus for the first time since the Middle Ages, the principle of Jordanus was reaffirmed in full vigor.

It is not that Hérigone omitted any allusion to the relation between virtual velocities, but he reduced this allusion to a very concise corollary, which he subordinates to the preceding proposition and formulates in the following terms:

Corollary: From which it appears that the time of the motion of a weight is all the longer, the easier the weight can be moved and all the shorter, the harder it is to move the weight and vice versa.

When Hérigone wants to know the ratio between the applied force and the resistance in a simple device, he compares the distances traversed and not the velocities, as the following quotes show:[32]

*On the endless screw.* The ratio between force and weight can also be found in this instrument by calculating the displacements which force and load make in the same time. *On the multiplication of the force of the agent by means of toothed wheels.* With wheels as well as with other instruments, the weight is to the force supporting it, as the displacement of the force is to the displacement of the weight.

The most important application which Hérigone was to make of the principle of Jordanus is the demonstration of the law of the inclined plane. This demonstration[33] follows:

If a straight line drawn from the apex of a triangle to its base is perpendicular to the horizon, the weights which have the same ratio to each other as the sides of the triangle on which they are supported will be in equilibrium.

Because in the same time that weight G (Fig. 78) descends from point C to point B, the weight D rises from point A to point E and, consequently, BC will be the vertical height of weight G and EF of weight D. Therefore, since D is to G as the vertical BC is to the vertical EF, the weights D and G are in equilibrium by reason of their positions.

This deduction is essentially the same as the one created by the great unknown mechanician who belonged to the School of Jordanus and whom we have called the Precursor of Leonardo da Vinci. The influence of this medieval geometer on Hérigone is here quite apparent even down to the wording, such as:

The weights D and G will be in equilibrium by reason of their position — *erunt situ aequilibria.*[34]

This reminds us of the positional gravity dealt with in 13th-century statics.

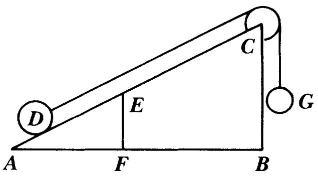


*fig. 78.*

The most prolific principles of the School of Jordanus must have reached Hérigone and, most likely, also the various other French geometers of his time, so that we may number the demonstrations by Jordanus and the Precursor of Leonardo da Vinci among the sources which contributed to the progress of statics in France. Furthermore, the *Cours* of Hérigone greatly contributed to the diffusion of ideas deriving from these sources.

Hérigone not only has recourse to the simple and rigorous demonstration of the Precursor of Leonardo da Vinci to justify the law of the inclined plane, but he also knows the ingenious demonstration of Stevin, which he states in his own way:

*Another demonstration of proposition eight.* If the weights which are proportional to the sides of a triangle were not in equilibrium, a perpetual motion around the triangle would ensue, which is absurd in view of the fact that nature does nothing which does not come to an end. Therefore, the weights proportional to the sides of a triangle are in equilibrium.

That perpetual motion would ensue around a triangle if the weights proportional to the sides of the triangle were not in equilibrium, can be shown in the following way: Let us imagine that BCAEB (Fig. 79) is a tube of uniform diameter and filled completely with water or some other substance so that nothing can move inside the tube. Because AB is assumed parallel to the horizon, the water in the tube AEB will be in equilibrium and the weight of the water in the tube CB will be to the weight of the water in the tube AC as the length of the tube AC is to the length of the tube CB, because water is a homogeneous liquid and because we assumed the tube to be of uniform diameter.

Now if one assumes that the force of descent of the water along one of the sides — side AC, for example — is greater than the force of descent of water along the other side CB, the water of the tube AC will descend while the water of the tube BC will move into its place. In this way, the tube AC will always be filled with water and will always have a greater force of descent than the water in tube CB. Consequently, the motion will be continuous, which is absurd. Therefore, since a perpetual motion of the water

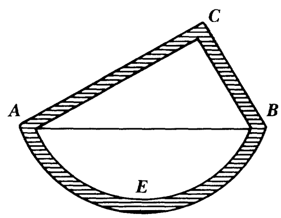


*fig. 79.*

inside the tube is impossible, it is necessary that the force of descent of the water in tube AC be equal to the force of descent of the water in tube CB. This is what was to be demonstrated.

For the wreath of spheres devised by Stevin, Hérigone substituted a column of liquid of uniform diameter. This change is exasperating. One could just as easily have supposed that the two tubes AC and BC are of different diameters, because the equilibrium of the liquid would be maintained at any rate. If the demonstration by Hérigone were conclusive, one could prove that any two given weights are held in equilibrium on two planes of any given inclination.

Further evidence of Hérigone's ignorance of the laws of hydrostatics can be found in the brief tract entitled *Les principes ou axiomes des spiritales*, which concerns hydrostatics and can be found at the end of his *Méchaniques*. He was unable to gain from Stevin's work an accurate understanding of the properties of fluids.

Most assuredly, it was not by reading the hydrostatics of Stevin that Hérigone was inspired to modify the theory of the inclined plane formulated by the great geometer of Brugge, a rather clumsily made modification in any case. Thus we can assume with a high degree of probability that this modification was suggested to him by an author of the 13th century.

We have seen that Hérigone surely knew the treatise on mechanics written by the Precursor of Leonardo da Vinci during that century. One of the texts,[35] copied in the 13th century, which brought this treatise to our attention contains an interesting feature. At the bottom of the page where the author concludes the first part of the treatise with a beautiful solution to the problem of the inclined plane, which Hérigone reproduces here, an annotator of the 13th century added the following statement:

Note that the following consequence derives necessarily from the last proposition of this section: Let us take two tubes of the same size and similar in all other respects, join them in such a manner that they form an angle, fill them with water and put one of the extremities into water in such a way that the two extremities are at equal distance from the horizontal plane. The water will remain in equilibrium and will not descend. If one slightly lowers the extremity of the tube which does not rest in the water, the water will start to flow on the side. From this it follows that in such devices the water can not move either to a position higher than its original position or to a place of the same height; it must necessarily be lower.

If one compares the theory of the syphon to the solution of the inclined plane given by Stevin, one immediately has the demonstration devised by Hérigone. Thus it is entirely possible that Hérigone read the passage we have just quoted.

One becomes even more convinced after examining his few texts on hydrostatics. Among the *Principes ou axiomes des spiritales*, one finds the following statement bearing the number III:

The water in a tube which has a longer vertical exerts more pressure than the water in a tube with a shorter vertical.

Further on, in the *Conséquences*, we read:

From the third axiom it follows that if ABC (Fig. 80) is a syphon filled with water and if one extremity A rests in the water of the vessel DF, while the other extremity C is lower than extremity A, all of the water in vessel DF which is higher than the extremity A, will flow out of the syphon ABC.

The insight Hérigone has on the syphon is obviously the same as those demonstrated by our 13th century annotator.

When writing his *Cours mathématique*, Hérigone borrowed heavily from the mechanicians of the School of Jordanus. He contributed greatly to the diffusion of this fecund principle among the geometers of the 17th century. It was undoubtedly mainly through him that this principle came to the attention of Descartes, who made it the basis for his entire statics.

Hérigone takes from Stevin the various propositions which remain to be discussed. The first is a corollary from the theory of the inclined plane. What is the force exerted on the plane by the weight placed on the plane? The correct answer[36] is given by Stevin:

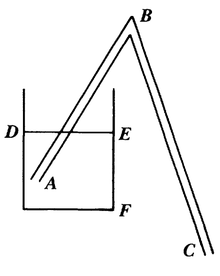


*fig. 80.*

It appears that the weight of body D (Fig. 81) is to the force exerted on the plane AC by the body as length AC is to length AF.

Stevin had not furnished a convincing demonstration using this correct proposition. Will Hérigone be more fortunate? He joins to the inclined plane AC a second inclined plane BC which is perpendicular to the first, and then says:

Since the body D presses against the side AC as much as the body G pulls on the rope CG; and since the body E weighs as much as the body G pulls on rope CG, the truth of the corollary is obvious.

This does not even remotely resemble a demonstration.

One can hold the weight D on the plane AC (Fig. 82) by a force exerted along the line DL, parallel to the plane. But one can also exert a force Q along the line DP which forms angle PDL above DL. Which law enables us to determine the weights Q and H? Stevin had formulated this law precisely without, however, having been able to establish it with satisfactory argumentation. Hérigone postulates it[37] purely and simply. He draws the inclined plane BC which forms with the vertical CE an angle BCE equal to the angles PDL and LDI and assumes that the weights Q and H are both equal to the weight G, which can be put in equilibrium with weight D by positioning it on the plane BC. Stevin had shown from these propositions on the inclined plane how one can deduce the law according to which two concurrent forces can be composed. Hérigone also states this law.[38]

The *Cours mathématique* of Hérigone certainly made a great contribution by publishing the most important discoveries made by Stevin in physics. Thus we find the name of Hérigone associated with Stevin by both Borelli,[39] who attacks the law of the composition of forces

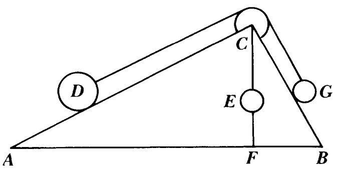


*fig. 81.*

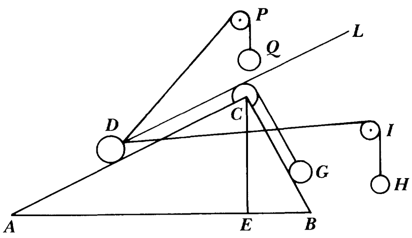


*fig. 82.*

formulated by the geometer of Brugge, and by Varignon, who defends the law.[40]

However, Hérigone did not add anything to what Stevin had demonstrated. He did not fill the gaps in the deductions of his illustrious predecessor. It is Roberval who will fill them.

## 2. GILLES PERSONE DE ROBERVAL (1602–1675)

Only once in his life did Roberval print a book devoted exclusively to one of his own works and, even then, he did not dare to openly claim its authorship, but he pretended that it was the publication of an ancient treatise written by Aristarchus of Samos and that he was himself only the editor and annotator.[41] To find his works on statics, we must look for them among the texts of Father Mersenne.

Marin Mersenne (1588–1648) is one of the most bizarre figures of the first half of the 17th century. After having been a fellow student of Descartes at the Collége de la Fléche, he entered the religious Order of the Minims. Blessed with an inexhaustible energy and a passionate love of science, he corresponded incessantly with all the French geometers and physicians of his time. In the intellectual world of this time, such correspondence played the same role as scientific publications do today. Such correspondence insured a continual flow and exchange of ideas between the capital and the provinces and a constant debate over discoveries and controversies among scholars in Paris. Compare the correspondence of Etienne and Blaise Pascal, Beaugrand, Roberval with Desargues of Lyon, with Fermat, Counselor to the Parliament of Toulouse, with Jean Rey, Physician in Bugue in the Perigord, and with Descartes in his self-imposed and proud exile deep in Holland. Mersenne welcomes the most diverse research, particularly that dealing with physics and mechanics, in order to disseminate it in his numerous books, most of which were devoted to acoustics and music. Not only did he make public the discoveries of his compatriots Fermat and Roberval as well as his own, but he also reported on many of the works from abroad. Furthermore, he contributed greatly to keeping France informed about the progress accomplished in statics, hydrostatics, and dynamics by Simon Stevin, Giovanbattista Benedetti, Guido Ubaldo, Villalpand and Galileo. Through Father Mersenne, Blaise Pascal came to know of the experiment with quicksilver carried out by Torricelli.

As early as 1627, Father Mersenne had published[42] a *Traité d'Harmonie universelle, où est contenuë la musique théorique et pratique des anciens et modernes.*[43] In 1634, he wrote[44] the *Préludes de l'Harmonie universelle, ou questions curieuses, utiles aux prédicateurs, aux théologiens, aux astrologues, aux médecins et aux philosophes*[45] at the same time he was publishing the translation of the *Mechanics* of Galileo.

In Paris in the year 1636, the publisher Guillaume Baudry came out with the *F. Marini Mersenni, ordinis Minim., Harmonicorum libri.*[46] The second part was entitled: *Harmonicorum instrumentorum libri IV.*[47] These two volumes, now adorned with a new dedication and preface and bound together with a new title page, were for sale again in 1648 as an *edito aucta*[48] with the title *Harmonicorum libri XII.*

Previously, however, the *Harmonicorum libri*, translated into French and enlarged by several supplements, had been published as a large treatise and the first volume came out in Paris in 1636 with the title *Harmonie universelle*, while the second volume was printed in 1637 with the title *Seconde partie de l'Harmonie universelle.*[49]

Inserted into the first part of the *Harmonie universelle*[50] and with its own pagination, we find the *Traité de Méchanique; des poids soustenus par des puissances sur les plans inclinez à l'horizon; des puissances qui soutiennent un poids suspendu à deux chordes; par G. Pers. de Roberval, Professeur royal ès Mathématiques au Collège de Maistre Gervais, et en la chairs de Ramus au Collège Royal de France.*[51]

The only authors cited by Roberval in this short treatise are Archimedes, Guido Ubaldo and Luca Valerio. However, he hardly borrowed anything from any of them. Yet, in keeping with the annoying custom of his time, he was careful not to mention any authors who had provided him with inspiration. However, we can easily supply the missing names.

To begin with, we know for certain that Roberval was thoroughly familiar with the statics of Simon Stevin because his own *Traité de Méchanique* reads like a supplementary text to this major work of Stevin. Its sole objective is to establish convincing proof for the propositions which the geometer of Brugge had formulated without sufficient demonstration.

Secondly, Roberval was well acquainted with the methods which Galileo had used in his *Mechanics*. The procedure by which he justifies the propositions which Stevin had been unable to deduce from his principles, closely resembles the procedure used by Galileo to reduce the problem of the inclined plane to the problem of the lever.

Finally, Roberval's introduction of the concept of moment is similar to the approach used by Giovanbattista Benedetti. Roberval undoubtedly read this author, whom Mersenne was studying and quoting at about the same time.

The short treatise by Roberval — only thirty-six pages long — is a striking example of the false rigor to which geometers enamored of the geometrical method often succumb. Frequently, a superabundance of axioms or a complicated and erudite display of deduction merely serves to cover up certain essential hypotheses. And quite often these hypotheses consist more or less in alleging what actually ought to be proven.

Thus Roberval alleges that it does not matter as far as the equilibrium of a lever with equal arms CAB (Fig. 83) is concerned whether the two weights E and D are fixed at B and C, whether the weight D, held by a rope which passes over the arm of the lever AC, passes over a small pulley at A and carries a second weight K, or whether this rope extended beyond B is attached to a hook I or whether this weight C (sic) is resting on an inclined plane perpendicular to AC. Roberval could have spared the reader many absolutely useless preliminaries by asserting this latter supposition at the outset, as Galileo had done. Indeed, it easily provides the solutions to the problems which he set out to examine. By reducing the numerous axioms to a single postulate, he could have asserted that two independent constraints applied to the same weight are equivalent when the virtual trajectory which one of them traces along with the weight is tangential to the virtual path which the other imposes on the weight. Roberval makes use of some of the consequences provided by this postulate, just as Leonardo da Vinci and

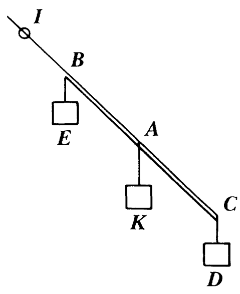


*fig. 83.*

Galileo had done before him. But he leaves it to Descartes to formulate this principle explicitly and in a general form.

If one asserts that it does not matter whether a weight D is forced to move on an inclined plane AB (Fig. 84) or whether it is attached to the extremity D of the arm of the lever CD, which is perpendicular to AB and mobile around point C, it becomes quite easy to resolve the two problems, which Roberval formulates in the following way:[52]

*Proposition I.* Given a plane inclined to the horizon with a known angle of inclination, determine the force which, either by pulling or pushing along a line parallel to the inclined plane, can maintain a given weight on the same plane.
*Proposition II.* When the line by which a weight is maintained on an inclined plane is not parallel to that plane and with the inclination and magnitude of the weight given, determine the force.

All one needs to do in order to solve these problems is to apply the general law of equilibrium of a *circonvolubile* — a method Benedetti undoubtedly borrowed from Leonardo da Vinci — and to state that the vertical weight D has the same moment with respect to the point C as force Q, directed along DP. This is, indeed, the solution which Roberval gives, but not without useless digressions.

To the two preceding problems one can easily reduce a third problem, which Roberval now states[53] in the following manner:

*Proposition III.* Given a weight supported by two ropes or by two supports of known position, determine the force developed in each rope or each support.

Roberval deals with the problem of the decomposition of forces using the following procedures:

The rope AB (Fig. 85) is fastened to a fixture at A. What force

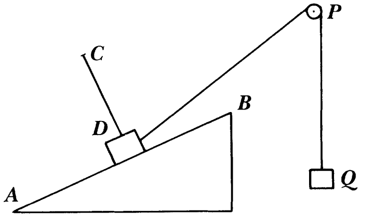


*fig. 84.*

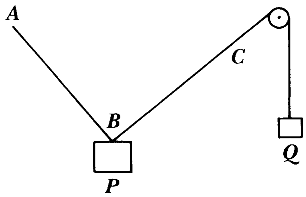


*fig. 85.*

Q must be exerted on the rope C in order to keep the weight P in equilibrium?

In accordance with the axioms formulated by Roberval on the equivalence of constraints, one can imagine that the weight P slides on an inclined plane perpendicular to AB, instead of assuming that it is held in place by the rope AB. The solution sought can then be deduced immediately from the axioms previously given.

This solution implies several consequences, which Roberval formulates[54] in the following terms:

*Corollary.* It can be noted that in each case one can draw two perpendiculars from a point on the line of action of one of the forces. The first perpendicular is drawn to the line of action of the weight and the other to the line of action of the other force. Furthermore, it can be observed that the ratios between the weight and the forces are equivalent to the ratios of the lengths of the perpendiculars dropped to the lines of action of the forces, and the forces are equivalent to the lengths of the perpendiculars dropped to the line of direction of the weight. . .

*Scholium II.* In this second scholium we shall give a demonstration of a general nature showing that regardless of the arrangement of the weights and forces which are supported by two ropes, provided that the ropes are not in a straight line, the weight and the two forces are always analogous to the three sides of a triangle. . . . If from any point taken on the line of action of the weight a line is drawn parallel to one of the ropes and intersects the other rope, the triangle thus formed by this parallel, the line of action of the weight and the other rope, will be similar to the triangle described above and, consequently, will be analogous to the weights and the forces. A geometer will be able to prove this easily by using several other properties which we shall not mention here.

Here we find clearly stated and demonstrated the laws for the composition of forces which Stevin had formulated, but which he had been unable to support with convincing demonstrations. Roberval established his proof by reducing the equilibrium of a weight supported by ropes to the equilibrium of a weight sliding on an inclined plane and by subsequently reducing the latter to the equilibrium of a weight suspended at the extremity of the arm of a lever. He could have saved himself an unnecessary intermediate step by avoiding the consideration of the inclined plane altogether and by immediately reducing the equilibrium of a weight suspended by ropes to the equilibrium of a weight suspended at the extremity of the arm of a lever. Such a demonstration would have been more direct and would have shown great similarity to the demonstration proposed by Leonardo da Vinci.[55]

If we consider only the essence of these two demonstrations, that of

Roberval and that of Leonardo da Vinci are identical. The latter has the advantage of being more direct and of avoiding completely useless digressions.

Leonardo, as we have seen, had unfortunately abandoned the law on the composition of forces, for which he had given such an ingenious demonstration. It took the successive efforts of Stevin and Roberval to reestablish the truth which Leonardo had allowed to excape after having grasped it momentarily.

Roberval not only gives the demonstration of the composition of forces which we just analyzed. He also provided a second demonstration. Because of the intrinsic importance of this new proof and the inaccessibility of the book where it appears, we feel obliged to report in its entirety what our geometer has to say about it:[56]

*Scholium VIII.* We have observed something about a weight suspended from two ropes which pleased us very much. This is because when the weight is suspended by two forces as described and the ratio is as we demonstrated in the third proposition, the weight can neither ascend nor descend unless the reciprocal proportion between the paths of the weights and the forces changes, which is against the natural order of things. And furthermore, let the weight be placed at A (Fig. 86) on the ropes CA and QA, supported by the forces C and Q or K and E, and let the weight be to the forces as the perpendiculars CB and QG to the lines CF and QD, as we said in Proposition III.

If one takes a line such as AP below the weight A and in its line of action, it will follow that if weight A descends to P by pulling the ropes with it and causing the forces K and E to ascend, there will be a reciprocally greater ratio between the path traversed[57] by the forces ascending to the path followed by the weight descending than

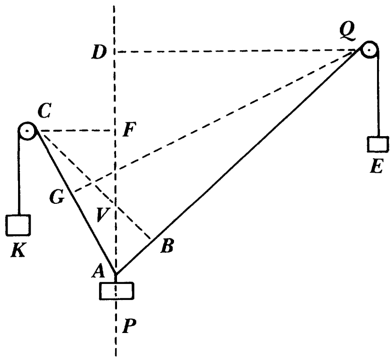


*fig. 86.*

between the same weight and the two forces taken together. Thus the forces would ascend further to the extent that the weight descends while pulling them, which is contrary to the natural order of things.

If one takes a line such as AP above the weight A in its line of action and if the weight moves up to V and the ropes move also, pulled by the forces K and E which are descending, there will be a reciprocally greater ratio between the path traversed by the ascending weight and the path traversed by the descending forces than between the forces taken together and the weight. Thus the weight would ascend further to the degree that the forces descend while pulling it with them, which is contrary to the natural order of things, where the weight or the force which pulls the other always traverses a longer path in proportion to the weight or the force which is being pulled.

One will find in our mechanics the demonstration that the ratios between the paths traversed by weight A and its forces when ascending and descending are as we just described and contrary to the natural order of things, but this demonstration is too long to include here. Consequently, the weight A, by remaining in its place, by reason of Proposition III, thus remains within the natural order of things, which is what we wanted to demonstrate.

This demonstration of the law according to which two forces are composed is deduced by comparing the work of the applied forces and the work of the resistance, to employ the term by which modern mechanics designates the product of weight by the height of its fall.

We have seen that this comparison served to justify certain laws of statics as early as the 13th century. Jordanus de Nemore deduced from it the demonstration of the condition of the equilibrium of the lever, already known for such a long time. His successor, the Precursor of Leonardo da Vinci, makes use of this comparison to obtain the first satisfying solution to the problem of the inclined plane.

In both cases, the comparison between the work of an applied force and the work of the resistance leads to a very simple result. Whatever the virtual displacement imposed on the mechanism under study, there is equality between the motor work and the resisting work when the conditions for equilibrium are fulfilled. This very simple relation depends on another property inherent in these mechanisms: their equilibrium is an indifferent equilibrium.

When a mechanism in stable equilibrium is considered, the comparison between the motor work and the resisting work which occurs in a virtual displacement no longer leads to a result as simple as before. There is no longer equality between these two quantities of work, or, at least, this equality only exists between infinitely small quantities of work which correspond to an *elementary* virtual displacement.

The geometers whose works we are studying — the students of

Jordanus as well as Roberval — consider only finite displacements. Thus their analysis becomes somewhat complicated when they attempt to determine the conditions for stable equilibrium in a mechanism. They must show that in each displacement of this mechanism, the work of the weights ascending is greater in absolute value than the work of the weights descending.

The Precursor of Leonardo da Vinci had given a very elegant example of this method when he established the law of equilibrium for two weights suspended at the extremities of the arms of a bent lever. In the passage just quoted, Roberval finds a second application for this method, which is in no way inferior to the first.

Did Roberval know of the use which had been made of this same method of demonstration as early as the 13th century? We cannot give a definite answer to this question. Nothing prevents us from assuming that he did not know of the demonstration of the law of equilibrium of a bent lever which the Precursor of Leonardo da Vinci had formulated. Indeed, this demonstration is nowhere to be found in the *Quesiti et Inventioni diverse* by Tartaglia. It is reproduced in only one printed work, the *Jordani opusculum de ponderositate*, published by Curtius Trojanus, where it is so muddled and confusing that a reader cannot be blamed for failing to recognize it.

On the other hand, we saw that Hérigone had in his possession a manuscript which contained the treatise of the Precursor of Leonardo da Vinci, and it is not improbable that it was known by Roberval.

The passage which we quoted summarizes the demonstration by Roberval but does not give it in its entirety. Roberval tells us that the complete demonstration can be found in his *Méchaniques*. Form this reference and from a similar reference inserted in the statement of Proposition III, we can only conclude that the *Traité de Méchanique* which Roberval inserted in 1636 in the *Harmonie universelle* by Mersenne is merely an excerpt from a larger treatise which he published earlier.

Furthermore, in the first part of the *Harmonie universelle* which includes the *Traité de Méchanique* by Roberval, Mersenne studies[58] the laws of the accelerated fall of heavy bodies in accordance with the *Dialogue* of Galileo *on the Chief Systems of the World*. Proposition X of the theory which he sets forth is formulated as follows: With the plane being inclined to the horizon by a given angle, determine the force needed to hold the given weight on the plane. The demonstration given by Mersenne is precisely the same as the one which Roberval was to state later in the same volume. The figures used are the same. After the statement which we have just quoted, we find the following observation by Mersenne:

I would not have put this proposition here if it had been in French and if the booklet containing it had been widely known, although it deserves wider dissemination because of the great use one can make of it.

This observation confirms for us that the demonstrations on mechanics by Roberval had already been published before the *Harmonie universelle* went to press. However, we also learn that this publication had appeared in Latin and that the book containing it was already quite rare by 1634.

This last circumstance explains why we were unable to find any reference to this work in the various bibliographies which we had at our disposal or in the various library catalogues which we consulted.

But we can affirm that this treatise on mechanics by Roberval existed as early as 1634 and that Mersenne knew of it by then. Indeed, at the same time, Mersenne published the *Mechanics* of Galileo. In Supplement X, which concludes this work, Mersenne deals with the apparent weight on an inclined plane, about which he says:

I discussed this in detail in the tenth and eleventh theorem of the second book of the *Harmonie universelle.*

The formulation of the demonstrations of Roberval was, therefore, at that time in the same form it was to have in 1636 in the *Harmonie universelle*. Furthermore,[59] we know that Mersenne was working on this work as early as 1634.

The first draft of the *Discorsi e dimostrazioni matematiche intorno a due nuove scienze*, written by Galileo in 1636 and printed by the Elseviers in 1638, contained only Three Days. The final three days were added by Galileo between 1636 and the time of his death. They were only included in the 1655 edition of the works of Galileo, published by Viviani. These additions could, therefore, have been influenced by the *Traité de Méchanique* of Roberval. The passage ending the Fourth Day can probably be traced to this influence.

Galileo — or Sagredo who speaks for him — considers a weightless rope AB (Fig. 87) from which are suspended the two heavy and equal weights C and D. He wants to prove that by suspending a weight H,

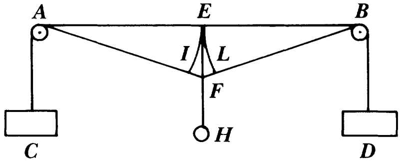


*fig. 87.*

however small, at the middle of the rope AB, the rope will assume the shape of a broken line AFB and, consequently, will lift the two weights C and D, however large they may be.

The weight H does indeed descend by the length EF, while the weights C and D ascend by lengths equal to IF and FL, respectively, and equal to each other. One can make EF small enough so that the ratio between EF and IF is larger than the ratio between the weight H and the weight C.

The proportion is greater between the fall or the velocity of weight H and the ascension or the velocity of weights C and D than that between the gravity of weights C and D and the gravity of weight H. Thus it is obvious that the weight H will descend and that the rope will be displaced from its horizontal position.

Even assuming that Galileo knew of the *Traité de Méchanique* by Roberval when writing this passage, he is still far from equalling the beautiful demonstration in the latter's treatise.

The article on Roberval in the *Dictionnaire historique et critique* by Bayle contains only this brief remark:

Roberval, Professor of Mathematics in Paris, a contemporary of Mr. Descartes and his great enemy.

The animosity was indeed fierce between the philosopher and the Professor at the Collège de France.[60] The former treated the latter with contempt and brutality, as will be made evident in the next chapter. For the moment, let us quote the following excerpt from a letter addressed by Descartes[61] to Mersenne:

I am including some of the mistakes which I found in the *Aristarchus* and I shall tell you, just between us, that I have so many proofs of the mediocrity of the knowledge and mind of its author that I could not be more amazed that he has acquired any reputation in Paris. Besides his invention of the caster wheel, which is so simple that it could have been invented by an infinite number of others just as well as by him, if they had wanted to, I have never found anything of his making which did not prove his inadequacy.

The harshness of such a judgment diminishes Descartes in our eyes more than Roberval. Even if Roberval only had to his credit the *Traité de Méchanique* — and he can claim many other titles — his name would deserve to be remembered, for he demonstrated in his treatise, in two different ways, the law of the composition of concurrent forces. No one else prior to him, including Simon Stevin, had published a convincing proof of that law, which so many mechanicians after him used so frequently.

The disdain of Descartes for Roberval was, therefore, supremely unjust. It is true, indeed, that Roberval could find some solace for this by reading the exaggerated compliments addressed to him by Mersenne, who declared[62] that his friend was "almost the equal of Archimedes."

CHAPTER XIV

# THE FRENCH CONTRIBUTION TO STATICS (CONTINUED) RENÉ DESCARTES (1596–1650)

On September 8th, 1637, outside of Breda, Constantine Huygens, father of the great geometer, Christian Huygens, wrote to Descartes:[1]

I don't intend to leave you alone, "donec paria mecum feceris",[2] by favoring me with a treatise of three pages on the subject of the foundation of mechanics and the demonstrations on the four or five machines which accompany it, "libra", "vectis", "trochleon",[3] etc. I have seen in the past what Guido Ubaldo wrote about this and I read Galileo later in the translation by Father Mersenne, but neither one of them wrote to my satisfaction. It seems to me that these people have merely enveloped in superficial obscurity something which I am sure you could grasp in two or three statements because there is nothing to my mind which should hold together more clearly and logically.

On October 5th, 1637, Descartes answered[4] this insistent request of Constantine Huygens:

Concerning what you asked for in mechanics, I have never been less inclined than now to write about it.

However, he enclosed in his letter a brief treatise entitled: *Explication des engins par l'ayde desquels on peut, avec une petite force, lever un fardeau fort pesant.*[5] In this treatise, the theory of the pulley, the inclined plane, the wedge, the wheel or the lathe, the screw and the lever are all deduced from a single principle. This principle is as follows: The work (Descartes says force) which is necessary to lift various weights to given heights keeps the same value when the product of the weight and its ascent remains the same. Here are the terms used by Descartes to formulate it:

The construction of all these machines rests on a single principle, which states that the same force[6] which can lift a weight of one hundred pounds, for instance, to a height of two feet, can also lift a weight of two hundred pounds to a height of one foot or a weight of four hundred pounds to a height of half a foot and so on, as long as the force applied remains the same.

And this principle must be accepted if one considers that the effect must always be proportional to the action necessary to produce it. Thus, if it is necessary to employ an action by which one can lift a weight of one hundred poinds to the height of two feet, another weight could weigh two hundred pounds and the same action would lift it to a height of only one foot. For it is the same to lift one hundred pounds to the height of one foot and then lift one hundred pounds again to the height of one foot or to lift one hundred pounds to the height of two feet.

The machines which serve to apply this force acting over a great distance to a weight which it is lifting by a smaller weight, are the pulley, the inclined plane, the wedge, the lathe, or the wheel, the lever and others. Because if one does not want to classify them together, one can count more of them and if one wants to classify them together, one does not need to have as many.

Constantine Huygens received with obvious and ardent admiration the short treatise on statics which Descartes had sent him.

I pray God, he said,[7] to inspire you to continue to make your works public to the world because they are obviously destined to rid the word of a universal deluge of errors and ignorance. Moreover, sir, I foresee that people will solicit me on all sides, because I shall be unable to keep to myself the precious object from your hand.

An opportunity arose which caused Descartes to write a kind of second edition to the treatise first sent to Constantine Huygens. A book by Jean Beaugrand, which we will deal with later, had caught the attention of geometers regarding the following problem: Does the weight of a body vary with its distance from the earth?

On July 13th, 1638, Descartes wrote to Mersenne[8] in order to discuss the question whether a body weighs more or less depending on whether it is near or far from the center of the earth. In this letter, Descartes takes up again in new terms the statement of the principle he had discussed with Huygens:

And the proof of this depends on a single principle, which is the foundation of all of statics and which states that one needs no more and no less force to lift a heavy body to a given height which is all the greater, the lighter the body, or to lift a heavier body to a height which is all the smaller. Since, for instance, an amount of force which can lift a weight of one hundred pounds to the height of two feet can lift a weight of two hundred pounds to the height of one foot, or a weight of fifty pounds to the height of four feet and so on, as long as one applies it.

One will readily agree with me, if one considers that the effect must always be proportional to the action which is necessary to produce it and, therefore, if it is necessary to use the force by which one can lift a weight of one hundred pounds to the height of two feet, to lift a weight to a height of only one foot means that this weight weighs two hundred pounds.

Because it is the same to lift one hundred pounds to the height of one foot and again another one hundred pounds to the height of one foot, as to lift two hundred pounds to the height of one foot or to lift one hundred pounds to the height of two feet.

This principle immediately accounts for the relation which exists between the apparent weight of a heavy body sliding over an inclined plane and its gravitational weight. Meditations on the arguments which Galileo developed in his *Mechanics* on the inclined plane undoubtedly inspired in Descartes his general principle. At least one may make that assumption after comparing the two following passages. The first, which we have already quoted, is in the treatise *Della Scienza Meccanica.* Let us reproduce it here from Father Mersenne's translation,[9] which had recently appeared and which had been surely sent to Descartes by the translator.

Since F (Fig. 88) will not cover less distance in descending vertically than weight E ascending obliquely, it is necessary that F descend further than it causes weight E to ascend, but weight E can ascend no further than the vertical BC. So that the line of descent of F will equal CA when it has caused weight E to move vertically from B to C. Because the weight does not resist motion parallel to the horizon and because this motion does not take it away from the center of the earth, it is of great importance to consider the directions along which the motion takes place, especially when they are occasioned by inanimate forces which have large moments and resistances in a line perpendicular to the horizon. However, they descrease proportionally as the line inclines towards the horizontal plane.

The second passage can be found in a letter written by Descartes to Mersenne on July 13th, 1638. It is a continuation of the previous quote:

And from this it obviously follows that the relative weight of each body, or what amounts to the same thing, the force needed to support it and prevent it from descending when it is in a given position, must be measured at the onset of the motion. The force which is supporting the body must lift it as well as follow it when it descends. So that the proportion between the straight line which this motion would describe and that

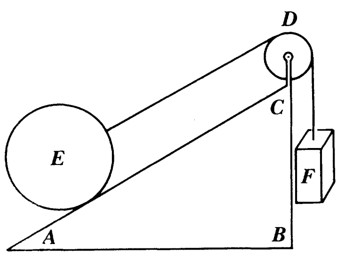


*fig. 88.*

which would indicate by how much this body would move towards the center of the earth is equal to the proportion between the absolute and the relative weight.

One can notice only a single difference between these two passages. Galileo, who had established the theory of the inclined plane from a different approach, makes the equality between the motor work and the resisting work the object of a sort of a corollary, while Descartes considers this equality to be the very cause of the equilibrium between a weight sliding over an inclined plane and a weight hanging vertically. Descartes writes to Mersenne[10] on November 15th, 1638:

What Galileo has written on the balance and the lever explains quite well *quod ita sit*, but not *cur ita sit*,[11] as I do with my principle.

This undoubtedly fixed in his own mind the comparison which we have just made. Obviously, it is on this very point that the thoughts of Descartes and Galileo merge.

The juncture is so apparent and the influence of Galileo so obvious that we cannot read without amazement the following lines which Descartes wrote[12] to Mersenne on October 11th, 1638:

First of all, as far as Galileo is concerned, I shall tell you that I have never seen him nor ever communicated with him in any way and that, consequently, I cannot have borrowed anything from him. I also see nothing in his books which make me envious nor hardly anything for which I would like to claim credit.

Descartes' boundless arrogance blinded him to the point of not being able to recognize the worth of any of his predecessors. We shall see the kind of haughty insolence with which Descartes rejected the claim of priority for Roberval.

Stevin concluded his theory on the block and tackle by formulating the following: The force is to the resistance as the path described by the resistance is to the path described by the force. Descartes first applies this principle to the block and tackle in his *Explication des engins* addressed to Huygens as well as in the copy sent to Mersenne. However, Descartes does not allude to Stevin, and it was not because he did not know the work of the great geometer from Brugge because on the same day — July 13th, 1638 — when Descartes sent Mersenne his statics, he wrote:[13]

I want to tell you that just recently I took a look — quite by chance — at the statics of Stevin and found the center of gravity of a parabolic conoid discussed there.

This corollary, which is only formulated once by Stevin, is repeated *ad nauseam* by Guido Ubaldo on the subject of every type of block and tackle. Although Descartes does not cite Guido Ubaldo, he knows nonetheless what this geometer had said about the assemblage of pulleys because he writes[14] to a mathematician, who was perhaps Boswell:

It seems to me foolish to see a lever in a screw; if memory serves me right, that is just the fiction which Guido Ubaldo uses.

But if there ever existed a geometer who, long before Descartes, dealt with the problem of the inclined plane in precisely the same manner which the great French philosopher was to use, it was certainly that anonymous mechanician of the 13th century whom we have called the Precursor of Leonardo da Vinci. At the time Descartes was writing his mechanics, the solution proposed by this geometer had already appeared in print seven times. It can be found in the five consecutive editions of the *Quesiti et Inventioni diverse* by Nicolo Tartaglia, in the collection of the *Opere* by the same author, and in the *Jordani opusculum de ponderositate*, edited by Curtius Trojanus. How can we believe that the philosopher never leafed through any of these works, or that the great algebraist never took a look at the work which contained the first solution to the equation of the third degree or that the clear and profound reasoning of the medieval mechanician never caught his eye to influence profoundly his way of dealing with statics? Yet, neither the name of Jordanus nor the name of Tartaglia can be found in his treatise on mechanics. Stevin and Galileo, we have to admit, were equally unjust.

Even if we suppose that Descartes did not know of any of the works where Tartaglia had published the doctrine from the School of Jordanus, can we possibly believe that he did not know the *Cours mathématique* by Hérigone? In 1634, the commission charged with examining the astronomical methods of Morin put Hérigone in contact with Etienne Pascal, Mydorge and Beaugrand, all three geometers in regular communication with Mersenne. It was Clerselier, who, in the name of the King, granted the printing license for the *Cours mathématique* on December 29th, 1633. Can we believe that neither Mersenne nor Clerselier thought of getting a copy of this work to Descartes? Therefore, Descartes must have known about the work and he must have found in it, formally stated and applied to the lever and the inclined plane, the very principle which he was to use as the foundation for his statics. Hérigone, in turn, had taken this principle from the School of Jordanus.

The influence of Galileo can easily be seen in the solution which Descartes gave to the problem of the inclined plane. It is even more obvious in what he has to say about the lever. In this case, as in the case of the inclined plane, his formulation is in a certain sense the formulation of Galileo taken in reverse order.

If Descartes had treated the force and the resistance of a lever in equilibrium as two weights suspended from that lever, the demonstration of the well-known condition of equilibrium would have posed no problem for him, since Jordanus de Nemore had long before deduced this demonstration from the very principle on which Descartes based his entire statics. In a letter undoubtedly addressed to Boswell,[15] Descartes gives a kind of summary of this demonstration by Jordanus. But he considers the equilibrium of the lever in a totally different way in both his *Explication des engins* sent to Constantine Huygens and in the statics addressed to Mersenne. Resistance is always a weight suspended from a lever, but the force is constantly perpendicular to the arm of the lever, as is the case when the force applied to the arm lifts a burden by means of the lever. It is in this way that Guido Ubaldo dealt with the lever. Then the problem becomes more complex and this is why Descartes declares the following in the *Explication des engins* addressed to Huygens:[16]

> I have deferred my discussion of the lever because, of all devices for lifting burdens, it is the most difficult to explain.
>
> Let us consider that while the force which moves this lever describes the entire semi-circle ABCDE (Fig. 89) and acts along the arc ABCDE, although the weight also describes the semi-circle FGHIK, it does not move upward by the length of this curved line FGHIK, but only by the length of the straight line FOK. Thus the proportion between the force necessary to move this weight to its gravitational weight must not be measured by the ratio between the two diameters of these circles, or between their two circumferences, but rather by the ratio between the semi-circle of the larger circle and the diameter of the smaller circle.[17]

This passage, characterized by such profound insights into the work of a force of variable direction, only deals with a kind of average relationship between the force and the weight to be lifted. Indeed, the force holding a given weight in equilibrium varies with the inclination of the lever:

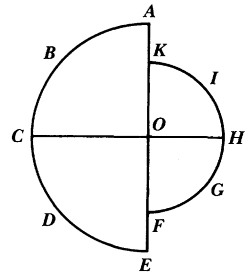


*fig. 89.*

Let us consider, furthermore, that it is far from being true that this force needs to be greater to turn the lever when it is at position A or E, than when it is at position B or D . . . , that is the reason the weight ascends less there, as can be easily seen . . .

And in order to measure precisely what this force must be at each point of the curved line ABCDE, we need to know that it acts everywhere the same as if it were pulling weight on a circularly inclined plane, and that the inclination of each of the points of this circular plane must be measured by the slope of the straight line which is tangent to the circle at this point.

Galileo had taken for granted in all of his works that it is all the same whether a weight is compelled to move on an oblique line or on a circle tangent to this line. This postulate had allowed him to deduce the theory of the inclined plane from the notion of the moment of a weight. The analysis which had led Roberval to prove the law of the parallelogram of forces was also based on this implicitly accepted postulate. When Descartes reverses the order followed by Galileo, he deduces the theory of the equilibrium of the lever from the law of the inclined plane, and he, too, does so by invoking this identical postulate. But instead of hiding it behind the complicated obscurity of a feigned rigor, as Roberval had done, Descartes attempts to establish it with complete clarity.

When a displacement is imposed on a machine held in equilibrium by two weights, one of the weights will ascend and the other descend. The work performed by the motor weight equals the work suffered by the resisting weight. However, this equality does not hold for just any displacement — great or small — imposed on the mechanism, but is only true, in a general way, for an infinitely small displacement from the position of equilibrium. No predecessor of Descartes had ever clearly understood this essential restriction, or at least none of them had formulated it explicitly. Descartes formulates it clearly.

The relative weight of each body, he writes to Mersenne, must be measured at the beginning of the motion which the force which is supporting the body must traverse in order to lift it as well as to follow it when it descends.

He adds:[18]

Note that I say *begins to descend* and not just *descend*, because it is the beginning of the descent to which one must pay attention.

A heavy body forced to move on a curved surface and tangent to that surface at one point could thus be considered as if it were sliding on a plane tangent to this surface at that point:

So that if, for example, this weight F (Fig. 90) were not supported at point D by a flat surface — as we suppose ADC to be — but were supported on a spherical or any curved plane — as EDG, and as long as the flat surface which we imagined tangent at point D is the same as ADC, then in so far as the force H is concerned, the heavy body would not weigh more or less than if it were supported on the plane AC. Although the motion described by this body ascending or descending from point D toward E or G on the curved surface EDG would be entirely different than the motion described on a flat surface ADC, the body positioned at point D on EDG would nevertheless be compelled to move towards the same side as if it were on ADC, that is to say, towards A or C. It is obvious that the change which occurs to this motion as soon as it ceases to touch the point D, cannot change anything as far as its gravitational weight is concerned while it is touching it.

The predecessors of Descartes had used this principle. Leonardo da Vinci had deduced from it the law of the composition of forces. Thanks to this principle, Galileo was able to reduce the theory of the

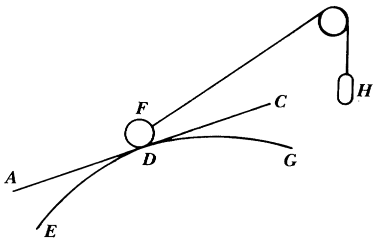


*fig. 90.*

inclined plane to the theory of the lever, and Roberval, also using this very same principle, was able to prove the propositions which had been inadequately demonstrated by Stevin. Yet none of these authors had formulated in an explicit and general manner the postulate supporting all of their demonstrations.

Descartes was, therefore, the first to clearly affirm the infinitesimal property of the Principle of Virtual Displacements.[19] At the very moment when Descartes was sending Huygens the *Explication des engins par l'ayde desquels on peut avec une petite force lever un fardeau fort pesant,* he was publishing the *Discours de la Méthode.* When he wrote his mechanics, he must assuredly have had in mind the rules which he stated in the *Discourse.* By formulating the principle from which he deduced all of statics, he certainly intended to comply with the first of the precepts which he had stated and which enjoined him:

> To never accept anything as true that he did not know to be obviously so; that is to say . . . to never include anything in his judgement which did not appear so clear to his mind that he would have no occasion to doubt it.

Descartes thought that this perfect clarity, this absolute certainty, was also present in his principle of statics, which seemed to him to be as certain as the truths of arithmetic.

> The same quantity of force[20] necessary to lift this weight to a height of one foot is not sufficient *eodem numero*[21] to lift it to the height of two feet, and the fact that one needs double the quantity of force is as clear as two and two make four.

However, this principle was not immediately accepted by all who knew it. Some, and among them not the least famous, like Mersenne or Desargues, found it to contain obscurities.

These obscurities were due above all to a misunderstanding. Descartes was talking about the force necessary to lift a weight to a certain height. Several of his readers understood the word force in the same sense we take it today. Descartes, on the other hand, meant by force a scalar magnitude representing the product of the weight and the distance by which it ascends or descends. In other words, he meant by force what we today would call work, and he was surprised, even irritated at times, when he saw that this confusion could baffle geometers and prevent them from accepting his principle.

On November 15th, 1638, Descartes wrote to Mersenne:[22]

You have finally understood the meaning of the word force as I understand it when I say that the same force is needed to lift a weight of one hundred pounds to a height of one foot as fifty pounds to a height of two feet, that is to say, that the same action or effort is needed. I would like to believe that I did not explain myself well enough on this point previously, since you did not understand me. But I was so far from thinking of the force that one calls the strength of a man when referring to someone who is stronger than someone else, that it did not occur to me that the word force could be understood in that sense. And when it is said that less force is needed for one effect than for another, that does not mean that less strength is needed — even if one had more strength, it would have no bearing — but that less action is needed. And in this work I did not talk about force as equivalent to the strength in a man, but only as action and equivalent to the force by which a weight can be lifted. The action can come from a man or from a spring or from another weight, etc. It seems to me that there is no other way to know *a priori* the quantity of this effect, that is to say, how much and what type of weight can be lifted with this or that machine, unless one measures the quantity of this effect, that is to say, of the force which it is necessary to use. I have no doubt that Mr. Desargues will agree with me on that point, if he takes the time to reread the few lines I have written on this topic. Since I can be very certain about the capacity of his mind, I don't think that I ought to doubt the capacity of my reasoning.

This impatience over not being understood can be explained and excused all the more in the case of Descartes, since as early as September 12th, 1638, in a letter to Mersenne,[23] he had defined precisely his use of the word force and had clearly separated this meaning from all other meanings attributable to the same word.

One has to consider above all, he said, that I was talking about the force necessary to lift a weight to a given height, and such a force always has two dimensions and is not the same as the one necessary to support the weight at any point, since that force has only one dimension. These two forces differ as much from each other as a surface differs from a line.

Work, which Descartes calls force, depends on two variables or, as Descartes said, has two dimensions: the magnitude which we call force today and which is of the same nature as weight, and a length which is the projection of the length onto the path of the force. These two variables can be understood as rectangular coordinates of a figurative point. The work accomplished by a constant force will be represented by the rectangle formed by these two coordinates. This graphic representation of work, so commonly used today, did not remain unnoticed by Descartes:

I do not simply say that the force which can lift a weight of fifty pounds to the height of four feet, can also lift a weight of two hundred pounds to the height of one foot, but I say that it can do so to the extent that it is applied to the weight. Yet, it is only possible to apply it by means of some machine or by some other device which causes the weight to rise by only one foot, while this force will act along the total distance of four feet. Thus it transforms the rectangle representing the force required to lift this weight of two hundred pounds to the height of one foot into another rectangle which is equivalent and similar to the one representing the force needed to lift a weight of fifty pounds to the height of four feet.

In this letter to Mersenne, Descartes continually uses this geometrical representation of work.

What Descartes calls force and what we today call work is fundamentally different from the *momento* discussed by Galileo. This latter quantity, which is the product of a weight and its velocity, depends upon three kinds of variable magnitudes: the weight, the distance traversed by the mobile body and the time needed to traverse that distance.

If I had wished to consider velocity together with distance, I would have had to attribute three dimensions to force, instead of attributing only two, in order to avoid the consideration of velocity.[24]

Some reproached Descartes for excluding velocity in establishing the magnitude upon which all of statics depends and they invoked the authority of Galileo. Descartes, however, disdainfully dismissed this ill-founded criticism because, like Stevin and perhaps even inspired by him, he had become convinced that the velocity of motion is not at all proportional to the motor action and that this ancient Peripatetic law should no longer be considered as the foundation of statics.

Between us, I think that those who maintain[25] that I should consider velocity rather than distance in order to satisfactorily explain machines, as Galileo has done, are merely fantasizing and are completely ignorant in this matter. Although it is obvious that more work is needed to lift a body rapidly than to lift it slowly, it is nevertheless pure fantasy to say that the work must be precisely double to double the velocity and it is very easy to prove the contrary.

The attack by Descartes on the principle of Peripatetic dynamics was, by the way, not the first which he made against this axiom. Shortly before, he had written the following lines:[26]

What must be considered first in this matter is that many people have grown accustomed to mistaking the consideration of space for that of time or of velocity. So, for instance, when they consider the lever, or what amounts to the same thing, the balance ABCD (Fig. 91) and when they assume that the arm AB is double the arm BC and that the weight C is double the weight A so that equilibrium holds, then instead of saying that which causes this equilibrium is the fact that if the weight C would lift or were lifted by the weight A, it would only traverse half of the distance of A, they say that it would travel only half as fast, which is a mistake, all the more harmful in that it is hard to recognize. It is not all the difference in velocities which requires that one weight be double the other, but the difference in displacement, as happens for instance when we lift the weight F to G by hand. In that case, we do not need to use an amount of force precisely double the amount used the first time if we wish to lift it twice as fast, but we need to apply an amount of force which must be more or less double the amount according to the diverse proportion which this velocity can have with the forces resisting it.

These two letters were obviously not enough to convince those in the circle of Mersenne who still clung to the approach of Galileo, that is to say, in the final analysis, who adhered to the Ancient foundation defined in the *Mechanical Problems.* On February 2nd, 1643, in a letter[27] addressed to the scholarly man of the cloth, Descartes is obliged to take up the task again:

I now turn to your second letter, which I received almost at the same time as the first. First of all, in as far as you see fit to use in your work something I wrote on mechanics, I leave this to your discretion and you are authorized to use it in any way you please. Several others in this country have already seen it and even have a copy of it. Yet, the reason I chide those who use velocity to explain the efficacy of the lever and other related things, is not because I deny that the same quantity of velocity is always present, but because this velocity does not explain why the force increases or decreases — as does the quantity of space — and because several things must be considered relative to velocity which are not easy to explain. I fail to see the reason why you say that an amount of force capable of lifting a weight A to F (Fig. 92) in one moment could also

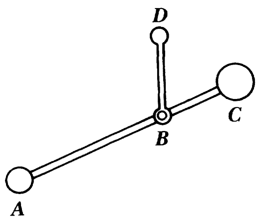


*fig. 91.*


*fig. 92.*

lift it in one moment from A to G, if it were doubled. I think you could easily establish by experiment the contrary. If you have a balance in equilibrium to which you add the smallest weight capable of tilting it, it will tilt very slowly. If, on the other hand, you add double the weight of the first one, it will tilt, but it will tilt far more than twice as fast.

The strength of the argument of the adversaries of Descartes obviously derived from the following reasoning: According to the great philosopher, the force and the resistance in any given machine are to each other as the projections on the vertical of the two paths which the mechanical arrangement makes interdependent. But these two paths are necessarily described in the same time so that the relation between the vertical components of these two displacements is exactly the same as the relation between vertical components of the velocities. It is of no importance if the relation between the two weights in equilibrium is in inverse ratio to the first or to the second, as Guido Ubaldo was always careful to observe. If that is so, and if the law proposed by Descartes leads invariably in all possible cases to the same result as the law formulated by Galileo, why abandon the older and more authoritative of these laws?

Descartes struggled persistently against this view, which furnished correct propositions of statics, it is true, but pretended to account for them on false principles of dynamics. In 1646(?) we see Descartes write again[28] to Boswell(?):

I do not deny the material truths of what the mechanicians are in the habit of saying, to wit, that the greater the velocity of the extremity of the long arm of the lever with respect to velocity of the other extremity, the less force is needed to move it. But I deny that it is the velocity or the slowness which is the cause of this effect.

We should not underestimate or take lightly the importance of this modification which Descartes makes of the formulation by Galileo. It is due to that modification that the laws of equilibrium are no longer founded on an inaccurate postulate or on the dynamics of Aristotle — already discredited — or even on the new dynamics which has not yet been formally established. Statics is becoming an autonomous science which is derived entirely from a principle of absolute certainty and obvious clarity.

And if I have demonstrated any skill at all[29] in any part of this brief treatise on statics, I want it to be known that it is more evident here than in anything else. It is impossible to say anything worthwhile about velocity, without having duly explained gravity and beyond that the entire system of the world. Since I did not wish to undertake this, I found a way to avoid this consideration and separate all the others from it so as to be able to explain them without it.

Descartes had read the statics of Stevin and he could not have been unaware of the importance of the law stating the composition of two concurrent forces. It is surprising that it did not occur to him to derive this law from the principle upon which he founded his statics. One could assume since he did not do so, that he considered the problem to be solved. We saw that Roberval was able to demonstrate quite successfully the law of the composition of concurrent forces by invoking the very axiom which Descartes was to formulate in complete generality. To attribute the silence of Descartes regarding the law of the parallelogram of forces to a reluctance to claim for himself a solution worked out by another geometer is to attribute to Descartes a sense of justice which he rarely felt towards his rivals and which he never felt toward Roberval.

Roberval had claimed priority for the postulate upon which the entire statics of Descartes was based. Roberval must have made this claim in the presence of Mersenne, who then made it known to the philosopher. Descartes responded[30] with a letter in a tone of almost unequalled contempt and insolence:

I have just finished reading the *Traité de Méchanique* by Mr. Roberval, in which I learned that he is a professor — a fact which I had not known — and then I remembered that you once told me that he was President in some Province and I was no longer surprised by his style. As for his treatise, I could find innumerable faults in it if I wished to submit it to rigorous examination. But I shall tell you briefly that he went to great lengths to explain a very simple principle and he made it more difficult with his explanation than it is by nature. Furthermore, Stevin demonstrated before him the same things in a far more simple and general manner. It is true that I do not know whether either of them provided an accurate demonstration, because I do not have the patience to read such books to the end. In as far as he claims to have put in a single corollary what I wrote in my work on statics, I say, *aberrat toto Caelo*,[31] because he makes a conclusion of what I make a principle and he talks about time and velocity, whereas I talk about space. This is a grave error on his part, as I have explained in my previous work.

This letter abounds with unfair judgments and it is obvious from it that Descartes had scarcely condescended to glance at the very elegant demonstration of Roberval, who had always considered the path traversed by the various weights and never the time or velocity. The letter furnishes us nonetheless with a valuable and precise bit of information. Descartes did not have the patience to carefully read the works of Stevin and Roberval. Thus it is not surprising to find him quite ignorant on the subject of the problem of the composition of forces.

We have very clear and obvious proof of this ignorance. On November 18th, 1640, Descartes wrote to Mersenne:[32]

It is certain that the weight C (Fig. 93) only exerts a force on the plane AD which is the difference between the force necessary to support it on the plane and the force necessary to support it in air. Since it weighs one hundred pounds and since only forty are necessary to hold it on AD, this plane AD exerts only sixty pounds.

Thus in 1640, Descartes still believes that the two components of a weight have as their algebraic sum the total weight! The arrogant philosopher could have profited greatly by reading more patiently the statics of Stevin and the *Traité de Méchanique* of Roberval or, simply, the *Cours mathématique* by Hérigone and the works of Mersenne.

The statics of Descartes signals the last stage in a long development. All the ideas of the predecessors of the great French geometer contributed to the construction of this doctrine. But it is in this doctrine alone that all of these ideas were brought to perfection. It is here that all their apparent contradictions dissolve into a harmonious synthesis.

Galileo had founded all of statics on a single principle, when he completed the work of Aristotle, Leonardo da Vinci and Cardan. Yet those in favor of a more rigorous method, such as Stevin, were able to raise a serious objection to this principle: to wit, it was only a corollary of Peripatetic dynamics, which, henceforth, had been discredited.

The thinkers who favored a rigid and geometrical certainty found the method of Stevin and Roberval entirely satisfactory because it established an autonomous statics, devoid of any hypothesis borrowed from

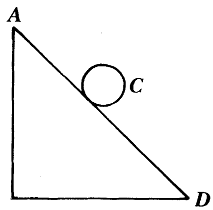


*fig. 93.*

a suspect dynamics. Although no geometer had attempted to refute or to question any of the postulates invoked by the mechanician from Brugge or by the Professor of the Collège de France, several geometers had hoped for a single axiom from which to derive all the postulates and which would be their true *raison d'être.*

The work of Descartes satisfies all of these different intellectual aspirations. It retains the breadth and the unity of the method of Galileo, which condenses all of statics into a single principle. It retains the rigor of the method of Stevin because its foundation, which seems certain and self-evident, borrows nothing from the outmoded doctrines of dynamics.

Before the statics of Descartes, Stevin and Galileo had already come upon the importance of the product of the weight of a body and the distance by which it descends. However, their encounter with it had been fortuitous and ephemeral. Neither the Belgian nor the Italian geometer pointed out its importance. In some of the demonstrations by the School of Jordanus as well as those of Roberval and Hérigone, the product of the weight and its vertical descent plays an important role. Yet nowhere is it stated that the entire body of statics could be reduced to a comparison of such products. Descartes was the first to see in this product the fundamental concept of mechanics. In this he is, if not the actual creator, at least the most influential promoter of the concept of work, which is the foundation of our entire contemporary science of equilibrium and motion.

His efforts to refine the concept of work and to distinguish it from the concept which Mersenne and Desargues confused with it, contributed to a large extent to the precise definition of the concept of work as we understand it today. It was that concept which continually haunted the minds of his critics.

Descartes clearly understood and underscored the infinitesimal property of the Principle of Virtual Displacements and he states what nobody had explicitly formulated before him: the necessity to apply this principle to an infinitely small displacement originating from a state of equilibrium. From this he deduced the equivalence of all relations corresponding to the same infinitesimal virtual path and thus gave this principle its definitive form.

One might object that the statics for which he furnished the blueprint lacks generality because the only force considered is that of weight. This lack of generality is only apparent. When the geometers of the

17th century — Stevin, Galileo and Roberval — analyze any given force, they replace it with a rope stretched in the direction in which it is supposed to act and which passes over a pulley and supports a weight equal to that force. It was through this artifice that the statics of weights came to include all of statics. Indeed, through this artifice, any geometer can effortlessly deduce from the principle stated by Descartes the Principle of Virtual Displacements in the exact form in which Jean Bernoulli was to transmit it to Varignon in 1717. In all probability, this procedure was the one which inspired Bernoulli to make his discovery. When Lagrange in his *Mécanique analytique* proposes it as the correct way to establish the Principle of Virtual Displacements, he is only reverting to the method of the inventor. Thus the principle which Descartes formulated contains implicitly, yet obviously, the axiom from which we derive today all the laws of statics.

The statics of Descartes is thus the harvest after a long period of growth. In order to find the seed from which this growth started, we had to go back a long way in time to the beginning of the 13th century, where we assembled the teachings of the School of Jordanus. From this seed, which was engendered by Occidental science in its budding youth, we can follow each of the advances, each of the transformations by which the science of equilibrium gradually developed. And when it finally reaches fruition in Cartesian mechanics, we can distinguish each of the layers and tissues comprising this fruit.

What was the precise contribution of Descartes to the formation of Cartesian statics? Certainly, he gave to it the order and clarity which are the very essence of his method and which characterize so perfectly his eminently French genius. But did the great philosopher add anything to the science of equilibrium beyond giving it form? Did he add any truth unknown before him? It would be vain to look for any trace of such a contribution. There is no truth contained in the statics of Descartes which was unknown before Descartes.

Blinded by his prodigious arrogance, Descartes sees nothing but errors in the works of his predecessors and contempraries. He believes[33] that the difficulties encountered by those who are preoccupied by the problems of equilibrium:

> . . . stem for the most part from the fact that people are already too knowledgeable about mechanics, that is to say, too preopccupied with the principles adopted by others on these matters, principles which are in no way true but deceive all the more by appearing to be so.

He obligingly allows Constantine Huygens to tell him that:[34]

> . . . his writings are destined to rid the world of a universal deluge of error and ignorance.

He is undoubtedly convinced that he alone knows the true foundations of statics and that he has built statics from the ground up on terrain cleared by his criticism of all the crumbling hovels built by other geometers. Faced with such a superb lack of conscience, one catches oneself recalling the following thought of Pascal:[35]

> Certain authors, in talking about their work, say: *My* book, *my* commentary, *my* history, etc. They smack of a complacent bourgeoisie priding itself on the ownership of a house and talking constantly about its "own place." They would be better off to say: *Our* book, *our* commentary, *our* history, etc., since, most often those things contain more from others than from themselves.

# NOTES TO VOLUME I

## A. ON THE IDENTITY OF CHARISTION AND HERISTON

We introduced in Chapter V, Section 2, the hypothesis that the mechanician Charistion might be the same person as Heriston, son of Ptolemy, to whom the father dedicated the *Liber diversarum rerum*.[1] The work, published in Venice in 1508, actually contains the following dedication in the title: *ad Heristonem filium suum*.[2] However, the manuscript, which is found in the Bibliothèque Nationale (Latin Section, No. 16208), carries the following remark in the title: *ad Aristonem*.[3] The name Ariston, attributed to the son of Ptolemy, is one of the arguments in support of the claim that the *Liber diversarum rerum* is apocryphal.

Ariston is actually the name of the person, unknown elsewhere, to whom Philo of Byzantium dedicated all of his works,[4]

> the dedication to Ariston, said Carra de Vaux, seems to be a kind of signature in all the works of Philo.

Some believe that the author of the *Liber diversarum rerum* borrowed the name of Ariston from the writings of Philo of Byzantium[5] and attributed it to a son of Ptolemy, ignorant of the fact that Philo lived nearly 250 years before Ptolemy.

Thus the fictitious person to whom the apocryphal *Liber diversarum rerum* is dedicated would in this case have had nothing to do with Charistion. I am indebted to Mr. Eneström for having brought this point to my attention.

Let us mention, in conclusion, that Steinschneider[6] introduced as an aside the hypothesis that the Arabic word *karastun* for the Roman balance probably came from the Greek *Charistion*. The rarely used word *faristun* has in Persian the same meaning.

## B. JORDANUS DE NEMORE AND ROGER BACON

It is well-known that the *Opus maius* of Roger Bacon (1214—1292 or 1294) was addressed to Pope Clement IV, who died in 1268. Therefore, it cannot be later than that date. Not only was this work printed twice, but certain parts of it were abstracted and published separately. For example, the first part of Section 4 was published in 1614 under the title: *Specula mathematica*.[7]

This work is divided into four sections.[8] The fourth section comprises fifteen chapters, the last, *De motu librae*, deals with the balance and its motion.

There Bacon attempts to demonstrate how effective geometry is for the study of motion. It is geometry, namely:

> which permits one to comprehend the science of statics. This is a very beautiful science, but too difficult for those individuals who have not studied the causes of the motion of heavy or light bodies . . .

*Dicit ergo Jordanus, in libro de ponderibus*, . . .[9] are the words with which Bacon begins his exposition. This exposition is devoted entirely to the theory of the equilibrium of the balance and the arguments developed by Jordanus. As soon as the balance deviates from the position of horizontal equilibrium, the positional gravity of the weight which is elevated becomes greater than the positional gravity of the weight which is descending, so that the balance, when left to itself, returns to the position from which it was displaced. Bacon not only makes no objection to the theory of Jordanus, but he even finds in this theory the solution to a difficult problem.

The velocity of a falling body increases in descent. However, the velocity of a falling body is proportional to its weight. So a body becomes heavier the further it descends. Such is the opinion of Aristotle, or, at least, such is the interpretation that most of the commentators give to his view. Bacon does not doubt in the least that this idea is correct, but he deduces from it an embarrassing corollary.

Suppose one displaces slightly the arm of a horizontal balance from equilibrium. The weight which descends moves towards the center of the earth. Thus it becomes heavier. The other weight, on the contrary, being elevated becomes lighter. As a consequence, the arm continues to move in the direction of increasing inclination and it does not cease to move in this direction until it is vertical. The equilibrium of the balance is essentially unstable. Bacon says *hoc est contra Jordanum et contra sensum*.[10]

The doctrine of Jordanus resolves this difficulty. The weight, which according to Aristotle becomes heavier as it approaches the center of the earth, is, according to Jordanus, the one which possesses the least positional gravity. Only the second effect, being more powerful than the first, manifests itself. In expounding this rather unusual theory, Bacon does not quote Jordanus alone, but his "commentator" as well.

Now we have seen that there existed from the 13th century on at least two distinct commentaries on the treatise *De ponderibus* composed by Jordanus. Which of the two commentaries was known to Bacon? The response to this question is difficult because Bacon hardly borrows anything from the *De ponderibus* other than the views and formulations common to Jordanus and his various commentators. Note, however, that in order to explain the diminution of positional gravity of a point which descends on a circle, Bacon invokes in two instances not only the obliquity but also the curvature of the trajectory. This remark leads one to believe that the commentary on the *Elementa Jordani super demonstrationem ponderis*,[11] which Bacon knew, is the very same one which we described in Chapter VII, Section 2, as the work of a Peripatetic commentator. Furthermore, it is the sole work which gives to its demonstrations the title of *comentum*.[12]

## C. ON THE VARIOUS AXIOMS PERMITTING THE DEDUCTION OF THE THEORY OF THE LEVER

We noted in Chapter V, Section 1, that *The Book on the Balance*, attributed to Euclid and rediscovered by Woepcke in an Arabic manuscript, begins with an awkward attempt to demonstrate the following proposition.

> If any number of weights maintain the beam of a balance parallel to the horizon and if Z and D are two weights suspended from the same arm of the beam and if weight Z is moved further away from the fulcrum by a given distance and if weight D is moved the same distance towards the fulcrum, the beam remains parallel to the horizon.

If, rejecting this demonstration, one takes this proposition as an axiom — for the sake of clarity, we will call it the Axiom of Euclid — one is then able to deduce from it the theory of the lever, as did the author of *The Book on the Balance*.

On the other hand, the classical demonstration of Archimedes, apart from the axioms explicitly enumerated by the great geometer, assumes this axiom to be implicitly acknowledged:[13]

If two equal weights A and B, suspended at two distinct points C and D from the same arm of the beam, are equilibrated by a weight E suspended from the other arm of the beam, the same weight E will equilibrate a weight equal to (A + B) or 2A suspended from a point M, at the midpoint of the interval CD.

We will call this axiom the Axiom of Archimedes.

It is clear that the Axiom of Archimedes, if one wishes, can be regarded as an immediate corollary to the Axiom of Euclid and vice versa, so that these two axioms are exactly equivalent.

In the *Mechanical Problems*, attributed to the Philosopher of Stagira, the law of equilibrium of the lever is derived from a third axiom, which we will call the Axiom of Aristotle.

The two weights which produce equilibrium at the extremities of the lever are inversely proportional to the velocities with which those extremities move in a virtual displacement of the lever.

This axiom was used not only by the author of the *Mechanical Problems*, but also by Charistion in the book of *Causes*, of which Thâbit ibn Qurra has left us a record.

Besides these axioms, all equally suitable for founding the theory of the lever, it is fitting to cite a fourth, which we shall call the Axiom on the Beam. This axiom can be stated as follows:

A heavy cylinder whose axis is identical to a segment of a lever arm is equivalent to a body of the same weight which hangs from the midpoint of the segment of the lever arm covered by the cylinder.

This axiom was employed by Stevin and Galileo to establish the theory of the lever, but it had already been known for a long time that it was suitable for this purpose. Lagrange remarks that,[14]

Archimedes had previously used a similar concept to determine the center of gravity of a figure composed of two parabolic surfaces in the first proposition of the second book on the *Equilibrium of Plane Figures.*

For our part, we discovered in a manuscript of the 13th century,[15] a demonstration of the law of the lever, very rigorously deduced from the Axiom on the Beam. This manuscript, as we have said, appears to us to be of Alexandrian origin, as for example the *Treatise on Specific*

*Weights*, to which it is appended but which is falsely attributed to Archimedes.

All four axioms, whose formulation we have just recalled, seem to have served since Antiquity to establish the law of equilibrium of the lever. Furthermore, it seems that their exact equivalence, i.e., the possibility of deducing any three of them from the fourth was commonly recognized. The *Causes* of Charistion, for example, as they are reconstructed by Thâbit ibn Qurra, take as a point of departure the Axiom of Aristotle and undertake to deduce from it the Axiom on the Beam. The fragment, comprised of four propositions and attributed to Euclid, which we analyzed in Chapter V, Section I, seems to have as its main objective the demonstration of the Axiom of Aristotle, the Axiom of Archimedes and the Axiom on the Beam. These three axioms, in fact, coincide respectively with the propositions we called A, C and D. The author, to justify these axioms, assumed as known the law of the equilibrium of the lever, which he deduced then from a fourth axiom, probably the Axiom of Euclid.

Jordanus also seems to have composed the *Elements on the Demonstration of Weights* to justify the Axiom of Archimedes, and by using it, the Axiom on the Beam. But to justify the law of the lever he implicitly invoked a completely new axiom which can be formulated as follows:

Whatever can lift a given weight to a given height can also lift a weight K times heavier to a height K times smaller.

We find no trace of this axiom of Jordanus in the works on mechanics bequeathed to us by Antiquity. It seems to be a spontaneous product of Western science. It presents a clear advantage over the axioms used by the Greek and Arabic mechanicians, to wit, its extreme generality. It is true that Jordanus only applied it to the theory of the straight lever. But his anonymous student, whom we call the "Precursor of Leonardo," deduced from it the laws of the bent lever and the inclined plane. Leonardo da Vinci, Cardan and Salomon de Caus pointed out its importance for industrial mechanics. Roberval made use of it to justify the rule for the composition of concurrent forces. Finally, Descartes proposed to use it as the foundation of all statics.

# THE ORIGINS OF STATICS

Volume 2

by
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Professor in the Faculty of Sciences at Bordeaux

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# PREFACE

We shall begin by redressing an unintentional injustice.

In the preface to Volume I, we stated that at the outset of our research we had been unaware of the solution to the problem of the inclined plane borrowed by Tartaglia from the School of Jordanus. We added that not a single historian of mechanics had ever mentioned that wonderful solution. However, it turns out that we were mistaken on this point.

Several years ago the Academy of Science at Turin received a significant paper[1] from Mr Giovanni Vailati, in which he reviewed the diverse intimations of the Principle of Virtual Velocities in the works of the Greek mechanicians. Vailati considered as one of those works the treatise which was plagiarized by Tartaglia but which had been written by the unknown author whom we have called the Precursor of Leonardo.

We shall not discuss here the date of composition which Vailati assigns to this treatise on mechanics because we shall examine that question elsewhere.[2] We shall merely state for the time being that the erudite professor at the Technical Institute of Florence correctly assessed the importance of this treatise, as the following quotation from his analysis shows:[3]

> To find a work in which statics is so completely tied to the Principle of Virtual Work — even though this principle is only partially and imperfectly developed — a work, in my opinion, where statics is so totally subordinated to this principle that the right to any intuitive initiative — so prevalent in the approach of Archimedes — is rigorously rejected; to find such a work, I say, one is compelled to refer to the short treatise by Descartes, entitled: *Explicatio machinarum atque instrumentorum quorum ope gravissima quaeque pondera sublevantur*. This short treatise is actually the very first attempt, after the previously discussed treatise, to construct the entire edifice of statics on the same basis which Lagrange's *Mécanique Analytique* was to use.

During the Middle Ages statics was taught in two ways. In the universities the Schoolmen taught the study of the laws of equilibrium together with their own commentaries on the cosmological writings of Aristotle. Outside the universities, statics was considered to be an autonomous mathematical science without any connection with philosophy. The basis for that science was found in works at times attributed to Euclid, at other times to Archimedes or Jordanus, although most often the authors were simply called by a collective name, *Auctores de ponderibus.*[4]

The main objective of our first volume was to trace through innumerable vicissitudes the development of the methods created by the *Authorities on Weights.* This development culminated in Cartesian statics, which was founded entirely upon the equality between the work of the impressed and resisting forces.

The first two chapters of the present volume will retrace the evolution of the ideas formulated by the Schoolmen. We shall see how this evolution leads to the famous principle of Torricelli:

> A system of heavy bodies whose center of gravity has descended as low as possible is indubitably in equilibrium.

The insight which was to lead to this truth can already be seen, although vaguely and indistinctly, in the very early commentaries on Aristotle, such as those of Simplicius. It becomes more precise in the XIVth century in the books of Albert of Saxony and is formulated as follows: In each descending body there exists a well-determined point, the center of gravity, which tends to move to the center of heavy things.

This proposition, which will prove to be so productive, contains nonetheless a major error: The existence of a fixed center of gravity in a heavy body is linked to the assumption that the vertical lines[5] from the different points of that body can be considered as being parallel to each other. This is incompatible with the existence of a common center for heavy bodies at a finite distance from the surface of the earth. However, as erroneous as this proposition was, it was universally and uncritically accepted "as the clearest and most evident of all possible axioms."

The Copernican revolution, which relocated the center of the universe, did not negate the preceding principle, but merely compelled its modification. The center of the earth was substituted for the common center of heavy bodies and the axiom thereby rejuvenated could then receive the unqualified approval of Galileo.

The obviously inadmissible inferences deduced by Fermat from this erroneous principle were enough to bring about its demise, although the useful corollaries which had been deduced from it assumed their correct form.

This erroneous principle, which for so long had guided the statics of the School, had also produced the geodesical theory most commonly taught in the universities. Thus the history of the science of equilibrium is inextricably tied to the history of the doctrines on the configuration of the earth and the oceans as developed in the Middle Ages and the Renaissance. So no one should be surprised to find in the present volume how closely the history of the latter era is tied to the history of the properties of the center of gravity.

The numerous notes which can be found at the end of this volume will present to the reader some discoveries made too late to find their appropriate place in this study. Some of these discoveries occurred to us spontaneously during the course of our lengthy investigation. Others were brought to our attention through the astuteness of many of the kind readers of our manuscript. Having thanked each of them individually for their separate contributions, we would now like to take this opportunity to thank them collectively.

We would also like to acknowledge our gratitude to the Reverend Father J. Thirion whose kindness provided us with documents which otherwise would have been difficult to obtain and whose vigilance brought to our atteniton errors we had overlooked.

*Bordeaux, July 14, 1906* P. DUHEM

CHAPTER XV

# THE MECHANICAL PROPERTIES OF THE CENTER OF GRAVITY FROM ALBERT OF SAXONY TO EVANGELISTA TORRICELLI

## First Period

## From Albert of Saxony to the Copernican Revolution

### 1. FORMULATION OF THE PRINCIPLE OF TORRICELLI

Lagrange once wrote:[1]

> Torricelli, the famous disciple of Galileo, is the author of another principle which also depends on the Principle of Virtual Velocities. This is so, because when two weights are connected and placed in such a way that their center of gravity can not descend further, they are said to be in a state of equilibrium. Although Torricelli only applies this principle to the inclined plane, it is easy to see that it is also applicable to other machines.

The formulation of this principle mentioned by Lagrange can be found in a collection entitled *Opera geometrica Evangelistae Torricellii*, published in Florence in 1644.[2]

Torricelli says the following in the section dealing with the motion of descending bodies:[3]

> We shall assume as a principle: Two descending bodies joined together can not move by themselves unless their common center of gravity descends. Indeed, when two weights are connected in such a way that the motion of one produces the motion of the other and this connection is made by means of a balance, a pulley or any other mechanism, then these two bodies will behave as a single body composed of two parts. Yet, such a body will never move by itself, unless its center of gravity descends. But when the mechanism is configured in such a way that its center of gravity cannot descend further, the body will certainly remain at rest in its final configuration. Furthermore, the body would move in vain because it would be moving in a horizontal line without any downward tendency.

Torricelli postulates this principle in order to solve the problem of the inclined plane. We shall see later why he was so interested in the solution to this problem. Immediately after formulating his fundamental postulate, he states this proposition:[4]

If two bodies are placed on two planes of unequal inclination but at the same elevation and if the weights of these bodies are to each other as the lengths of the planes, these two bodies will have the same *momento*.

Indeed, we shall demonstrate that their common center of gravity cannot descend further, because whatever movement one may impart to these two bodies, it will always be along a horizontal line . . . Thus two connected bodies would move, but their common center of gravity would not descend. This would be contrary to the law of equilibrium which we have assumed as a principle.

At the beginning of his work *On the measurement of the parabola*,[5] Torricelli also returns to this law of equilibrium. Indeed, he formulates the following hypothesis which is for him the very definition of the center of gravity:

We shall assume that the nature of the center of gravity is such that a body suspended freely from any given point is unable to remain at rest as long as the center of gravity is not at the lowest possible point on the sphere on which the body is moving.

From this Torricelli easily deduces that at the instant of equilibrium, the center of gravity is located on the vertical through the point of suspension and below this point. In the same work,[6] Torricelli attempts to construct from his definition of equilibrium the law of equilibrium for a lever. He gives two equivalent demonstrations but we shall only quote the second.

The lever AE (Fig. 94) turns around point B. It supports respectively

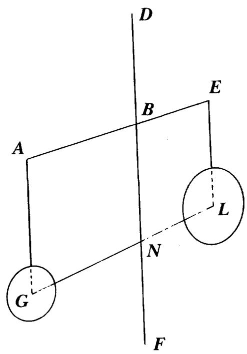


*fig. 94.*

two weights suspended from points A and E, and in inverse proportion to the lengths AB and BE.

Let us connect the two centers of gravity G and L by the line GL.

Since the magnitude of weight L is to the magnitude of weight G in the same ratio as AB is to BE, or, for reasons of parallelism, as GN is to NL, the common center of gravity for the two weights suspended from the lever is at N. If the balance AE does not remain at rest, the center of gravity N will rise because being on the vertical DF it cannot move without rising.

Torricelli overlooks something here. A virtual displacement of the balance would not cause point N to go up, but would leave it immobile. In this case as well as in the case of the inclined plane, the common center of gravity of the two connected weights is not at the lowest possible point. It would be at the same height after a virtual displacement. Today, thanks to Lagrange, we know how to connect this feature with another. The two cases of equilibrium of which Torricelli speaks are each cases of indifferent equilibrium. On the other hand, the equilibrium of a system of weights is stable when the center of gravity of this ensemble is lower in its actual state than in any other adjacent state. We have seen how Roberval had dealt with such a case of stable equilibrium before Torricelli.

Furthermore, it appears that on the subject of stability, Torricelli did not have as clear a conception as he might have had from the research and discussions of his predecessors. The demonstration of the law of equilibrium of the lever — quoted above — is followed by this passage:[7]

I am aware that a controversy has arisen among authors on whether a balance carrying weights with centers on the beam itself will remain in the position in which it is placed or whether it will return to its initial position. As far as we are concerned in this book, we have always assumed that the weights were suspended below the beam. We have always preferred to write on matters useful to our topic rather than to adapt our demonstrations to the controversies of others.

It is of little importance to the stability of a balance if the centers of gravity of the weights are above or below the beam. Its stability depends on the configuration of the beam with respect to the point of suspension. If the beam is nothing but a straight line passing through the point of suspension — as in the demonstration by Torricelli — the equilibrium of the balance is indifferent, even when the weights hang below the beam. These ideas were clearly stated as early as the 13th century in the treatise written by the Precursor of Leonardo da Vinci. Leonardo and Benedetti had further elaborated on them. One can only be surprised at the ignorance shown here by the most illustrious of Galileo's followers.

## 2. THE CONCEPT OF THE CENTER OF GRAVITY IN ANTIQUITY

The new principles introduced to statics by Torricelli reached the precise formulation which he gave them through a slow evolution and we shall recount its main stages.

Archimedes often spoke of the concept of the center of gravity and he taught us how to find this point in various plane figures. Yet, none of his works which have come down to us contain any definition of this concept. Among the authors of Antiquity, only Pappus includes a definition of the center of gravity. Let us imagine,[8] says Pappus, that a body is suspended about an axis $\alpha\beta$, and let us permit it to take its position of equilibrium. The vertical plane which passes through $\alpha\beta$

> . . . will divide the body into two parts in equilibrium, which will remain suspended on both sides of the plane, because they have equal weight.

Let us take another axis $\alpha'\beta'$ and let us repeat the same procedure: the new vertical plane passing through the new axis will surely cut the preceding one. Indeed, if it were parallel to it:

> . . . each of these two planes would divide the body into two parts which would have simultaneously equal and unequal weights, which is absurd.

Now let us suspend the falling weight by a point $\gamma$ and when it has come to rest, let us draw the vertical $\gamma\delta$ from the point of suspension. Next, let us take a second point of suspension $\gamma'$ and, by a similar procedure, let us trace a second line $\gamma'\delta'$. The two lines $\gamma\delta$ and $\gamma'\delta'$ will surely intersect. If that were not so, one could pass through each of the two a plane dividing the body into two balanced parts in such a way that these two planes would be parallel to each other, which, of course, one knows is impossible.

All the lines such as $\gamma\delta$ will intersect at the same point in the body which we shall call the center of gravity. Two remarks are in order on this definition. The first one is stated in the following way by Guido Ubaldo:[9]

The plane drawn by $\alpha\beta$ must divide the heavy body into two parts which are equally heavy on both sides. This does not mean that they would have equal weights if considered and weighed separately. But this is not what happens. The two parts of the body must be equilibrated in the positions they occupy so that neither of them outweighs the other.

The definition given by Pappus is thus not complete until one defines the equivalence between the two parts into which a falling weight is divided by a plane containing the center of gravity. In modern terms, we define this equivalence by stating that these two parts have the same moment with respect to the plane. It is this concept of moment that Pappus and the geometers after him have in mind when they determine the center of gravity for a body and this application is made by means of the Law of the Lever[10] which is the origin of the concept of moment. But occasionally, because the result of their reasoning was not false, geometers would not be sufficiently cautious with their logic and would argue as if the two parts of a body separated by a plane passing through the center of gravity, were of equal weight rather than of equal moment. Simply because of the fact that the median produces two triangles with equal areas, Pappus concludes[11] that the center of gravity of a triangle is situated on the median.

The second remark is of great importance for the investigation which we shall pursue in this chapter. Today, we know: one, the law of the lever, as formulated by Archimedes, two, the rules developed by the geometers for establishing the center of gravity for different bodies and three, the existence in a solid body of a fixed point which can be called by the name of center of gravity, are all consequences of the following hypothesis: gravity has the same magnitude and direction at every point in the body.

It is quite certain that geometers did not obtain until very late the precise conditions on which the accuracy of the law of the lever and the notion of the center of gravity itself depend. It is true that everything written by Archimedes in his treatise *On the Equilibrium of Planes* accords with the hypothesis of a gravity uniformly constant in magnitude and direction. Yet, nowhere does the great geometer mention that this assumption is essential for the accuracy of his propositions. One cannot even be sure if he had a clear opinion in this matter.

To read his books *On Floating Bodies* will reinforce this uncertainty. In the first of these two books, we see him continuously assume and represent the convergence of the vertical lines at the center of the earth, while the laws which he wants to demonstrate are inaccurate unless gravity is of uniformly constant magnitude and direction. Thus the illustrious Syracusan gives to the principle which still carries his name, a formulation which is both excessively general and blemished by a grave error.[12] But, in the second book, when he is about to apply this principle, he considers the verticals as parallel and thus the erroneous consequences of his first analysis are avoided.

There is no evidence that Pappus had any clearer understanding than Archimedes of the conditions necessary to determine the center of gravity of a body. Like his illustrious predecessor, it appears that he attached no importance to this question. He defines the verticals as lines converging towards the center of the Universe,[13] but immediately thereafter, he treats them as if they were parallel.

## 3. THE PROPENSITY OF THE CENTER OF GRAVITY TO MOVE TOWARDS THE CENTER OF THE UNIVERSE. ALBERT OF SAXONY (XIVTH CENTURY)

The notion of the center of gravity was vague and imprecise even in the minds of geometers. One can well imagine how uncertain and ill-defined it was in the eyes of the physicists and philosophers. One can observe the gradual emergence of a doctrine which gains in precision, and though it seems rather bizarre to us today, was nonetheless accepted by great minds for centuries and was one of the most enduring and least contested theories in the history of physics.

This doctrine can be formulated as follows: There exists in every falling body a point at which its heaviness seems to be concentrated, which can be called the center of gravity. Furthermore, in every falling body heaviness is a desire to unite this center of gravity with the center of the Universe. If its center of gravity coincides with the center of the Universe, the falling body is at rest. If the center of gravity is not at the center of the Universe, the first point seeks to join the second and, if unimpeded, will move towards it in a direct line. The earth is a falling body like all others. It seeks to join its center of gravity to the center of the Universe. Therefore, the earth will remain immobile at the center of the Universe.

In order to find the origins of this theory, one must go back to

Aristotle, where it can be recognized, though faint and indistinct, in a chapter of *On the Heavens*.[14]

Given that the center of the Universe coincides with the center of the earth, we shall ask, says Aristotle, towards which of these two centers all falling bodies tend to move by nature including the elements of the earth themselves. Do they move towards this point because it is the center of the Universe or because it is the center of the earth? They move, of necessity, towards the center of the Universe . . . Yet, it so happens that the earth has the same center as the Universe. Thus, when the falling weights move towards the center of the earth, they do so accidently because the earth has its center at the center of the Universe . . . That is the reason they move towards the common center of the earth and the Universe . . .

There is another question which can be solved in the same way. Let us assume that the earth is round, that it occupies the center of the Universe and that if we add a large weight to one of the hemispheres, the center of the Universe and of the earth will no longer coincide. What will happen then? Either the earth will remain immobile at the center of the Universe or it will not remain immobile since it is not at the center and consequently, it will be apt to move. This is the question at issue. But this question is easily resolved if we merely analyze the judgement we make when a given heavy body moves to the center. *It is clear that the descent of this body will not stop at the moment at which its lowest point touches the center of the Universe.* Its heaviest part will carry further as long as its center does not coincide with the center of the Universe because up to that moment, it will have the power to move. One can say the same thing about a part or about the entire earth. What we said does not happen because of magnitude or size, but is common to all bodies which tend to move towards a center. Thus, starting from any given point, the earth will move towards the center either as a whole or in parts, and it will move by necessity until it surrounds the center in a uniform way and the tendencies to movement in the various parts will counterbalance each other.

The doctrine of Aristotle is still very imprecise in this passage. The Philosopher fails to characterize the center which in every falling body tends to move to the center of the Universe. He does not connect it to the center of gravity, which was unknown to him.

Simplicius[15] in commenting on this passage by Aristotle makes a rather vague and tenuous connection between the center of a falling body and its center of gravity. He considers the last objection raised by Aristotle to be a result of the latter's research in the field of study called Centrobarics[16] by the mechanicians. Archimedes and others had formulated many elegant propositions on Centrobarics which attempts to determine the center of a given gravity. It is clear that the Universe, i.e. the earth, which is assumed to be spherical will have the same center of magnitude and gravity.

It does seem that this passage received much attention by the commentators after Simplicius. St. Thomas Aquinas[17] for example, merely quotes Aristotle almost literally:

It is clear that a body with gravity does not only move towards the center of the Universe until its lowest point touches this center, but if unimpeded, the larger part will prevail over the smaller part and the body in motion will tend to the center of the Universe until its center touches the earth's center. This is the goal and tendency of all falling bodies.

Imagine that the only existing falling body in the world was a single stone thrown from a height. The weight would fall until its own center touched the center of the earth. Indeed, the larger part would push the smaller away from the center until the gravity was equal on all sides, as stated above. And from this the Philosopher concludes that the exact same thing can be said about either any part of the earth, or about the entire earth.

Averroes before St. Thomas had said more or less the same,[18] but in a more verbose and confused manner. Furthermore, Albertus Magnus composed formulations[19] almost identical to those of Averroes.

What had been a casual remark in the works of Aristotle, assumed in the commentaries by Simplicius and St. Thomas Aquinas the dimensions of a theory during the 14th century. Walter Burley (1275—1357) had already extensively elaborated on the remarks of Aristotle.[20] The *natural locus* of the earth is not the internal surface of the element water:

The earth is only in its natural locus when it has as its center the very center of the Universe. Likewise, water is only in its natural locus when its sphere has as its center the center of the Universe which is the same as that of the earth.

And one can say further about the other elements:

No element is in its natural locus if its center is not at the center of the Universe.

A portion of the earth, unimpeded in its motion, moves towards the center of the Universe and not towards the internal surface of water.

It is true that a difficulty appears here.

When the earth has as its center the center of the Universe, each of its parts is subjected to violence, because, free of any constraint, it would move naturally towards the center.

Likewise, if a hole were bored from one side of the earth to the other through its center and a clod of earth were thrown down that hole it would move until its center reached the center of the Universe. One half of the clod would then be on one side of the center of the Universe and the other half on the other side. This, however, can only be achieved if one part of the clod moves away from the center of the Universe and approaches the heavens. This latter motion is an upwards motion, thus a violent motion, which is impossible.

Burley answers:

. . . that a part of the earth, detached from the whole earth, is submitted to violence when its center is not the center of the Universe, because, if unimpeded, it would move towards the center of the Universe. However, when joined with the rest of the earth, it can rest outside the center of the Universe without being submitted to violence, because it is at rest, not on its own, but in accordance with the position of the rest of the whole.

If all the elements were shaped like spheres having as centers the center of the Universe — according to Burley, they would thus be in their natural locus — the earth would be completely covered by water. How can we explain that such is not the case? John Duns Scotus,[21] the Subtle Doctor (1275—1309) asked himself this question, but was content with a teleological explanation:

If all the elements were distributed symmetrically, the earth would be completely covered by water. In fact, at present, only a part of the earth is covered by water with a view toward the salvation of human beings.

John of Jandun, in 1316, Master of Theology, followed in many points the opinions expressed by Walter Burley, and like him and Aristotle, he believed:[22]

. . . that the earth moves as a whole towards the center of the Universe and that its motion will only stop if it reaches the midpoint of the Universe.

He seems to accept the finalist explanation concerning the existence of continents which Duns Scotus had accepted. However, on this subject, he encounters a difficulty:

It is certain that water has weight. On the other hand, one part of the earth — the one inhabited by animals — is not covered by water. Thus it appears that the center of the earth cannot be the center of the Universe, because on the side where the earth is covered by water, which is heavy and covers the earth, it must push and move away from its locus toward the side not covered by water, since a heavy body, such as water, moves downwards if unimpeded . . . It is true that water has weight even in its natural locus. However, the gravity of water does not suffice to displace earth from the center, since the gravity of earth is much stronger and will resist. The arguments[23] would be convincing if water were as heavy as earth.

The insights of Walter Burley and John of Jandun, still vague and rather incoherent, are organized and developed into an expansive and powerful theory, the achievement of Albert of Saxony.

Albertus de Saxonia, to whom the Scholastics often refer simply as Saxonia, is certainly one of the most powerful and original thinkers of the School. Unfortunately, we know very little of his life, but all of his writings are known to us in numerous editions.

His place of birth, Saxony, is indicated by his surname. We also know with certainty that he resided and taught in Paris for a time. A manuscript[24] in the Vatican Library, the Codex Palatinus No. 1207, contains this reference:[25]

Explicit tractatus de proportionibus Parisius per Magistrum Albertum de Saxonia editus. Deo laus.

Albert must have composed his *Questions on the Physics of Aristotle* in Paris for in a passage,[26] where he wishes to give the example of a stone falling into water, he assumes this stone as thrown into the Seine.

We can also date this above information. The Bibliothèque Nationale[27] has a manuscript copy of the *Questions* on the *De Caelo* of Albert of Saxony, and this copy is dated 1378. The History of the University of Paris[28] mentions an Albert of Saxony who fits all the above descriptions accurately. He taught philosophy brilliantly at that University between 1350 and 1361. The *Registers of the English Nation* of the Faculty of Arts at the University of Paris[29] mention that he presided over exams in 1352, 1354, 1355, 1358, and 1359. The *Historia* of Du Boulay asserts that on several occasions he was Procurator for the English Nation. According to the same author, Albert of Saxony was elected Rector of the Academy in June 1358. In 1361, the University entrusted him with the parish S.S. Côme and Damien.

Those are the known biographical details on the author we are concerned with. J. T. Graesse,[30] J. C. Adlung[31] and U. Chevalier[32] identify him with Albert of Rückmersdorff, Rector of the University of Vienna in 1365 and Bishop of Halberstadt from 1366 to 1390, the year of his death. However, this identification is anything but certain.

Many details concerning the life of Albert of Saxony remain obscure. We do not know, for example, whether he was a member of the secular or regular clergy. Some of his editors refer to him as an Augustinian, some as a Dominican and others as a Franciscan. Many of them refrain from mentioning whether he belonged to a religious order or not.

When Chevalier,[33] using Sbaralea[34] as a reference, mentions another Albert of Saxony, surnamed Albertutius, allegedly a Franciscan monk of the 15th century, we, for our part, believe that this Albertutius is one and the same as our Albert of Saxony and we offer proof to support this opinion.

Nicolo Vernias de Chieti, a native of Vicenza, taught philosophy at Padua at the end of the 15th century where he wrote a book in 1499 entitled *De gravibus et levibus quaestio subtilissima.*[35] In 1516, when the editor published in Venice the *Quaestiones super libros de physica Auscultatione*[36] of Albert of Saxony, he appended to it this short work of Vernias. The author mentions and refutes the views of Albertutius, who had attributed the motion of projectiles not to the agitation of the air, but to an impetus. Such is, indeed, the opinion maintained by Albert of Saxony in many arguments throughout his *Questions on the Physics* of Aristotle and *On the Heavens.* It is most certainly Albert of Saxony, whom Nicolo Vernias believed he was quoting. Furthermore, he not only calls him Albertutius, but also Albertus parvus,[37] reserving the name of Albertus for Albertus Magnus.

We find similar information in a collection of commentaries to Aristotle's *On Generation and Corruption*, by Gilles Colonna (Aegidius Romanus), Marsilius of Inghen and Albert of Saxony.[38]

In this collection, Paulus of Genocano of the Augustinian Order implies at the end of the *Questions* of Aegidius and Marsilius that he revised the edition. He must have also done the same for the *Questions* of Albert of Saxony so that we can attribute to him the note in folio 132 col. a, in which the reader is informed that the *Questions* of Albertucius are concerned with the same texts as the *Questions* by Marsilius of Inghen.

During the 16th century, Albertutius or Albertucius was unquestionably accepted as the surname of Albert of Saxony who taught at the Sorbonne during the middle of the 14th century, so that editors sometimes coupled the two names on the title pages of their published works, as witnessed by the following title:[39]

Logica Albertucii perutilis. Logica excellentissimi sacrae theologia professoris Magistri Alberti de Saxonia ordinis divi Augustini, per Magistrum Aurelium Sanutum Venetum. Venetiis, aere ac sollertia heaeredum O. Scoti MCXXII.

The *Tractatus proportionum*[40] by Albert of Saxony, his *Acutissimae Quaestiones* concerning the *Physics, On the Heavens, On Generation* *and Corruption* by Aristotle enjoyed great popularity in the School at the end of the Middle Ages and throughout the Renaissance. The printing presses distributed them widely.[41] The works of Albert of Saxony are one of the main channels by which the physics of the Scholastics transmitted its ideas to the scientific community of the 16th century. His theory on weight can be found scattered throughout the *Questions on the Physics* or *On the Heavens.*

In an early passage,[42] he is supporting the opinion of Aristotle, according to which a body falling in a vacuum would move at an infinite velocity because the body does not possess on its own any intrinsic resistance to motion. There is nothing analogous here to what we call today inertia.

Certain thinkers were of the opinion:[43]

> ... that the different parts of the same falling body would impede each other because each of them would have the tendency to descend by the shortest path. But, since only the middle portion can descend in this line, it hampers the lateral parts. Thus, due to this mutual obstruction of the various parts, simple falling bodies move in unison. But this reasoning is untenable.
>
> In the first place, such reasoning claims that each part of some falling weight tends to descend along the shortest path. This is not valid. Each part does not strive to join its center with the center of the Universe which would be impossible. It is the whole which descends in such a manner that its center becomes the center of the Universe and all the parts strive so that the center of the whole becomes the center of the Universe. Therefore, they do not obstruct each other . . .

To this argument where we ascertain an initial formulation of the theory confronting us now, Albert adds others, including the following:

According to this view, ". . . a large body would descend more slowly than a smaller body which is, all other things being equal, not accurate . . . Ten stones joined together would descend more slowly than any single one, because they would obstruct each other's motion. However, this is inaccurate and contrary to experience."

When Benedetti demonstrated[44] that all bodies with the same specific weight fall with the same velocity within the same medium, he took great care to stress the originality of his discovery:

> This truth, he said, does not stem from Aristotle, nor from any of his commentators whose works I had occasion to read nor any of those with whom I was able to converse and who are in agreement with this philosopher.

It is plausible that the passage by Albert of Saxony just quoted might have been the seed which germinated in the mind of Benedetti.

The problem of the natural locus of the earth is paramount for Albert of Saxony in various passages of the *Questions on the Physics* and *On the Heavens*. According to Peripatetic philosophy, each element has its corresponding natural locus. In that locus, the substantial form of that element acquires its perfection. It is so disposed that it can receive as much as possible favorable influences while avoiding destructive actions. If an element is outside its natural place, it tends to return to it because each form strives toward perfection. When it is in its natural place, it remains at rest and can only be moved by being subjected to a violent action.

What are the natural loci of the various elements? What is, above all, the natural locus of the earth? That question was keenly debated within the School.[45]

For some, the natural locus of the earth was the concave surface which delimits the bottom of the sea — the natural locus of water; or more precisely, this surface added to a part of the lower surface of the atmosphere — the natural locus of air; and these commentators proved to be faithful interpreters of Stagirite doctrine, according to which the *locus* of a body is the internal surface of the bodies surrounding it. Others rejected this view. The internal surface of water is not the natural locus of the earth, because then a piece of earth surrounded by water would remain in equilibrium. However, if one throws a stone into a river, far from remaining at rest it descends until it reaches the bottom of the river. No piece of earth, free of all constraints, could remain at rest as long as it had not reached the center of the Universe. Thus the center of the Universe must be the natural locus of the earth. The supporters of the first view responded to this by saying that the earth, not being a single point, could not naturally rest at a point, even if this point were the center of the Universe.

Albert of Saxony applies his theories on gravity in particular to the resolution of this debate. He formulates the following thesis in the attempt to preserve the portion of truth contained in each of the two opposing views:[46]

> Earth, limited in part by the concave surface of air, in part by the concave surface of water, occupies its natural locus when the center of gravity of the earth is at the center of the Universe. If earth were outside the surface which so delimits it, it would descend and would move until the center of the aggregate which it forms with all other falling bodies became the center of the Universe, unless prevented from doing so . . .
>
> To this I shall add several remarks: First, if the entire mass of the earth were placed outside of its natural locus, as, for example, inside the concavity of the orbit of the

moon, and if it were held there by force and if, from elsewhere one dropped a heavy body, this body would not move towards the mass of the earth, but it would move in a straight line towards the center of the Universe. The reason for this is that once it had reached the center of the Universe, it would be in its natural locus, provided its center of gravity were the center of the Universe. Any being whose motion is not restrained tends naturally to move to its natural locus, because there it can persist longer and is furthest from anything inimical to its natural locus, because there it can persist longer and is furthest from anything inimical to it. Thus we can conclude that falling bodies moving towards the earth do not do so because of the earth, but because by moving toward the earth, they approach the center of the Universe.

However, it is now appropriate[47] to make two distinctions. The first one is as follows: there are two points which can be called midpoints or centers of falling bodies: the center of magnitude[48] and the center of gravity. In those bodies in which gravitational pull is not uniformly distributed, the center of gravity is not the center of magnitude, while in bodies with a uniform gravitational pull, the center of magnitude will indeed coincide with the center of gravity.

The second distinction is as follows: To say that a body is at the center of the Universe can be understood in two different ways: first, that its center of magnitude is at the center of the Universe; second, that its center of gravity is at the center of the Universe.

Therefore, I assume that the earth does not have a uniformly distributed gravitational pull. This is obvious, because the part not covered by the ocean and exposed to the rays of the sun is more dilated than the part covered by water. Furthermore, if its center of magnitude coincided with its center of gravity and consequently the center of the Universe, the earth would be completely covered by water.

From this, we can draw two conclusions: First: It is not the center of magnitude of the earth which is at the center of the Universe . . . Second: it is the center of gravity of the earth which is at the center of the Universe. We shall prove it: All the parts of the earth tend towards the center because of their gravity. Thus, if any given plane passing through the center of the Universe did not divide the earth into two parts of equal gravity, the heavier part would push the lighter until the center of gravity of the entire earth became the center of the Universe. Then, these two parts of equal weight would remain immobile, even though one would have a larger magnitude. They would counterbalance each other just as two weights in equilibrium.

Thus we have a paradox:[49] When the earth is in its natural locus, the different parts of the earth are subjected to violent action and they are outside of their natural locus. Indeed, each of these parts would naturally be at rest if its center of gravity were at the center of the Universe. However, it is the center of gravity of the earth which occupies this position.

It is clear that Albert of Saxony resolves this paradox just as Walter Burley[50] had done earlier, by using the reasoning which he had already developed to prove that the different parts of a falling body do not restrain each other in their motion. It is not that each part of the earth strives to have its center of gravity reach the center of the Universe. This tendency belongs solely to the earth in its entirety. More precisely, the aim of each part is to have the center of gravity of the whole reach the center of the Universe.[51]

Water, he says, does not form the natural locus of the earth as long as the center of gravity of the earth is not the center of the Universe. It does not suffice that for a part of the earth surrounded by water to be in its natural locus and remain immobile, because then its center of gravity is not yet the center of the Universe, and the center of gravity of the whole which it forms with the rest of the earth is also not at the center of the World. Thus it continues to descend until the center of gravity of the whole — comprised of that portion of earth and the remainder — is at the center of the Universe.

From this principle stating that the center of gravity of the totality of all heavy bodies strives constantly to occupy the center of the Universe, one can conclude that the earth does not possess absolute immobility which some thinkers attribute to it. Numerous phenomena, as, for example, the heat from the sun's rays, cause a continual redistribution of gravity throughout the terrestial mass and displace its center of gravity.[52]

Indeed, Albert[53] states, the earth moves constantly. Moreover, gravity in the part of the earth facing the sun is less than on the opposite side. Since the sun moves in a circular motion above the earth, this part changes from moment to moment. Thus, in order for the center of gravity of the earth to remain at the center of the Universe while that part of the earth which is lighter is changing constantly, it is necessary for the earth to move continually.

The cause cited here by Albert of Saxony to explain the displacement of the terrestial center of gravity is actually quite insignificant. In another passage[54] he invokes another more gradual, but more important action: the erosion caused by rainfall. And more than one geologist will be astonished at the precision with which Albert delineates the role played by erosion in shaping the earth.

It is quite probable that a given part of the earth moves continually in a straight line. One can easily be persuaded of this by the following reason: from the part of the earth not covered by oceans, a great amount of earth washed away by rivers continually flows into the ocean. Thus the amount of earth increases in the part covered by water while it decreases in the uncovered parrts and consequently, it does not keep the same center of gravity. However, after this shift in the center of gravity, the new center of gravity moves to occupy the center of the Universe, while, at the same time, the former center of gravity moves towards the surface not covered by water. Because of this continual flux and movement, the part of the earth which once occupied the center ends up coming to the surface, and vice versa.

Thus we can show how high mountains developed,. There is no doubt that some parts of the earth possess more cohesion than others. While those parts with weak cohesion flow into the oceans, carried along by rivers, the parts with greater cohesion remain in place and form promontories above the surface of the earth.

Up to this point, Albert of Saxony has only discussed the natural locus of the earth, but has not taken into account the masses of water. How can this theory account for the presence of this mass? Albert's thinking varies on this particular point. The ideas expressed in the *Questions on Physics* are different from those expressed in the *Questions on the Heavens.*

When Albert of Saxony commented on Aristotle's *Physics*, he wrote the following:[55]

[What I have said about the earth alone], must also be understood as being true for the aggregate formed by earth and water. These two elements constitute undoubtedly an integrated and unique gravity, with its center of gravity at the center of the Universe.

Thus, in the *Questions on Aristotle's Physics*, Albert of Saxony teaches that the center of the Universe coincides with the center of gravity of the totality of heavy bodies. It also coincides[56] with the center of lightness of the totality of light bodies.

Since cold is particularly intense at the poles, the layer of igneous element would be much thinner there than at the equator, if fire, constantly generated at the equator, did not flow continually towards the poles. Just as water flows constantly towards the lowest places, allowing the center of all gravity to be at the center of the Universe, so we must also admit that fire flows continually from the equator towards the poles allowing its center of lightness to be at the center of the Universe. One must be aware that at the poles fire is constantly transformed into air, while at the equator, air is continually transformed into fire. And fire continually flows from the equator towards the poles allowing the center of all lightness to be at the center of the Universe, which is also the locus of the center of all gravity.

Thus, according to the view propounded by Albert in his *Questions on the Physics*, one finds at the center of the Universe the *common center of heavy bodies*, — of the earth as well as of water — and the common center of light bodies — of air as well as of fire.

Just as John of Jandun had done earlier, Albert of Saxony rejects this view in his commentary *On the Heavens*:[57]

One will object that the center of gravity of the earth alone does not seem to be at the center of the Universe and that this position is more appropriate to the center of gravity of the totality formed by earth and water. Since a part of the earth is completely covered by water, that water combines with the part of the earth which it covers so as to counterbalance the other part. It must thus push back this other part until the center of the totality formed by earth and water is at the center of the Universe

We shall answer these objections by denying that the center of the Universe coincides with the center of gravity of the totality formed by earth and water. Indeed, let us imagine that all water were removed. The center of gravity of the earth would still be at the center of the Universe . . . because, in essence, earth is heavier than water . . . whatever volume of water might be placed on one side of the earth as opposed to the other, this latter side of the earth would not receive more help than before to counterbalance and push back the other side . . .

It is easy to account for the fact[58]

that a part of the earth protrudes out of the water. Indeed, the earth is not uniformly heavy, so that its center of gravity is far above its center of magnitude. It is much closer to one of the two convex caps which delimit the earth than the other. On the other hand, water which is uniformly heavy and which tends towards the center of the Universe, flows towards the earth cap which is closer to the center of gravity of the earth, so that the other part, the other cap, the one furthest from the center of gravity, remains uncovered (sic).

For Albert of Saxony the theory of gravity was bound up with the contemporary notions of geography.[59] It helped him justify the hypothesis of one terrestial hemisphere covered by a vast ocean. This hypothesis was rendered invalid by the discovery of Christopher Columbus.

The view expressed by Albert of Saxony that the waters of the seas do not exert any weight, any pressure, on the ocean floor appears rather strange to us today. However, this was not a haphazard conjecture. Albert derives the view from his own general principles on the pressure distribution in fluids. These principles, whose profound and lasting influence were demonstrated by Thurot,[60] were meant to answer the following question: does a body continue to have weight when it is in its natural locus?

A heavy body strives invariably and uniformly to unite its center of gravity with the center of the Universe. When the heavy body is placed in its natural locus, this tendency exists as a *potential* or *habitual* condition. In the latter condition the heavy body has the tendency to remain where it is.[61] As soon as one attempts to remove it from this place, the potential weight immediately changes into an actual state and manifests itself in the form of resistance. When the heavy body is placed outside of its locus, the actual weight sets it into motion as long as no obstacle restrains it.

If any support arrests it and holds it outside its natural locus, the weight remains in an actual state. While it is true that it no longer imparts an actual movement to the heavy body, it does produce an actual effort to reduce the violent force restraining the body.

When the various parts of a heavy body — whether solid or fluid — are in their natural locus, when, consequently, the body is in its habitual state and not in its actual state, then these parts neither press upon nor compress the underlying parts.

Albert makes the following objection to those who defend this view:[62]

The lower parts of the earth have more mass than the higher parts. This seems to be due solely to the compression exerted by the higher parts which results from their gravity. To which I respond, says Albert, by stating that if the central parts of earth are denser, it is not due to the fact that they are compressed by the parts above them, because these parts do not exert pressure on the parts below . . .

What is true of the parts of earth also applies to the parts of water:[63]

When the parts of a heavy body do not move towards each other, they do not restrain each other. This proposition becomes clear from the behaviour of water, in which the parts above do not compress the parts below . . .

Thus the ocean floor does not support any load, any pressure from the water above. In any state, be it habitual or actual, the force of the weight retains the same magnitude in the same body.[64]

A portion of earth tends toward its natural locus whether it is placed below or above it.

Without further explanation, this invariability in gravity could not be brought into agreement with the fundamental axiom on which rests the entire statics of Jordanus:[65] *Gravius esse in descendendo quando ejusdem motus ad medium rectior.*

In the preamble printed by Peter Apian,[66] the Peripatetician commenting on this doctrine in the XIIIth century had explained this apparent variation in gravity as due to an admixture of a certain amount of violent action. Albert of Saxony will define much more precisely the meaning to be given to the axiom of Jordanus:[67]

We feel compelled to state that a falling body does not tend to fall along one line rather than along another. If it descends along one line rather than another, it is because of the resistance applied to it . . . However, one will say, it seems that a falling body tends to descend along a perpendicular rather than along an oblique line. Moreover, we see that when a falling body descends along a perpendicular, it is more difficult to stop or impede its descent than when it descends along an oblique line. It seems obvious that this is indicative of a greater tendency to descend along a perpendicular rather than along an oblique line.

To this, I respond that a falling body is, indeed, more difficult to stop when it descends along a vertical line than when it descends obliquely. However, the reason for this does not lie in a greater tendency to descend along a vertical line rather than an oblique line. It is due rather to the fact that a heavy body encounters less resistance descending vertically than obliquely, as would be the case on an inclined plane. It is indeed more difficult to impede the displacement of a given motor force with a lesser resistance than with a greater resistance.

Thus, if a smaller effort is needed to stop a heavy body from sliding down an inclined plane than to halt it in free fall, it is because the resistance of the inclined plane is combined with the effort exerted. The resistance exerted by the supports is the true explanation for the effects which the School of Jordanus attributed to the variation in positional gravity.

It is amusing to observe that the arguments used by Guido Ubaldo[68] to refute this notion of positional gravity, are nothing other than a simple development of the arguments expounded by Albert of Saxony.

This is not the only evidence we shall find of the influence of our Scholastic on the Marquis del Monte. During the second half of the XVIth century, when the mechanicians instigated a spirited reaction against the statics formulated during the XIIIth century by the School of Jordanus, they did so not only because of their one-sided admiration for recently unearthed scientific monuments of Antiquity, but also because of the influence exerted upon them by the Schoolmen and, in particular, by Albert of Saxony.

## 4. THE THEORY OF THE SHAPE OF THE EARTH AND THE OCEANS, FROM ARISTOTLE TO ALBERT OF SAXONY

What we have just said about the doctrines of Albert of Saxony demonstrates, furthermore, the degree to which his writings linked the theory of gravity and the whole of statics to suppositions about the center of the Universe, the center of the earth and the center of the sphere of water. Therefore, it should not come as a surprise that we digress here to examine both what Albertutius taught concerning the sphericity of the earth and oceans as well as the sources of Antiquity he used for his doctrines. At any rate, we shall not exhaust this vast and important topic in this brief digression. We shall only examine what is necessary to understand the development of statics.

In order to discover the origin of the theories which concern us here, we must go back to Aristotle and his book entitled *On the Heavens and Earth*, which, for so long, guided the scientific evolution of civilization.

One of the most remarkable chapters in the book *On the Heavens and Earth* is certainly the one in which the Stagirite undertakes to prove the sphericity of the earth.[69] Among his arguments, we find *a posteriori* evidence which presents the sphericity of the earth as a fact. For example, the shape of the earth's shadow during a lunar eclipse or the observation that a traveller proceeding from north to south sees certain constellations descending and disappearing while others previously unknown to the traveller begin to appear. However, this observation is also useful for calculating the dimensions of the terrestial globe and Aristotle presents such a calculation which he perhaps found in Eudoxus.[70] The calculation, to be sure, is totally erroneous, but the fact remains that it is the first known to us.

The study of gravity furnishes Aristotle with an additional *a posteriori* argument in favor of the sphericity of the earth. Aristotle assumes that all falling bodies tend towards the same point, the center of the Universe. The trajectory of the fall of a heavy body — the vertical — which varies in direction from one point to another on the earth is always perpendicular to its surface. Thus this surface must be of a spherical shape.

Aristotle's study of gravity also provides him with an argument of a different nature, an *a priori* argument called at that time a physical proof but which we call today a mechanical proof. This proof seems so important to him that he puts it first.

The earth, says the Stagirite, must have a spherical shape. Indeed, each of its parts is endowed with weight and tends towards the center of the Universe. If a lighter part were pushed down by a heavier part, it could not escape, but would be compressed and in yielding to the pressure would end up at the center. One must understand that what is happening is identical to what would happen if the earth were shaped the way certain physicists imagine it to be. However, these physicists claim that the earth owes its origin to a violent downward projection of bodies. In opposition to this view we must formulate the true doctrine and state that this effect is produced because every weight tends naturally towards the center. Thus, when the earth existed only as potential mass, its various parts, separated from each other, were ubiquitous and were carried towards the center by a common tendency. Thus it does not matter whether parts of the earth were once separated from each other and came from the extremities of the Universe to unite at the center or whether the earth was formed in a different way the result will be exactly the same. If parts originating at the extremities of the Universe but travelling to the center from all sides in the same manner, unite at the center they must necessarily form a similar mass on each side, because if the parts accumulate uniformly in every direction, the surface which delimits the resultant mass will have to be equidistant to the center at each of its points. Such a surface must have a spherical shape. However, the explanation for the shape of the earth would not be different even if the component parts did not accumulate in equal quantities from all sides. Indeed, the larger part will necessarily push a small part in front of it, because both parts have a tendency towards the center, and a more powerful weight pushes a lesser one.

This passage contains in a rather sketchy and vague form the nucleus of a great truth which will continue to develop through the centuries. The earth owes its shape to gravity.

One cannot conclude from its gravity that the earth is spherical, but only that it tends to be. Due to their solidity, the earth's parts support each other and impede each other's displacement. The same does not hold true for water. The fluidity of this element precludes any obstacle to a change in shape. Since its various parts tend towards the center of the Universe, water could only be in equilibrium if its surface formed a sphere concentric to the Universe.

Aristotle fully recognized this truth and he tried to prove geometrically that the surface of the sea is spherical. Stated more precisely he proved that if a plane surface were to intrude on this perfect sphericity, the plane could not persist because the spherical shape would be restored by gravity. The *On the Heavens* formulates these arguments in almost excessively concise terms:[71]

It is evident that the surface of water is spherical if the following hypothesis is accepted. It is the nature of water to flow towards the lowest place and that place is all the lower, the closer it is to the center. Indeed, starting at the center $\alpha$ (Fig. 95), let us draw two lines $\alpha\beta$ and $\alpha\gamma$. Let us connect $\beta$ to $\gamma$. On the line $\beta\delta$ let us draw from point $\alpha$ a perpendicular $\alpha\delta$ and extend it to $\varepsilon$. This line $\alpha\delta$ will be the shortest line one can draw from the center to a point on the line $\beta\gamma$. This point $\delta$ will thus be the lowest point

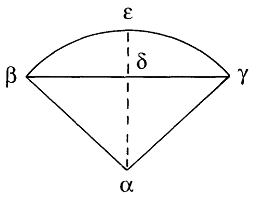


*fig. 95.*

such that the water will flow from all sides towards this point until its surface is equidistant from the center. The line $\alpha\varepsilon$ is assumed equal to the other lines $\alpha\beta$ and $\alpha\gamma$ which emanate from the center. Thus, water must assume the same depth as the length of all the lines emanating from the center before it will remain in equilibrium. Since the locus of the extremities of the equal lines emanating from the center is a circumference, the surface of the water which is $\beta\varepsilon\gamma$ will thus be spherical.

The extreme brevity of Aristotle's reasoning is not completely devoid of obscurity. We shall confront this reasoning again, but in a more explicit and clear form, in the works of Adrastus.

As a personal student of the Stagirite, Adrastus is believed to have lived from 360 to 317 B. C. All of his writings have been lost, but a copy or extensive summary of his teachings concerning the sphericity of the earth can be found in a work by Theon of Smyrna, who lived in a relatively obscure period somewhere between the reign of Tiberius and Antonius the Pious.

In order to prove the sphericity of the earth, Adrastus goes back to some of the arguments of Aristotle, developing and refining them. He first takes up the *a posteriori* arguments:[72]

The sphericity of the earth is demonstrated by reason of the fact that from any place on the earth we can only see half of the heavens, while we consider the other half to be hidden by the earth since we cannot see it . . .

First of all, the earth is a spheroid from the eastern to the western horizons. The rising and setting of the same stars furnish convincing proof. They appear earlier to people living in oriental regions and later to those living in occidental regions. A further proof is a lunar eclipse which takes place within the same rather short interval of time. To those who are able to see it, it appears at different times. The further east one is, the sooner one sees it and the more one sees of it . . .

It is also evident that the earth is convex from North to South, because the inhabitants of the northern region are able to see stars which southerners are unable to see, and vice versa.

To these proofs, Adrastus adds the mechanical reasons given by Aristotle. He develops and refines them in the following terms:

Furthermore, every heavy body tends naturally to move towards the center. Thus, if we were to imagine that certain parts of the earth were at a distance from the center, the smaller parts surrounding them, because of their size, would necessarily be pushed, repulsed and displaced from the center until equality of position and pressure had been reestablished like two balance arms in mutual support or two equally strong wrestlers at grips. If the various parts of the earth are at equal distances from the center, the shape of the earth must be spherical.

Furthermore, since heavy bodies always and everywhere tend to fall towards the center and since everything converges towards the same point and each object falls vertically, that is to say, makes equal angles with the surface of the earth, one must conclude that the surface of the earth is spherical.

Up to this point, Adrastus has restated (although making a few refinements), the proofs of the sphericity of the earth formulated by his master, Aristotle. He then adds: "The surface of the ocean and of still waters is also spherical." And continuing to draw inspiration from the Stagirite, he sets out to justify this affirmation:

Many times, he says, while at sea, it is impossible for anyone standing on the bridge to see land or an oncoming ship, while the sailors perched at the top of a mast are able to see them because they are higher and overlook the convexity of the ocean blocking the view of those below them.

After having presented this rather incomplete but classical proof about the sphericity of the ocean, the Peripatetic philosopher continues as follows:

It is physically and mathematically possible to demonstrate that the surface of any still water must have a spherical shape. Indeed, water always tends to flow from the highest to the lowest elevations. The highest are those furthest removed from the center of the earth, while the lowest are those closest to it.

Like Aristotle, Adrastus supposes momentarily that a part of the ocean is limited by a plane surface. He has no problem showing that on the surface $\beta\delta\gamma$ (see Fig. 95) there would be a point $\delta$ located closer to the center of the earth $\alpha$ than the other points $\beta$, $\gamma$. . . . This point $\delta$ is the end of the perpendicular drawn from point $\alpha$ to the plane $\beta\gamma$. Thus this point $\delta$ is lower than points $\beta$ and $\gamma$. . . :

the water will flow from point $\beta$, $\gamma$ . . . towards point $\delta$ which is lower, until this point, surrounded by the onrushing water, is as far away from point $\alpha$ as $\beta$ and $\gamma$. Similarly, all the points on the surface of the water will be at an equal distance from $\alpha$. Thus water has a spherical shape and the entire mass of water and earth is spherical.

This first mechanical attempt to determine the equilibrium configuration of oceans gave rise, from Antiquity on, to other similar attempts. Archimedes, in turn, attempted to prove by reference to gravity that the surface of still water is a sphere with a center which is also the center of the Universe. The demonstration by Archimedes seems more erudite than those of Aristotle and Adrastus. However, a more critical view of it will not fail to recognize[73] that it is not based on an accurate notion of hydrostatic pressure. We shall not dwell here on the demonstration given by Archimedes, which does not seem to have attracted the attention of physicists until the XVIth century. Because it was more straightforward than the formulation of the great Syracusan, the approach of Aristotle and Adrastus met with the approval of many philosophers. We have mentioned how Theon of Smyrna had preserved the argumentation of Adrastus. We find a trace of this proof, although it is blurred and faint, in the *Pneumatics*[74] of Hero of Alexandria. Pliny the Elder who was in all probability almost contemporaneous with Theon, presents[75] in a rather sketchy and summary form the mechanical proof of the sphericity of the oceans, as formulated by Aristotle. He admires "the geometrical subtlety exhibited by the Greek inventors in creating this fortunate and glorious doctrine."

To this physical proof of the sphericity of the oceans, Pliny adds yet another, which does not stem from Aristotle and which he must have read in the works of some other Greek philospher. It is astonishing to see, he says, that water spontaneously assumes the shape of a sphere:

> . . . and yet, there is nothing more obvious in all of nature. Everywhere, drops which are suspended shape themselves into small spheres. When thrown into the dust or deposited on the fuzzy surface of leaves, they present themselves to us in perfect sphericity. In a full container, the liquid is higher in the middle. We base this conclusion on the lack of density and consistency in the liquid rather than on direct observation. Indeed, even stranger is the fact that the liquid in a full container overflows when even a minute quantity of liquid is added. However, it does not overflow when one lets weights slip into it which can be as much as twenty dinars. In the latter case, the weights added only increase the convexity of the liquid; in the former case, the already existing convexity causes the liquid to overflow at once.

We know today how erroneous these comparisons are which do not distinguish between phenomena due to the action of gravity, and those due to the effects of capillarity. However, should we reproach those physicists of Antiquity or the Middle Ages for not having grasped clearly the distinction between these two categories of phenomena? Didn't we often encounter just a few years ago, physicists who due to a very similar confusion, were seeking in the experiments of Plateau on the phenomena of capillarity an explanation for the rings of Saturn and a proof of the cosmological system of Laplace?

In the *Almagest*, Claudius Ptolemy formulates[76] rather unconvincingly proofs for the sphericity of the earth and of the oceans. He does not allude to the *physical* demonstrations of Aristotle and Adrastus. In support of the spherical shape of the oceans, he reasons that water, being a homogeneous element, must confer upon the whole the same shape as its parts. He must have wanted to deduce the sphericity of the oceans from the sphericity of liquid droplets. At least, this is the way many of his commentators have understood him.

Simplicius develops at length[77] what Aristotle had said about the shape of the earth. Using the calculations of Eratosthenes, he corrects the dimensions which the Stagirite had attributed to our globe. He explains[78] clearly and explicitly the reasoning by which the spherical shape of the oceans is proven in the *On the Heavens*. To this proof, he adds several lines which very obviously resemble the following passage in Pliny the Elder:

> The following observation leads us to conclude that the surface of water is spherical: when drops of water fall upon a smooth surface, such as the blade of a reed or the leaf of a tree, they roll themselves into a ball and, once this spherical shape is achieved, they remain in equilibrium . . .. If one fills a chalice with water and introduces carefully into the water coins or other objects, one can see that the surface of the liquid assumes a spherical shape and that the water only overflows when it has exceeded the surface of the sphere.

Averroes, whom Scholastics call the "Commentator" *par excellence*, merely expands upon what Aristotle had said about the shape and dimensions of the earth[79] and about the spherical shape of the oceans.[80]

We come now to the XIIIth century. In his treatise, *On the Sphere*, which will long remain the most widely circulated cosmography, John of Sacrobosco merely gives the previously quoted proofs of Claudius Ptolemy on the subject of the sphericity of the oceans.[81]

> That water is taut, he says, and that it tends towards sphericity can be demonstrated in

the following way: let us place a light on the beach and let a ship depart from the harbor until it reaches a point where the eye of an observer standing at the foot of the mast can no longer see the light. If the ship then stops and the observer climbs to the top of the mast, he will be able to see the light once more. . . . Another proof: since water is a homogeneous element, the whole is identical to its parts. Since the individual parts of water naturally tend towards a spherical shape, as one can see in droplets of water or in the dewdrops on the grass, the whole formed by these parts must also tend towards a spherical shape.

The philosophers and the physicists who commented on Aristotle hold more tenable opinions on the sphericity of the earth and the oceans than those held by their contemporaries — the astronomers of the XIIIth century. As early as the first book of his *Meteorology*, Albertus Magnus gives an explanation for the sphericity of water drops which makes no analogy between this phenomenon and the shape of the oceans. Albert declares that the water drops assume this shape because their various parts, since they are more intimately connected, better resist the forces of disintegration. In his *On the Heavens*, he merely imitates Averroes in padding the argumentation of Aristotle.[82]

Without adding anything to the arguments of Aristotle, St. Thomas Aquinas explains with great clarity and fidelity the arguments concerned with the shape of the earth[83] and those concerned with the shape of the oceans.[84]

Roger Bacon, as well, adheres to the formulation of the mechanical proof by Aristotle[85] as far as the sphericity of the oceans is concerned. He adds this corollary which found such favor in the School:[86] a vase contains less liquid the further it is removed from the center of the earth.

We must wait until the XIVth century for the teaching of Albert of Saxony to have the Peripatetic doctrine on such questions enriched by several important additions.

When Albert of Saxony examines whether the "whole earth is spherical"[87] he undoubtedly has before him the text of Aristotle as well as the commentary of Simplicius. However, he also consults the text of Theon of Smyrna or a formulation inspired by that text. This is clear for several reasons.

If we read, for example, the proofs for the sphericity of the earth in the *Questions* of the venerable Scholastic Master, we find the arguments of Adrastus arranged in the same order in which they were presented by Theon of Smyrna:

*First Conclusion*: The earth is not strictly spherical, as evidenced by its numerous mountains and valleys.

*Second Conclusion*: The earth is round from East to West. We shall prove it. Indeed, if such were not the case, the same stars would rise and set as early for those living in the West as for those living in the East . . . This conclusion is false. Day and night occur earlier for those living in the East than for those living in the West. This clearly results from the often observed fact that the same lunar eclipse seen by Orientals around the third hour of the night is seen by Occidentals around the first or second hour, depending on how far west they are from the Orientals. This would not occur if night did not begin earlier for the Orientals.

*Third Conclusion*: In the same way, the earth is round from North to South. We shall prove it. If a traveller moves a sufficient distance from North to South, he will see the pole noticeably rise and this can only be a result of the bulging of the earth between north and south. Furthermore, a traveller can move just enough from North to South to see certain stars which were not visible to him before. At the same time, certain constellations will go out of sight which were previously visible to him. This can only be due to the bulging of the earth between North and South.

*Fourth Conclusion*: The earth is round to the degree that the elevations of the mountains are small and negligible when compared with the entire earth. We shall prove it. First, because when heavy bodies fall on soil which is not that of a mountain or of a valley, they all fall at 90° angles. This would not occur if the heavy bodies did not tend towards the same center. And since all of the parts of the earth are heavy bodies, it follows that all of them tend towards the same center. This would not occur if the earth were not round or did not naturally tend towards sphericity. Secondly, the parts of the earth all tend equally towards the center of the Universe. They descend to the lowest places, unless one sustains another as in the case of the mountains. Nevertheless, in time, all things will descend towards the center of the Universe. This seems to be the cause for the sphericity of the earth. From this, one can see that if the earth were as fluid as the water, so that its various parts could not support one another, it would flow into a uniform roundness and a perfect sphericity.

Up to this point, Albert of Saxony has merely given Scholastic form to the arguments formulated by Adrastus in support of the sphericity of the earth. To this, he adds the argument deduced from the shape of the earth's shadow during a lunar eclipse which Aristotle had mentioned, but which Adrastus had left out. He then adds the following passage:

As far as this conclusion is concerned, one must recognize that the sphericity of the earth — as least from north to south — can be determined by experiment. Let an observer leave a given location and move northward until the pole appears to him to be higher by one degree than before. And let him measure the distance covered to that point. After having done so, let him return to his point of departure and from there, move south until the same pole appears to him lower by one degree that at his point of departure. Let him again measure the distance covered to that point. If the two distances are equal to each other, it is an accurate indication that the earth is circular from north to south. If, on the contrary, it turns out that the distances are not equal, it would be an indication that the earth is not round from north to south.

The Ancients had discovered in the measurement of the arc of one degree the means by which to determine the circumference of the earth assumed to be spherical. We have seen that this method was already known to Aristotle, who might have learned it from Eudoxus. However, the measurement of one degree of arc above the meridian taken at different latitudes could have helped determine the actual shape of the globe but was an idea which seems not to have occurred to the astronomers of Antiquity.[88] The passage which we quoted above from Albert of Saxony shows that XIVth century Scholastics had formulated it precisely. It was up to the science of the XVIIth century to initiate its implementation.

Let us add that Albert of Saxony in no way imitates those who seek in the phenomenon of capillarity a reason for the convexity of the oceans. In the last of the *Questions* concerning *On the Heavens*, he considers as one of many objections to be refuted the following proposition which he takes from Ptolemy, from Simplicius and John of Sacrobosco:[89]

In a homogeneous body, the whole must have the same shape as the parts, otherwise it would not be homogeneous. Since particles of water appear to tend towards sphericity, as demonstrated by dew drops and water drops, the entire mass of water must also be spherical.

Albertutius, like Albertus Magnus, responds as follows to that proposition:

As concerns the spherical shape of water drops, I say that it is not at all a consequence of the essential shape of water; rather, it results from the flight of opposites moving away from each other, because this spherical shape is one in which the various parts are closely bonded and capable of best resisting a disruptive force. Thus any mass whatsoever tends to assume that shape, unless prevented from doing so by some other cause, such as hardness or gravity. This tendency is especially apparent when a body exists as a small quantity and it is true not only of water, but of all liquids, as evidenced by quicksilver.

Concerning the sphericity of the earth, Albert of Saxony not only expounded upon the various arguments of Aristotle and Adrastus, already fully developed in one important aspect; he also added to them a series of strange corollaries, seemingly paradoxical and undoubtedly intended to impress the minds of his disciples. These corollaries are, as we shall see, of special importance for the history of the development of statics. Let us quote them at length:

1. Due to the fact that the earth is spherical, it follows that perpendicular lines to the surface of the earth, when prolonged towards the center, will continue to draw closer together until they meet at the center.
2. From the above, it follows that if two vertical towers are constructed, the higher they are, the further apart they are from each other. The shorter they are, the closer they are to each other.
3. If one were to dig a well with the aid of a plumb line, this well would be wider at the opening than at the base.
4. Any line with all of its points at equal distance from the center is a curved line, because if it were straight, some of its points would be closer to the center while others would be further away. Its various points would not be equidistant from the center. Some would not be as low as others. If a straight line touches the terrestial surface at its midpoint, its own midpoint is closer to the center of the earth than its extremities. It follows from this that if a man were to walk along this straight line, he would be walking down towards the center of the earth for awhile and then would ascend away from it. Indeed he would descend, as long as he was headed towards the point which is closest to the center of the earth. He would start walking upwards the moment at which he walked past this point. It is clear that during the initial period, he would continue to approach the center of the earth, while during the second period he would move away from it. To approach the center of the earth, is to descend: to move away from it, is to ascend.

   From the above we can conclude that a body which moves between two points and describes a trajectory which continually rises and falls, can end up having covered less distance between the two points, than if it had moved without rising and falling. This can be clearly seen by supposing that the first trajectory is a diameter of the earth, while the second is a semi-circumference with the diameter as its chord.[90]
5. When a man walks over the surface of the earth, his head moves faster than his feet, because his head, being in the air, describes a larger circumference than his feet which touch the ground. One can imagine a man so tall that his head, far up in the air, would move twice as fast as his feet on the ground.

These corollaries on terrestial sphericity, which were quite capable of catching the imagination of the students at the Sorbonne who clustered at the foot of the chair occupied by Master Albert of Saxony, were to ultimately lead Leonardo da Vinci to his discovery of an important theorem in statics.

## 5. THE INFLUENCE OF ALBERT OF SAXONY IN THE SCHOOL: THEMON JUDAEUS, MARSILIUS OF INGHEN, BLASIUS OF PARMA, PIERRE D'AILLY, GIOVANNI BATTISTA CAPUANO, NIFO, GREGORY REISCH

George Lokert, who, in 1516 and 1518, published two editions of Albert of Saxony's *Questions on the Physics, On Generation* and *On the Heavens of Aristotle*, was certainly in a good position to know the traditions at the University of Paris. In 1516, he was professor of physics at the Collège de Montaigu and in 1518, he taught at the Sorbonne.

In the *Epistola nuncupatoria et paraenetica*,[91] which he puts at the head of his two editions, George Lokert tells us that during the XIVth century three men were outstanding in natural philosophy and formed within the Parisian school a kind of triumvirate. These three men were Albert of Saxony, Themon Judaeus and Jean Buridan. He adds that the Italians and, in particular, the Venetians, were eager to print the works of the first two, but the works of Buridan remained unpublished. The French, more casual, appear to have allowed the works of their most illustrious masters to gather dust. It was in order to remedy this negligence, that George Lokert published not only the commentaries on the *Physics, On Generation and Corruption* and *On the Heavens* by Albert of Saxony, but also the *Quaestiones super quatuor libros meteorum compilatae per doctissimum Philosophiae professorem Thimonem*[92] as well as what Buridan had written on the various treatises which comprised the *Minor Works* of Aristotle. Thanks to the efforts of Lokert, we possess today a precious legacy from the physics taught at the Sorbonne during the middle part of the XIVth century.

Who was this Themon? Du Boulay provides us with some basic information on Judaeus.[93] He tells us that he was a scholar in the town of Münster in Westphalia and that he commenced his studies in the Arts at the Sorbonne in 1349, under Master Dominique de Chivasso. On August 26, 1353 he was elected Procurator of the English Nation, an office entrusted to him again on November 18, 1355.

> He was a most celebrated professor of philosophy; we have read that a good many students who started their studies with him went on to earn their degree and completed their studies with him.

Younger than Albert of Saxony, Themon Judaeus obviously followed the teaching of this master. Evidence of those teachings can be found throughout the *Questions on Meteorology* in which the commentaries written by Albert of Saxony on Aristotle's *On the Heavens* are explicitly quoted and discussed.

The thoughts of Themon Judaeus do not always possess the logical coherence which characterizes the teachings of Albert of Saxony. Sometimes they vacillate between two opposing opinions. However, they are ingenious and original. On many questions on physics, Themon goes further than his predecessors and with greater accuracy. The solutions which he proposed and the hypotheses which he formulated greatly influenced the development of physics during the Renaissance. The discovery of many a truth unhesitatingly accepted today was stimulated and prepared by his investigations.

The *Questions* of Themon Judaeus on the *Meteorology* of Aristotle deserves an in-depth study, but this is not the place to undertake such a study. We will concentrate on those statements of our author which are directly related to the tendency of the center of gravity of every body to move towards the center of the Universe.

Themon is familiar with the teachings of Albert of Saxony. He also knows the two doctrines of this master. The first one, formulated in the *Questions on the Physics*, asserts that the center of the Universe is occupied by the common center of falling bodies, be they water or earth. The second doctrine, formulated in the *Questions on On the Heavens*, maintains that the center of gravity of solid earth alone is located at the center of the Universe. Themon wavers between these two doctrines. Sometimes he adheres to the first, at other times to the second, and such vacillations give rise to contradictions.

In the first book of his *Quaestiones perutiles*,[94] we see Themon maintain, contrary to the theories of Albert of Saxony, that the water of the oceans weighs upon solid earth and that this weight must be taken into account when determining the position of the earth in relation to the center of the Universe.

I imagine, he says, that on the side of the globe opposite us, the ocean penetrates into cavities hollowed out in the earth. Between those cavities arise rocky protuberances which are much heavier than the earth on our side. Perhaps the weight of the water contributes to the gravity of those parts of the earth located outside the center. Thus, thanks to the addition of the water's weight, those parts weigh more than the inhabited parts, although the latter are more voluminous. This is why the convex surface of the latter can be further from the center of the Universe than the convex surface which delimits the water on the opposite side of the globe.

There are philosophers, he says in another passage,[95] who have the following opinion: the earth and the ocean constitute a single body. The center of gravity of this aggregate coincides with the center of the Universe. At the center of the Universe one finds neither the center of gravity of the solid earth, nor that of water, nor the geometrical center but only the center of gravity of the whole formed by earth and water. This view appears to me highly probable and convincing.

Nonetheless, Themon raises somewhat muddled objections which bring him back again to the view supported by Albert of Saxony in his *Questions* on *On the Heavens.*

It appears to me more probable that the center of gravity of the solid earth is at the center of the Universe or close to it. In that part of the globe covered by water, the solid earth is much heavier than it is on our side. As far as water is concerned, although it is naturally heavy, it is less heavy than earth. Therefore, water is merely superimposed on the most dense part of the earth while the lightest part of the earth protrudes through it.

Incidentally, Themon rejects an inadmissible theory with these words:[96]

The following has been proposed: Earth and water are both eccentric to the Universe; that is the reason why the earth is not entirely covered by water, because earth and water are both spherical.

This unacceptable doctrine attacked by Themon stems from Nicolas of Lyre,[97] who presented it in his commentary on the first chapter of Genesis if we are to believe Giuntini.[98]

Against this view of Nicolas of Lyre, Albert contended[99] that the solid earth was more or less spherical, but that its center of gravity, not its geometrical center, was at the center of the Universe. Water, on the other hand, was bounded precisely by a spherical surface with its center coinciding with the center of the Universe. It is this very doctrine to which Themon refers when he writes:[100]

The center of gravity of the solid earth in its entirety coincides with the center of the Universe. It is around this same center that water seeks equilibrium, and therefore, moves towards this center as it can.

Let us imagine for a moment that the earth did not exist and that water was collected about the center of the Universe. Let us imagine then that we submerge the heaviest part of solid earth until the center of gravity of that part occupied the center of the Universe. Because we assume that this terrestial sphere does not possess a uniform gravity and that one fourth of this sphere, for example, is heavier than the rest, this heaviest part would remain near the center [or below it] while the other three-fourths would remain above it. Thus it would be possible for a part of the earth to remain outside of the water because of its greater lightness.

We can see the great influence exerted by Albert of Saxony on his contemporaries in the case of Themon. This influence was particularly powerful and persistent within the School.

In 1386, Marsilius of Inghen was named vice-chancellor at Heidelberg, where he died on August 20, 1396. His *Questions* on the *Physics* of Aristotle,[101] conceived in the same vein as the *Questions* of Albert of Saxony, were constantly inspired by his readings in the latter. Their formulations are often identical. Whether accepted or challenged, most of Albertutius' doctrines on physics can be found here again, often further developed or refined. However, Albert's name has been persistently omitted as we shall have the opportunity to observe many times. Marsilius of Inghen merely declares that he is following the doctrines of the Nominalist School and is dealing with physics *secundum nominalium viam.*[102] Moreover, the works of Marsilius of Inghen are much inferior to those of his predecessor. Sometimes it seems that he restates the views of his predecessor without having sufficiently understood them.

This happens, for example, in the inquiry[103] which Marsilius of Inghen devotes to the following problem: "Is water the natural locus of the earth?"

After having stated in more or less the same fashion as Albert of Saxony the various objections which can be made against this affirmation: "Water is the natural locus of the earth," Marsilius remarks that the difficulty with the question at hand stems from yet another question which has to be answered first: "Why is a part of the earth covered by water and the rest not covered?"

The Rector of Heidelberg then cites several arguments which he rejects. Some, for example (this is the view advocated by Duns Scotus as well as Campanus of Novara at the end of the XIIIth century in his treatise *On the Sphere*), claim that solid earth exists for the welfare of those animals which are unable to live under water. "This answer provides a final cause and not an efficient cause . . ., while we seek an efficient cause and therein lies the difficulty."

"Others answer that earth and water are two spheres which intersect, because they do not have the same center. On the part not covered by water, the center of the earth is higher." This view, as we have remarked, was held by Nicolas of Lyre and Marsilius refutes it, just as Themon had done in his book on *Meteorology*, which the Rector of Heidelberg seems very likely to have read: "The same point is both the center of the Universe and the center of gravity. The entire mass of water as well as the entire mass of solid earth thus have the same center . . . otherwise the habitable earth or, at least, the solid earth would have a circular shape. This deduction is erroneous because the habitable part is longer than it is wide."

After having presented these various views, Marsilius of Inghen formulates the following view in which we can recognize the favorite doctrine of Albert of Saxony:

In this argument we shall first assume that the various parts of the earth do not have the same gravity. Experience shows us that some are heavier than others . . . . From this follows the second assumption that the center of gravity of the earth does not coincide with its geometrical center. Once these assumptions are made, let us imagine that the earth intrudes into the water like a column with its lower part completely surrounded by water while the upper part protrudes and forms what we call the solid earth. Let us imagine, for example, that a nail is in equilibrium at the center of the earth. Only a tiny segment of the nail — the part near the head — would be on one side of the center because the head is much heavier than the rest of the nail. Well, let us assume that the solid earth is similarly arranged with respect to the center and under water.

Marsilius of Inghen rejects this explanation with an argument which is barely comprehensible. He then proposes yet another explanation according to which water, which possesses a very small total mass, fills only certain cavities existing within the solid earth. Let us not dwell any longer on this theory which is obviously less philosophical in nature than that of Albertutius.

One point deserves our further attention for a moment. In presenting this doctrine, Marsilius not only refrains from mentioning Albert of Saxony, he explicitly attributes this theory to Campanus of Novara: "Quinta via est quam ponit Campanus in tractatusuo de Sphaera."[104]

Indeed, in his treatise *On the Sphere*,[105] Campanus deals with the nature of solid earth. However, he limits himself to the affirmation that the surface of the earth is a sphere whose center is coincidently the center of the Universe and that the surfaces of continents, protruding like islands, are further away from the center of the Universe than is sea level. He does not give any mechanical explanations in support of this assertion, but merely invokes a teleological cause, to wit, the needs of animal life.

In a later passage,[106] Marsilius of Inghen asks, like Albert of Saxony, whether a falling body contains an intrinsic resistance to motion. He expounds with great precision the view of those who like Roger Bacon, claimed to find the origin of such a resistance in the tendency in each part of the falling body to move to the center of the Universe and in the constraint that the tendency of each single part experiences from the desire of all others to go to the center of the Universe. Like Albert of Saxony, Marsilius responds that:

> . . . each part of the falling body does not wish to move to the center by following the line which connects each one of them to the center . . . .It is the falling body in its entirely which falls in such a fashion that its center becomes the center of the Universe, or better yet, so that it will join the sum of all falling things with a center which must also be at the center of the Universe . . . . In order for this desire in the falling body to be fulfilled, the center of gravity of that body must constantly be located on one of the terrestial radii.

The section of the book containing this passage by Marsilius is interesting in many respects. We can observe him here first refute a view formulated by the Precursor of Leonardo da Vinci and then appeal to him for a proposition which he claims to have taken from the *Tractatus de ponderibus*. We find here new arguments in favor of the hypothesis suggested to us by our reading of Albert of Saxony that the discoveries of the School of Jordanus were made by mechanicians who, in general, paid little attention to philosophical questions. The Scholastic philosophers were preoccupied, from early on, with reconciling the obvious discrepancies between those discoveries and the principles in Aristotle's *Physics*. This preoccupation produced as early as the XIIIth century the *Peripatetic Commentary* to the *Elements on Weights* by Jordanus. It is evident again during the XIVth century in the *Questions* by Albert of Saxony or by Marsilius of Inghen.

The passages just mentioned are not the only ones in which Marsilius of Inghen alludes to the writings of the School of Jordanus. When he sets out to establish [107] that variations of velocity of a moving body are proportional to the variations of motor force, Marsilius confronts the following objection:

> A heavy body suspended from a balance at times moves faster and at other times slower, even though it remains within the same medium.

He answers this objection in the following way:

> Although the essential gravity always remains the same, there is an increase in accidental gravity due to its position and stemming from the fact that the falling body faces more directly towards the center, which it is approaching more directly than

before. It is this accidental gravity which is called positional gravity as can be seen in the treatise *On Weights*.

In the example of Marsilius of Inghen, we have seen how pervasive the influence of Albert of Saxony was at the end of the XIVth century. We shall see that this influence continued far beyond this period.

For example, during the XVth century, this influence especially affected Biagio Pelacani. One only needs to read attentively the *Treatise on Weights* of Master Blasius of Parma to discover clear evidence of the doctrines of Albert of Saxony.

The third and last part of the *Treatise on Weights* by Blasius of Parma deals with hydrostatics. There is no doubt that the characteristics of specific weight and the use of the aerometer with constant weight which are dealt with here go back to Antiquity. We can find them in the book *On Weights*, erroneously attributed to Archimedes and in the *Carmen de ponderibus*.[108] By the sequence and the form of the questions treated by Pelacani, they appear to have been taken almost literally from Albert of Saxony.[109]

The second proposition of the second part of the treatise by Biagio Pelacani is stated as follows:[110]

Triplum pondus ad aliud in aequilibri positum, medio uniformiter ut unum resistente, subtriplum ad ipsum non levabit.

This proposition and its demonstration are borrowed almost word for word from the *Questions*[111] of Albert of Saxony and Marsilius of Inghen on the *Physics* of Aristotle.

Albert of Saxony denies[112] that the intensity of gravity varies with the distance from the center of the Universe.

The distance from the center of the Universe causes the various parts of a falling body to reach their natural locus while following different trajectories, but the distance will never keep a falling body from tending towards its natural locus.

It seems that this passage, which apparently derives from an argument of Roger Bacon, suggested to Blasius of Parma an idea which he develops further and which we mentioned earlier: Even though each part of a falling body maintains invariable weight, the mutual inclination between these various parts causes the total weight of the falling body to be all the smaller, the closer the body is to the ground. This remark seems to have become canonical in the Schools because we can find it all the way up to the writings of Mersenne and Descartes.

The celebrated Pierre d'Ailly was a contemporary of Blasius of Parma. Born in Compiègne in 1330, he was Grandmaster of the College of Navarre in 1384, Bishop of Cambrai, Cardinal in 1411, Papal legate in Germany and at Avignon. He died in 1420. Among his numerous writings, we find a commentary comprising fourteen questions on the treatise *On the Sphere* of John of Sacrobosco. This commentary is almost always included in those collections of cosmographical treatises which were published quite often at the end of the XVth and at the beginning of the XVIth century.[113]

Pierre d'Ailly formulates the Fifth Question as follows:

> Do the heavens and the four elements have a spherical shape?

In order to answer this question, Pierre d'Ailly reproduces almost word for word what Albert of Saxony had written on the same topic in his *Questions* dealing with *On the Heavens*. However, despite these extensive and quite obvious borrowings from Albert of Saxony, he fails to name their legitimate author. Albert of Saxony is indeed a prime example of one of those misunderstood geniuses whose fertile minds were able to nourish for centuries a science which was unwilling to acknowledge their contributions.

To the seemingly paradoxical corollaries which Albertutius deduced from the sphericity of the earth and the oceans, Pierre d'Ailly adds some of his own. Let us quote some of them.

> He who owns a field bordering on another and who digs up his property and who hollows out a cavity of an invariable depth, wrongs his neighbor.
>
> If the earth were cut by a plane surface with its midpoint at the center of the Universe and if water were to be poured over this surface, the water would tend to take the shape of a hemisphere with its center at the center of the Universe.
>
> Secondly, if the bottom of a pond is flat, this pond is surely deeper in the middle than at its edges.
>
> Thirdly, the same container contains more liquid at a low elevation that at a high one.

These aphorisms, the latter borrowed from Roger Bacon, were meant to strike the imagination. Like those of Albert of Saxony, they were very popular in the Schools. One also finds them in the work of many authors of the XVIIth century.

According to Tiraboschi, Giovanni Battista Capuano of Manfredonia[114] was living around 1475. He was a Canon in the Augustinian Order and was a devotee of astronomy. We have by him an *Exposition* on the treatise of Sacrobosco, which can usually be found in the same collections containing the *Questions* by Pierre d'Ailly.

When Giovanni Battista Capuano lists the reasons why water does not cover the entire earth, he first mentions the following, which happens to be the favorite theory of Albert of Saxony:

> The earth taken in its entirety, does not have a uniform gravity, but it is heavier on one side than on the other. This is so, because one of its sides is denser and thicker and has neither pores nor cavities while the other side is porous and has many cavities. Thus the geometrical center does not coincide with the center of gravity so that the lighter side, which is much further removed from the center of the Universe, protrudes from the water and remains uncovered.

Furthermore, Giovanni Battista Capuano completely misunderstood the reasoning which he repeats. This is obvious when he makes the following objection: "It seems unlikely that the earth in those parts where it is uncovered is light enough to protrude from the water." Even more curious is the following remark by our author: "This explanation has been attributed to Campanus." In the *Quaestiones subtilissimae in libros Physicorum*[115] we have already encountered the crediting of Campanus for a doctrine belonging entirely to Albert of Saxony and which Campanus himself never mentions. We fail to understand why the Scholastics so persistently borrow doctrines from Albert of Saxony and yet so studiously avoid mentioning his name, Moreover, they occasionally replace his name with the name of an author who has nothing to do with their doctrines.

Thus Giovanni Battista Capuano attributes to Campanus a doctrine which is that of Albertutius. Should we assume that he did not read Albert or that he has learned of his ideas through an unknown tradition? How can we believe this when we compare the *Questions* of Albert of Saxony with this passage from Capuano:

> The earth moves constantly in a straight line . . . . The proof of this is at the same time both the reason and the cause. The earth on the side not covered by water is constantly rarefied by the sun's rays and the heat from the stars. It is transformed to vapor and is expanded. This is obvious both from experience and the first book on *Meteors*. Indeed, all vapors rising from the earth come from the uncovered part. But on the side covered by water, the intense cold condenses the water at the bottom of the ocean and changes

it into earth. At the same time, since this is the lowest of all regions, all heavy bodies in the ocean descend to it. The solid earth thus increases constantly on that side, as does its gravity. Thus, on one side some parts of the earth are expended, while, on the other side, new earth is generated so that the center of gravity constantly changes place. The hemisphere covered by water is heavier than the uncovered hemisphere. Therefore, it draws closer to the center and exerts pressure on the other half. Thus the center of the Universe does not remain in the same part of the earth. The part of the earth originally at the center approaches the surface and this movement continues until that part reaches the surface.

Thus the *Questions* of Albert of Saxony were widely read and inspired profound meditation but rarely was his authorship acknowledged by scientists at the end of the XIVth and throughout the XVth centuries. Augustine Nifo (1478—1538) takes his entire theory on gravity from Albert of Saxony. He writes the following based on that theory:[116] "It does not matter if water is at rest or in motion, it is not *deorsum in respectu*[117] as long as its surface is not equidistant from the center. It is only when this condition is fulfilled that air occupies its natural locus. The earth is not *deorsum simpliciter*[118] as long as its center of gravity does not completely coincide with the center of the Universe. Therefore, water will only form the natural locus for the earth to the extent that the earth so placed, is in the middle of the Universe."

Gaëtan of Tiène,[119] like Nifo, does not mention Albert of Saxony. However, in his *Commentaries on Aristotle's Physics*, it is obvious that he has borrowed a great deal from Albert. Although he gives it no credence he does mention Albert's theory on the center of the earth:

Some people imagine, he says,[120] that the geometrical center of the earth is not the center of the Universe. Indeed, the part exposed to the action of the sun and of the stars is very dry and light. And since the center of gravity of the earth coincides with the center of the Universe, it follows that the very dry and light part of the earth is at a much higher elevation than the other part where much water is generated. Thus there is one part of earth which is at a higher elevation than any portion of water.

Gaëtan of Tiène also mentions the theory according to which earth and water have different centers. According to this theory:

Water, freed from any restraint, would tend not towards the center of the Universe, but towards the center of its sphere so that water placed at the center of the Universe and without anything to restrain it would rise naturally to the center of its own sphere.

However, Gaëtan falsely attributes this peculiar theory to Campanus, who had never said anything even remotely resembling this theory. We know that it is the work of Nicolas of Lyre.

In his book on the celestial orbits,[12] Alessandro Achillini of Bologna (1463—1512), makes a very obvious reference to one of the doctrines of Albert of Saxony:

> I propose as a principle, he says, that there are two centers to the Universe: a natural center which is a part of the element of the earth, and a mathematical center, the point which is the center of gravity, if the center of gravity differs from the geometrical center, because the latter may be called the center of the earth, but not the center of the Universe.

To account for all of the traces left by the theories of Albert of Saxony would be an endless task. Towards the end of the XVth and the beginning of the XVIth century, it is almost impossible to open a book dealing with gravity, the immobility of the earth, its position in the Universe, the relationship between water and solid earth, without encountering — sometimes in an obvious form, sometimes in an altered form — the influence of the doctrines which Albertutius had taught at the Sorbonne in the middle of the XIVth century.

We shall not list all of these traces, but merely call attention to a final one, because it was to have a lasting effect owing to the extraordinary popularity of *The Philosophical Pearl* of Gregory Reisch.

At the end of the XVth century and the beginning of the XVIth century Gregory Reisch was Prior of a Carthusian Order near Freiburg.[122] In 1496[123] under the title *Margarita philosophica totius philosophiae rationalis, naturalis et moralis principia dialogice duodecim libris doctissime complectens,*[124] he wrote a kind of philosophical encyclopedia in dialogue form.

This short work which had lumped together so many various theories, was extremely popular. During the XVIth century there were numerous editions[125] and at the beginning of the XVIIth century it was translated into Italian by Giovanni Paolo Galluci.[126] Book VII deals with the principles of astronomy. In chapter XLII of the first treatise, the author examines the distribution of water in relation to solid earth. Concerning this distribution, he propounds a rather strange view, which was to find, nonetheless, many supporters in the XVIth century. He attributes the shape of a sphere to the surface of the oceans and the shape of a smaller sphere to the solid earth. He assumes that this second sphere is entirely contained within the first one, except at one point where both spheres are tangent.

Gregory Reisch supports this improbable view by means of arguments in which we easily recognize a crude and inexact resume of the theories of Albert of Saxony.

The substance of earth and water, he says, forms a single spherical body. The philosophers have attributed two centers to it. The gemoetrical center divides into two equal parts the axis of symmetry of the figure formed by the aggregate of earth and water. It is the center of the Universe. With respect to the center of gravity it must be said that it is outside the geometrical center. It is located on the diameter of the terrestial sphere, this diameter is necessarily longer than half of the diameter of the sphere formed by water and earth together. Otherwise, the center of the Universe would not be inside the earth and nothing more absurd could be stated whether in physics or in astronomy.

It is necessary to distinguish between the two centers, because emerged earth is lighter than submerged earth. When a part of earth emerges, it is at first saturated, but soon it dries and becomes lighter. The center of gravity of the earth cannot possibly coincide with its geometrical center. When located on the diameter of the earth, the center of gravity constantly tends to approach the part of the terrestrial surface covered by water. On the other hand, water flows constantly towards this part, because it is closest to the center of the Universe. From there it follows that the earth is agitated by a constant local movement, because the parts furthest from the center of gravity tend to locate themselves at the same distance as the others. Yet the whole is delimited by a single convex surface and water does not inundate the surface of the earth.

We have to admit that this conclusion is hardly compatible with the configuration which Gregory Reisch attributes to water and earth, as Giuntini[127] correctly remarked. As a matter of fact, the hypothesis of Gregory Reisch is utter nonsense, but it will, nonetheless, exert a profound and lasting influence on the doctrines of geodesy of the XVIth century.

## 6. THE INFLUENCE OF ALBERT OF SAXONY AND LEONARDO DA VINCI

At the beginning of the XVIth century the influence of Albert of Saxony was still very much alive among the Scholastics. However, he exerted no less an influence on those who studied and taught outside the School. Among these, perhaps no one borrowed more from the old Master of the Sorbonne than Leonardo da Vinci.[128]

One of the most important documents among the manuscripts of Leonardo in the Bibliothèque de l'Institut is the notebook which Venturi designated by the letter F. According to a note on the right hand side of the first leaf, this notebook was begun in Milan on September 12, 1508.

On the reverse side of the cover is a list of objects doubtlessly belonging to Leonardo. Among the book titles we find: *Archimedes, de centro gravitatis.*[129] We further read:

> "Albertucco et Marliano decalculatione."
> "Alberto decelo et mundo, da fra bernardino."

M. Ravaisson-Mollien[130] translates these two lines in the following way:

> "Albertucco et Marliano, de calculatione."
> "Albert, de Caelo et Mundo, par fra Bernardino."[131]

What is the significance of these works cited in these brief lines deriving from the hand of Leonardo?

A footnote by M. Ravaisson-Mollien reminds us that Marliani, who was the principal physician to John Galeasz Sforza and who died in Milan in 1483, had written a work entitled: *De proportione motuum in velocitate,*[132] The subject of this work deals with various questions treated by Leonardo in Notebook F. Thus it is reasonable to assume that the work alluded to by Leonardo is the same one referred to by M. Ravaisson-Mollien.

Yet, how is one to interpret the name of Albertucco which accompanies the title of this work? M. Ravaisson-Mollien proposes with some hesitation the following surmise: Leone Battista Alberti. Eugène Müntz[133] asserts that this name is a reference to Alberti.

First of all, one need only make the following observation to cast doubt on the previous interpretation: Leonardo refers to Alberti in other passages,[134] but he does not call him Albertucco, but rather Battista Alberti.

In the Table of Contents of Notebook F, under the word Albertuccius, Ch. Ravaisson-Mollien notes:

> My brother Louis Ravaisson-Mollien of the Bibliothèque Mazarine calls my attention to the fact that one of the two Alberts of Saxony, a Franciscan of the XVth century, was called Albertuccius.

This note gives us the true interpretation of the name Albertucco as written by Leonardo on the cover of Notebook F. This name does not designate Leone Battista Alberti, but rather Albert of Saxony, often referred to during the XVIth century as Albertutius or Albertuccius.

Indeed, the second part of the *Tractatus proportionum* of Albert of Saxony which was reprinted so often at the end of the XVth and the beginning of the XVIth century, bears the following title: *Tractatus de proportione velocitatum in motibus.*[135] Thus it seems evident that Leonardo compared this work to the work by Marliani.

What did Leonardo borrow from the *Tractatus proportionum* of Albertutius and from the treatise *De proportione motuum in velocitate* of Marliani? Undoubtedly, those propositions[136] (which are) all derived from the old Peripatetic axiom that the velocity of a moving body is proportional to the force which moves this moving body. At first sight, it seems difficult to make a formal affirmation, because these propositions were commonly known and had been developed by all of the commentators on Aristotle, from Alexander of Aphrodisias to Simplicius. We are fortunate to have the actual declaration by Leonardo, which enables us to substantiate our view in this matter. In a notebook published after Notebook F, Leonardo writes:[137]

In his *De proportione,* Albert of Saxony says that if a force moves a body at a given velocity, it will move half of this body at double that velocity. This doesn't appear to me to be so . . .

We now know with certainty to whom Leonardo was referring when he wrote the name Albertucco on the cover of Notebook F. But what does the other name mean, i.e., *Albert, de Caelo et Mundo*? M. Ravaisson-Mollien believes that it refers to Albert the Great. Yet, there is nothing in the footnotes of Notebook F which brings to mind any of the physical theories of Master Albert. On the other hand, one can recognize traces of the *Quaestiones in libros de Caelo et Mundo,*[138] written by Albert of Saxony. It is certainly this work which Leonardo had before him and which he had in mind when he wrote: *Albert, de Caelo et Mundo.*

In a previous publication[139] we pointed out some of the clearest evidence of the influence of Albert of Saxony on Leonardo da Vinci. We shall mention here only that evidence which deals with the theory of the center of gravity because that will be ample proof to the reader that Leonardo had read and meditated on the doctrines of the old Master from the Sorbonne.

The following is an early fragment[140] in which Leonardo repeats the essential distinction between the geometrical center of the earth and the center of gravity, a distinction upon which Albert of Saxony bases his entire theory.

On the center of a falling body. Every non-uniform body has three centers, the geometrical center, the center of accidental gravity and the center of natural gravity.

However, if one were to include the center of the Universe, the center of accidental gravity[141] would be omitted.

On non-uniform bodies with a geometrical center and a center of gravity. The center of the Universe can only be thought of as the center of gravity and the geometrical center would be left out.

In another fragment,[142] Leonardo follows the view held by Albert of Saxony and demonstrates how the center of gravity of the earth is constantly changing place.

Because the center of natural gravity of the earth must be at the center of the Universe, if the earth is always becoming lighter in some parts, then any part which has become lighter must push upwards and submerge as much on the opposite side as is necessary for it to connect the aforesaid center of gravity with the center of the Universe.

The earth becomes lighter on the part directly beneath the sun since it is covered only by air — water and snow are absent from this region. On the opposite side, rainfall and snow weigh down the earth, push it towards the center of the Universe and displace the lighter parts from the center. Thus the sphere of water retains its geometrical center, but not its gravitational center.

Albertutius had demonstrated how the earth, through this play of weight, constantly tended towards sphericity. Leonardo returns to[142] these same considerations:

On the earth. Every heavy body tends to move downwards, and things up high will not remain at that elevation but, in time, will descend and, therefore, throughout time, the earth will remain spherical and, consequently, will be covered eventually by water.

Albert refused to draw this conclusion. He had attempted to explain how solid earth would always emerge from water. It is true that he wrote:[144]

Omne grave tendit deorsum nec perpetue potest sic sursum sustineri, quare jam totalis terra esset facta sphaerica et undique aquis cooperta.[145]

But this sentence is to be found among the propositions to be refuted.

Leonardo was more audacious and did not hesitate to assert that the very play of gravity itself would bring about the total inundation of earth. Not only does he repeat word for word[146] the Latin formulation of the proposition that Albert of Saxony had proposed in order to refute it:

Omne grave tendit deorsum nec perpetuo potest sic sursum sustineri, quare jam totalis terra esset facta sphaerica;

but, he also returns insistently to the following prophecy:[147]

If the earth were spherical, every part of it would be covered by the sphere of water. The depths of the oceans are eternal but mountain peaks are the opposite. It follows that the earth will become spherical and be entirely covered by water and will be uninhabitable.

This passage, like so many other reflections inspired by Albert of Saxony, can be found in the *Trattato del moto e misura dell'acqua*. A manuscript copy, kept by the Bibliothèque Barberini in Rome was published[148] by Francesco Cardinali in 1826. It is Chapter XXV of Book I in the treatise.

In this unrelenting work of gravity, which incessantly tends to make solid earth round, erosion caused by rivers plays an essential role. Albert of Saxony has called our attention to this. He pointed out to us how erosion shaped the topography of the land. Leonardo goes back to these considerations, but, as an engineer accustomed to meticulous observation, he explains them[149] as phenomena produced by running water.

If the earth supporting the antipodes were to rise up and would emerge above the sea which is nearly flat, how could mountains, valleys, and rocks of the various layers be created in time?

Water running off mud or sand uncovered from the inundation of rivers teaches us what we have to ask ourselves to understand this.

The water which flowed across the part of the earth uncovered by the ocean — since that earth rose well-above the sea although the latter was almost flat — must have begun to form various streams at the lowest elevation of this surface and those streams, beginning to dig their way through, must have formed receptacles for the surrounding waters. In this way, the streams must have increased in length, depth and breadth the amount of their water until all of the water had run off. And then these hollowed out riverbeds must have become the paths of the torrents formed by rain water and so they must have continued to eat away at the riverbanks until the earth on either side had become sharp mountains which, in the absence of water flowing over them, would become dry and create stones in more or less thick layers according to the thickness of mud which the rivers must have carried with them in their run into the ocean.

Albert alleges, at least in his *Questions* on *On the Heavens*, that it is the center of gravity of solid earth which occupies the center of the Universe. Neither the presence of water in certain areas on the surface which delimits the outer edge of the entire earth nor the absence of water in other areas on this same surface can dislodge this center of gravity. Did Leonardo da Vinci accept this doctrine?

Leonardo knows the principle on which that doctrine is based and he formulates it[150] by summarizing Albert of Saxony:

No simple element possesses either lightness or gravity in its own sphere. And if a bladder filled with air weighs more than when empty, it is because this air is compressed. Fire could also compress in this way until it was heavier than or equal to air and, perhaps, be heavier than water and even equal to earth.

However, it does not follow from the fact that he knew this theory that he also accepted it. In any case, he did not accept it without first contesting the corollary which Albertutius assumed could be drawn from it.

The modification which Leonardo seems inclined to perform on this corollary is quite peculiar. He thinks that water does not weigh down the part of the globe it covers, but, on the contrary, makes it lighter. He believes this proposition follows from the Principle of Archimedes. The following passage contains this strange view[151]

Is the earth covered by the sphere of water more or less heavy, when uncovered? I answer that the heavy body weighs more when in a lighter medium. Thus earth which is covered by air is heavier than earth covered by water . . .

Each of two small sketches represents a pyramid partly submerged in a liquid sphere, partly protruding from it. Next to these sketches one reads:

I claim that with the center of gravity of the pyramid placed at the center of the Universe, the pyramid will change its center of gravity if it is subsequently partially covered by a sphere of water. I give as an example two equal and cylindrical weights. One is half submerged in water and the other entirely submerged in the same water. I claim that the first one is heavier, as has been proven.

Leonardo has substituted for a theory which is in absolute contradiction to the laws of hydrostatics another theory which does not agree any better with the principles of that science.

However, it appears that it was on this occasion that Leonardo made a discovery which gives positive evidence of his talent as a geometer.

The theory of weight developed by Albert of Saxony required constant reflection on the center of gravity of solid bodies. However, research on such centers had hardly even attracted the efforts of geometers. In his immortal work, Archimedes taught only how to determine the center of gravity of plane figures. His research on floating bodies shows us beyond doubt that he knew how to determine the center of gravity of the paraboloid of revolution. However, his method of calculation has not come down to us. While Pappus stated the definition of the center of gravity for bodies with three dimensions he subsequently only deals with plane figures. It is not until the middle of the XVIth century that the works of Maurolico and Commandino inaugurate the study of the center of gravity of solid bodies.

However, as the following brief note[152] shows, Leonardo had done such work a half century before Maurolico and Commandino.

The center of gravity of any pyramid is within the lower one-fourth of its axis. If the axis is divided into four equal [parts] and if two of the axes of this pyramid intersect, this intersection will result in the above mentioned one-fourth.

What was the demonstration which had furnished Leonardo with this beautiful theorem which Maurolico would not rediscover until 1548? We are reduced to mere conjectures which are suggested to us by the drawings accompanying the statement.

With his habitual inaccuracy, Libri wrote:[153]

The drawing accompanying his note proves that Leonardo sliced the pyramids into planes parallel to the base, as is presently done.

In reality, the two figures drawn by Leonardo do not show any trace of such division. In each one, Leonardo has merely drawn the median of the diverse faces of the tetrahedral and the lines which join each vertex with the point of conjunction of the medians of the opposite face. By a demonstration which we no longer have, he proved beyond doubt that the center of gravity of a solid body is situated on the line connecting one vertex to the center of gravity of the opposite face. Thus the center of gravity of a tetrahedron is located at the point of intersection of the four analogous lines drawn from the four vertices.

There can be no doubt that this geometrical problem was present in the mind of Leonardo when he meditated on the theory of weight of Albert of Saxony. Indeed, we saw that when Leonardo discussed the doctrine of Albert on the relation between a solid sphere, its center of gravity and the sphere of water, he considered an analogous configuration where solid earth was replaced by a pyramid. Marsilius of Inghen had done the same by using the image of a nail.

Among the questions examined by Albert of Saxony, there are hardly any which attracted the attention of Leonardo more than the theory of the shape of the earth and the oceans. That is easy to understand, given the fact that Leonardo was also the most erudite hydraulic engineer of his time. Everything concerning the equilibrium and the motion of water in natural environments was of interest to him.

In Notebook F where Leonardo recorded daily the thoughts inspired by his readings of Albert of Saxony, he devotes[154] an entire page to repeating in various formulations the argument of Aristotle and Adrastus in favor of the spherical shape of the oceans.

Proof that the sphere of water is perfectly round. Water does not move by itself if it is not descending, and, therefore, it follows that it is descending when moving by itself.

No part of the sphere of water can move by itself, because it is surrounded by water of equal elevation which confines it so that it cannot escape in any direction. We shall prove it here in the margin.

Indeed, Leonardo draws a circumference of a circle on which he marks a point *c* between two points *a* and *b*; he then adds:

Let *c* be a quantity of water surrounded and confined by the water *ab*. Based on previously established conclusions, I claim that the water *c* will not move because it is impossible for it to descend in accordance with the definition of a circle. Since *a* and *b*, like *c*, are not at the center of the earth, it follows that *c* remains immobile.

The passages just quoted are perhaps a reflection of the thoughts of Pliny the Elder.[155] However, the following passages[156] have a much greater affinity with the demonstrations of Adrastus, as reported by Theon of Smyrna:

Given a plane of water on the surface of the sphere of water, the extremities of this plane will move to its middle.

The spherical falling body placed at the extremity of the perfect plane (Fig. 96) will not stop, but will immediately move to the middle of the plane.

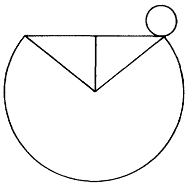

*fig. 96.*

Leonardo refers frequently to the ideas illustrated on this page. The initial formulation given to the proof of the sphericity of the ocean, which seems to reflect the arguments of Pliny, can be found more highly developed in the following fragment:[157]

Any flexible and liquid element has, of necessity, a spherical surface. This can be proved for the sphere of water, but first certain concepts and conclusions must be given.

The higher the object the further it is from the center of the Universe and the lower the object the closer it is to this center. Water does not move by itself, if it is not descending and when it moves, it descends. Let these four concepts, taken in tandem, assist me in proving that water which does not move by itself has its surface equidistant from the center of the Universe (disregarding water drops or other minute quantities which attract one another, such as steel attracts filings, and considering only large quantities).

I claim that no part of the surface of water moves by itself, if it does not descend. Thus, since the sphere of water has no part of its surface capable of descending, it is necessary, according to the first concept, that it not move by itself. And if you carefully consider any minute particle of this surface, you will find it surrounded by other similar particles all at equal distance from the center of the Universe. Moreover, the particle we considered is at that same distance, surrounded by all the other particles. Thus, according to the third concept, the particle of water will not move by itself because it is surrounded by boundaries of equal height. Thus each circle of such particles forms a receptacle in the shape of a circle with its boundaries at equal height. This is the relation of our particle to other similar particles which make up the surface of the sphere of water. It will, in itself, necessarily be without motion. Consequently, since each particle is at equal distance from the center of the Universe, the surface must necessarily be spherical. . .

However, it is no longer the influence of Pliny, but that of Adrastus and Theon through the vehicle of the *Questions* of Albert of Saxony, which is evident in the following passage[158]

If the earth were spherical, no part of it would be uncovered by the sphere of water.

The following passage[159] seems to have been borrowed directly from Pierre d'Ailly:

There will never be any flat earth on which water will not assume a convex shape and be concentrated in the middle of that flat surface. And the water will never move towards the extremities of the plane. Thus, on a perfectly flat surface, water can have various depths.

Figure 97 represents a plane which cuts through a part of the terrestial sphere. On this plane, a mass of water is placed and assumes the shape of a segment of a sphere concentric with the earth. Beneath this drawing, Leonardo writes:

What appears to be flat here is a steep mountain.

He then adds the following:

It is impossible to find any flat part on the surface of any body of water, no matter how large.

The depths of the ocean are eternal, the mountain peaks are the opposite. It follows that the earth will become spherical and entirely covered by water and will be uninhabitable.

This last sentence is a word for word translation from Albert of Saxony.

Not only did Albert of Saxony restate the arguments of Aristotle and Adrastus in favor of the sphericity of the earth, but he also added several paradoxical corollaries based upon that proposition. These corollaries had also attracted the attention of Leonardo da Vinci. The thoughts which they inspired fill an entire page of his notes.[160]

A man who is walking, says Leonardo, repeating what Albert of Saxony had written, is going faster with his head than with his feet. A man who walks across a flat surface leans first forward, then just as much backward.[161]

Albert of Saxony had remarked that if two towers were built with the

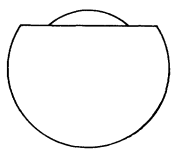

*fig. 97.*

aid of a plumb line, the distance between their tops would be greater, the higher the towers. Leonardo inverts this remark in a certain sense. From a given point of the earth, he draws a vertical line. Then on either side of this point and at a given distance, he imagines that two towers have been constructed parallel to this vertical line and consequently parallel to each other. He shows that these two towers must necessarily collapse if they are tall enough. This passage has great significance and we shall quote it word for word:

If one constructs two perfectly straight towers and if the distance between them remains constant, the towers will undoubtedly fall if construction continues uniformly for both towers.

Let the (Fig. 98) two vertical lines through points B and C continue on outward. If they intersect with one tower along line CG and the second tower along BF, it follows that these lines do not pass through the center of gravity anywhere along their length. Thus KLGC, a part of one of the towers, weighs more than the rest of it, i.e., CGD. And with the parts being unequal, one prevails over the other in such a way that, of necessity, the heavier part of the tower will pull down the rest of the tower. This second tower will do the same, but inversely, to the first tower.[162]

Beneath the sketch reproduced in Figure 98, Leonardo draws another quite similar sketch, where the cylindrical towers are replaced by two very high pyramids, and he writes:

With the axes of the two pyramids parallel, they will fall against each other if they are of a very great height.

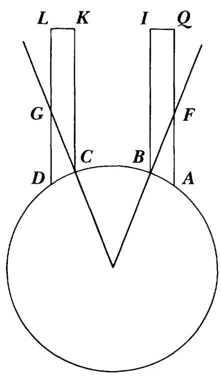


*fig. 98.*

In attempting to reformulate a conclusion of Albert of Saxony in a slightly different way, Leonardo makes use of a theorem which no one before him seems to have stated: In order for a heavy body, resting on the ground, to remain in equilibrium, it is necessary and sufficient that the center of gravity of this body not protrude beyond its base.

In our opinion Leonardo can justly be considered the inventor of this theorem. But it is certainly worth noting that this theorem is only true if one attributes to the gravity at each point of the heavy body, the same magnitude and direction. However, Leonardo discovers this theorem while dealing with a problem where he not only takes into account the convergence of the verticals, but, even more importantly, where he proposes to justify a result of this convergence. In the present chapter we will have many opportunities to return to this matter. Most of the mechanical properties of the center of gravity were discovered by arguments in which the convergence of the verticals played an essential role. However, these arguments were accurate only under the condition that the verticals be considered parallel.

The theorem which we discussed above is of great significance because of its innumerable applications. In the above-mentioned fragment, Leonardo makes a very special application of it, but did he recognize the generality of the proposition which he had discovered in this very special case? We have no doubt that he did.

Leonardo constantly demands of the painter that he have a universal spirit, which he himself had to the highest degree. His mind was universal, but not like those people who amass a large body of disparate facts without being able to establish a connection between them. On the contrary, no one has felt more vividly than he did to what extent the various branches of human knowledge are interrelated. As soon as he had discovered a truth in any field in which he was working, he also recognized how this truth was related to other areas he was studying. At the same time he was drawing from the *Questions* of Albert of Saxony ideas for the *Treatise on the Motion and Measure of Water*, which he planned to write, he was jotting down in his notebook the draft of certain chapters of his *Treatise on Painting*,[163] or returning to the study on the flight of birds, an ever present topic in his meditations. Thus, as soon as the demonstration of the sphericity of oceans has led him to conceive of a property of the center of gravity, he immediately draws from it useful rules for a painter wishing to depict his subjects in a calculated pose, or he deduces from it the explanation for the various behavior of birds.

We have already seen Leonardo commenting on the corollaries of Albert of Saxony and excited about the possibilities for applying them to the upright posture of a man:

A man who walks across a flat surface, leans first backward, then just as much forward.

However, if one wishes to know the full extent of that theorem: A heavy body resting on the ground cannot be in equilibrium when its center of gravity is projected beyond its base, if one wishes to know how he accounts for the differing postures of man and animal, we must leave Notebook F, which up till now has been almost exclusively our concern, and begin to leaf through the notebook designated by the letter A by Venturi.

Notebook A was written after Notebook F and Leonardo sometimes corrects hypotheses there which he had formulated in Notebook F.[164] There is hardly a question dealt with in Notebook F to which Leonardo does not return in Notebook A. In particular, the theory of the shape of the earth and the convergence of the verticals to which the *Questions* of Albert of Saxony had attracted the great painter's attention, are the object of much reflection in the later manuscript.

The following reflection is an almost literal translation of one of the conclusions of Albertutius:[165]

If you build a tower 400 fathoms high and if you use plumb lines, the tower will be narrower at the foot than at the top and will be the beginning of a pyramid.

Furthermore, Leonardo thinks that it would be possible to measure the difference in distance between the two verticals at the top and the base of a tower and deduce from it the length of the radius of the earth.

Among these thoughts, obviously suggested to him by his readings of Albert of Saxony, are reflections dealing with the role played by the center of gravity in statics, such as the following one[166]

A perfectly spherical body, placed on a perfect plane, will not move[167] if you do not impart motion to it. The reason for this lies in the fact that all its parts are at equal distance freom the center. Thus they always remain in equilibrium just as a balance with arms of equal weight and length remain motionless. If the two halves of the above-mentioned spherical body are equal, it too remains motionless.

Leonardo not only applies certain rules from statics to his considerations of the center of gravity, but he also wishes to discover on this point certain properties of dynamics. But dynamics is not advanced enough at the time he is writing for his final intuitions to sense their own truth.

When the center of gravity of a body placed on the ground projects beyond the base supporting this body, the heavy body ceases to be in equilibrium, it moves and falls. And it falls precisely to that side to which the heavier part pulls it — the part containing the center of gravity. Based on this remark, which holds true for a heavy body without an initial velocity, Leonardo claims to establish a general law of motion. He alludes many times in his notes to this law.[168]

Everything, he says, placed on perfectly flat ground in such a way that its pole is not between parts of equal weight, will never stop. An example is furnished by those who are spinning on ice and who will not stop unless the parts become equidistant from their center.

Every heavy body[169] moves to the heavier side ... The heaviest part of bodies moving in the air directs their motion.

The heaviest part[170] of any moving body will direct its motion.

The main goal of Leonardo's stressing the static and dynamic properties of the center of gravity is to explain the behavior of living beings, either at rest or in motion. Consider these reflections, inserted in Notebook A.[171] The first one resolves a problem stated in the *Mechanical Problems* of Aristotle.

A person seated cannot get up without making use of his arms if the part in front of the pole does not weigh more than the part behind the pole.

Whoever climbs to any given place must put a greater part of his weight in front of the foot which is highest rather than behind it: that is to say, in front of the pole rather than behind the pole. Thus man will always put a greater part of his body weight in the direction towards which he wishes to move rather than towards any other side.

Anyone running bends more in the direction in which he is running and puts more of his weight in front of his pole than behind it so that whoever runs uphill runs on the toes of his feet and whoever runs on flat ground first touches ground with his heels and then with the toes of his feet.

Such a runner cannot carry his weight, if he does not establish equilibrium with the weight in front by bending backwards, so that the foot touching the ground is always in the middle of the weight.

Leonardo continues by outlining[172] one of the chapters which will be contained in the *Treatise on Painting*. There we read that when a figure "rests on one foot, this foot becomes the center of the weight above it."

These reflections on the posture of living beings, show the influence of Albert of Saxony and can be found in Notebook A in the brief and imperfect form of a first draft. In order to find them in a more perfect and completed form, one must turn to the *Treatise on Painting*, where many variations can be found on the following proposition:[173]

A man walking will have the center of his weight over the center of the leg which touches the ground so that the weight of the man[174] who is standing on only one of his legs, will always be equally divided on both sides of the perpendicular or central line which supports him.

Any figure[175] sustaining its own weight on the central line of the mass of its body, must place as much of its natural or accidental weight on the opposite side as is needed to establish the balance of the weight equally about part of the central line[175] which starts at the center of that part of the foot [the center of gravity of the man][177] which bears the load. Its line passes through the entire mass of the weight and comes down on that part of the foot which touches the earth.

One often sees a man lifting a load with one of his arms naturally extend his other arm, and if this is not enough to create a counterweight, he adds enough of his own weight to it by bending his body by as much as necessary away from the load in order to carry it. One can also see that a man falling always extends one of his arms and always in the opposite direction . . . We must remark here[178] that the weight of a man's body pulls all the more to the extent that the center of gravity is removed from the center of the axis supporting him.

On could find numerous such quotes which would show Leonardo's constant preoccupation with the position of the center of gravity of a body in relation to the base supporting it.

The Vatican Library possesses a very complete copy of the *Treatise on Painting.* The sketches contained in it, doubtlessly crude imitations of the drawings of Leonardo, show human figures in various positions. In each case a vertical line is drawn through the figure, showing that the center of gravity can be projected to a point within the surface by which the figure makes contact with the ground. The same vertical line was also used in some of the drawings which Nicolas Poussin made for the Italian and French editions which appeared in 1651.

In the *Treatise on Painting,* Leonardo not only makes use of the static properties of the center of gravity; he also includes and applies the dynamic properties he attributes to it and he formulates it as follows:[179]

The arrest or cessation of motion in an animal standing on its feet, comes from the equality or absence of inequality between the opposing weights which support themselves by their own intrinsic weight.

Every motion[180] is produced by breaking equilibrium, i.e., equality, because nothing moves by itself without leaving its state of equilibrium and the motion is all the faster and more violent, the more the object departs from equilibrium.

Here we find once again the thought which Leonardo had rapidly sketched in his notes and applied to ice skaters. For a body to move over a horizontal plane, the center of gravity of this body must protrude beyond the base. The further it protrudes over the base, the more rapid the motion.

It is this principle which Leonardo refers to in the study entitled *On the Motion of Running Animals.*[181]

The fastest running animal will be the one which leans forwards the farthest. The body which moves by itself will be all the faster, the further its center of gravity is from its center of support.

Leonardo most eagerly applies the dynamic properties which he attributes to the center of gravity to the flight of birds:

On the way to establish equilibrium, we read in his notes:[182] the heaviest part of bodies will always guide their motion.

The further development of this thought can be found in the *Treatise on Painting.*[183]

This can be said principally about the flight of birds which, without batting their wings or being helped by the wind, move by themselves. This occurs when their center of gravity is beyond their center of support, that is to say, outside of the center of their wingspan. Since for every bird, the centerline of its wings is further [towards the front or] towards the rear than the midpoint or center of gravity, the bird will direct its motions upwards or downwards depending on how far or near the center of gravity is to the centerline of the wings. That is to say, when the center of gravity is far away from the midline of the wings, it causes the descent of the bird to be very oblique and if this center is close to the midline of the wings, the descent of the bird will be less oblique.

The dynamic properties attributed by Leonardo to the center of gravity provided him with the first solution he proposed to the problem of the inclined plane. He wrote several drafts of this solution which he obtained by a process which seems to reflect the influence of Pappus. The draft which we included in Chapter II and the one included in Chapter V, paragraph 3, can be found in Notebook A next to the devices which Leonardo contrived[184] in order to deduce the radius of the earth from the obliquity of the verticals. On that same page[185] the following principle is stated:

Every object located on perfectly flat ground in such a manner that its pole is not between parts of equal weight, will never stop moving.

The following solution to the problem of the inclined plane is an application of the very principle of which the preceding one is but a particular case:

A body which moves by itself will move all the faster the further its center of gravity is from the center of support.

Let us repeat that the influence of Pappus seems quite obvious in this solution. However, an influence by the *Questions* of Albert of Saxony is not remote either and can be noted in the following remark which precedes the sentence quoted above:

Every heavy body strives to fall to the center and the opposing force which is most oblique offers the least resistance.

This sentence faithfully reflects what Albertutius had written against the notion of positional gravity and the principles of the School of Jordanus.

Furthermore, one can ask whether these attempts by Leonardo dealing with the inclined plane might not have been suggested to him by reading a particular question by Albert of Saxony on the *Physics* of Aristotle. No reference of Leonardo up to this point seems to indicate any influence of that book in which we find the following remark:[186]

Let us assume an empty space between heaven and the earth and a surface equidistant from the center. On that surface let us place two heavy spheres, *a* and *b*. Let us further assume sphere *a* is heavier than sphere *b*. Any force (vertu), however small, could move the two spheres with infinite ease over the surface. We shall prove it: Each of the spheres would touch the surface at a particular point. Thus the weight of the upper hemisphere is counterbalanced by that of the lower hemisphere, like two weights in equilibrium. Since any amount of force, no matter how small, suffices for movement, any force could move each of these spheres with infinite ease.

If a plane were placed obliquely into the space and if one placed a simple and spherical heavy body on the plane, the heavy body would descend on the plane at a finite velocity. This is evident because since it cannot descend on a straight line [to the center], it would descend by rolling. Since one part of the sphere would have to lift the other part the latter would be lifted by violence and would thus act as a resistance.

## Second Period

## From the Copernican Revolution to Torricelli

### 7. THE INFLUENCE OF ALBERT OF SAXONY AND THE COPERNICAN REVOLUTION

As early as 1508, while discussing[187] the view of Albert of Saxony on lunar spots and attempting to formulate his own opinion on this subject, Leonardo was led to reject the geocentric hypothesis and to formulate the following verity:[188]

Now the earth is neither in the middle of the orbit of the sun nor in the middle of the Universe, but rather in the middle of the elements which accompany it and are tied to it.

Thus, in 1508, harbinger signs of the Copernican revolution were already appearing. For an entire year, Copernicus had meditated upon the world system and he was to continue doing so until 1530. Not until 1543 — the year of his death — were the results of his meditations to appear in print. From 1515 on, at the latest, Celio Calcagnini ascribed diurnal motion to the earth, without, however, giving up the geocentric hypothesis.

The Copernican revolution overturned the Peripatetic theory of gravity on an essential point, because it no longer located the center of the earth at the center of the Universe. However, once this reorientation had been accomplished, Copernicus and his followers retained as fas as possible, the laws formulated by the Scholastics and especially by Albert of Saxony. In their view, as in the view of the Doctors of the School, the gravity of a terrestial body is the tendency which this body has to unite with the center of gravity of the earth, a tendency existing in each body so that the earth may remain spherical.

The earth, says Copernicus,[189] is spherical because, from all sides, it strives towards its center.

The element of the earth[190] is the heaviest of all and all heavy bodies move towards it and tend towards its innermost center.

The Scholastics attributed this tendency solely to the component parts of the earth. The Copernicans attributed a similar tendency to the fragments which might detach from the sun, the moon or from any other planet. Each of these fragments tends towards the center of the heavenly body from which it originated, so that the integrity of the heavenly body is preserved:[191]

In my opinion, gravity is nothing but a certain natural propensity given to the component parts of the earth by the Divine Providence of the Creator of the Universe, so that they might converge into unity and integrity, by uniting in a spherical shape. It is probable that this attraction also exists on the sun, the moon, and other wandering stars so that through the effect of this attraction, those bodies persist in their spherical shapes as they appear to us.

The geographical and cosmographical knowledge of Copernicus is too sophisticated for him not to reject certain views held by Albert of Saxony. Copernicus is aware that neither hemisphere of the surface of the globe is entirely covered by water. He also knows that the continents and the seas form an almost perfect sphere and that the direction followed by a heavy body in its descent will end up at the center of this sphere. Thus, just as the Scholastic Doctors, he can only affirm that the geometric center of the earth is outside of its center of gravity and that the latter, to the exclusion of the former, is the center of the liquid sphere. On several occasions, he attacks the following statements of Albert of Saxony, whom he does not name, but whom he must have read:[192]

Water and earth both tend towards the same center because of their gravity .... One should not heed the Peripatetics who claim ... that the center of gravity is distinct from the geometric center. That such a distinction between the geometric center and the center of gravity does not exist, can be shown in the following fashion: the surface of the earth not covered by the ocean does not swell in a continuous way. Otherwise, it would very much restrain the ocean water and could not be penetrated by inland waters, which resemble vast bays .... Because of all of these reasons, it is apparent to me that earth and water strive simultaneously towards the same center of gravity which is not distinct from the center of the earth.

Thus, according to Copernicus, the earth and the oceans form a mass clearly spherical in form, so that there is no need to distinguish the center of the earth's shape from the center of the shape of the surface of the seas.

Both of these points are very close to one another. This hypothesis which accorded very well with all the geographical and astronomical observations was independent of any hypothesis concerning the motion of the earth. Thus it seems that it should have been widely accepted without any difficulties. However, such was not the case and it met with very vivid and prolonged opposition.

The source of this opposition must be sought in the rather bizarre view stated by Aristotle in his book *On Meteorology* and which we can formulate in modern terms as follows:[193]

The four elements, earth, water, air, and fire all have equal mass so that the volume which they occupy is in inverse ratio to their densities.

But, according to many Peripatetics, when a given mass of one of these elements becomes corrupted and through this corruption, it engenders the next element, its volume increases by tenfold. Thus densities of the four elements form a geometrical progression by ten. Therefore, the total volume of water must be ten times the volume of earth. The volume of air must be ten times the volume of water and the volume of fire ten times the volume of air.

This theory, widely accepted throughout the Middle Ages, had given rise to curious geodesic theories such as, for example, the theory of Nicolas of Lyre,[194] which we mentioned previously. From the XIVth century on, we see the Nominalists of Paris reject this particular point in the doctrine based on Aristotle. We see Albert of Saxony advance geodesic ideas quite similar to those of Copernicus. We see Themon systematically refute the hypothesis according to which the volumes of the elements form a geometrical progression.[195]

However, the very reasonable arguments advanced by Albert of Saxony and by Themon were far from meeting with unanimous approval. The assumption of Aristotle was still much in favor at the end of the XVth century. Gaëtan of Tiène, after having mentioned the Aristotelian view, remarks simply[196] that "others think differently and that there is no solution to the question." Others, such as Gregory Reisch, attempt, as we have seen, to adapt[197] the ideas of Albert of Saxony to the hypothesis that water occupies a volume tenfold the volume of the earth.

It can easily be seen that such views could be held until navigators transformed the geographical knowledge of man. It might seem highly improbable that men continued even after Vasco da Gama, Christopher Columbus and Magellan, to claim that the solid earth forms a sphere ten times less voluminous than the ocean and that the solid earth forms a continent with a very small surface in relation to the surface of water; yet, such was the case.

Anyone who is surprised by this strange, intellectual phenomenon does not, in our opinion, have a good grasp of the mentality prevalent in the XVIth century. The main characteristic of many men of science of this excessively praised period is a narrow-mindedness which often extends to sectarianism. Thus, as at all times, we can distinguish among those seeking knowledge, between the innovators and the conservatives. However, the innovators, or those pretending to be so, demonstrate such a high degree of intransigence that they do not wish to retain any of the conquests of preceding ages. Anything remotely attached to Peripatetic Scholasticism seems to them totally false and pernicious. They reject it unexamined and retain only what they have inherited from the geometers of Classical Antiquity. We already met with such innovators who weaken science by cleansing it of any discoveries made during the Middle Ages, when we discussed the reaction of Guido Ubaldo and Giovanbattista Benedetti against the School of Jordanus.

Opposed to these innovators who would like to do away with the work of entire centuries, are the conservatives whose goal is to retain everything of that same work, even that which is blatantly false.

To be sure, in the Scholasticism of the XIIIth and XIVth centuries, the thought of Aristotle is deeply venerated. However, that veneration is far from being a blind servility. The Alberts of Saxony and Themons discuss with respect the views of the Stagirite, but they discuss them and when they believe they have valid reasons, they reject them. During the XVIth century, however, we see the birth of a slavish Aristotelianism, which consists in taking the most insignificant view which one of the commentators believed to have discovered in the Master and considering it as an infallible oracle against which even the most cogent counter-arguments, the most solidly deduced reasonings, the most indubitable facts avail nothing.

Twelve years had passed since the crew of Magellan had succeeded in circumnavigating the globe, when Mauro of Florence (1493—1556), a monk of the Servite Order, repeated the opinions of Gregory Reisch and maintained[198] that the solid earth forms a sphere cropping out of a small portion of the spherical mass of water which is ten times more voluminous. Elsewhere Mauro of Florence repeats a theory which Albert of Saxony had formulated in his *Questions* on the *Physics* of Aristotle but had subsequently omitted in his *Questions* on *On the Heavens*, a theory which, for a certain time, had found the approval of Themon. Mauro of Florence remarks that the aggregate of earth and water form a heterogeneous body with a center of gravity not located at the geometric center. He affirms that it is the general center of gravity which must coincide with the center of the Universe, so that the terrestial sphere and the surface of the oceans are each individually eccentric to the Universe.

Copernicus believes he has accomplished worthwhile work[199] by refuting the theories of Gregory Reisch and Mauro of Florence. He notes that if the sphere of the solid earth were not ten times, but only seven times less voluminous than the mass of water, then the center of the spherical surface which delimits the ocean would be outside the volume occupied by the earth. Thus it could not coincide with the center of gravity of the solid earth, as Albert of Saxony asserted in his *Questions* on *On the Heavens*. It should be noted that while Copernicus appears to admit this doctrine of Albertutius, Mauro of Florence does not.

Cardan, who read Copernicus and quotes him,[200] shares the great astronomer's view of the masses formed by solid earth and water;[201]

> It is not true, he says, that the water is so voluminous or that it forms a very considerable part of the entire earth. In reality, there exists only a very small quantity of water which, due to its lightness, remains on the surface of the earth and fills the lowest cavities of that surface. If we only consider the surface of the water, we might conclude that it was more voluminous than solid earth. However, as soon as we take into account its depth, we can no longer compare the two.

It would be impossible to reject any more clearly the view held by Gregory Reisch and Mauro of Florence.

Alexander Piccolomini was also a convinced opponent of Mauro of Florence. In his treatise on natural philosophy,[202] Piccolomini had expounded on the doctrine concerning the shape of the earth and water, considered classical after Albert of Saxony. Using the demonstration of Aristotle and Adrastus, he proved[203] that water is delimited by a spherical surface with the center of the Universe as its center. The center of gravity of the earth is situated at the same point,[204] but due to the heterogeneity of the earth, this center of gravity does not coincide with its geometric center. However, in the above-mentioned work, Piccolomini does not consider whether water does or does not occupy a much larger volume than earth.

To this problem, he devoted a special work[205] in which he repeats at length all the reasons of Themon and Copernicus to refute those thinkers who attributed to water a volume ten times that of earth, especially Mauro of Florence.[206] Incidentally, his discussion is not without error, as indicated by the following curious example. He thinks[207] that the shadow causing a lunar eclipse is due to the solid element only since water does not cast a shadow because of its transparency.

This work of Piccolomini by no means settled the debate raging among the physicists. In 1578, Giuntini,[208] after having copied whole pages from Albert of Saxony without mentioning him, simple states:[209]

As far as I am concerned, I think that earth and water have the same center which is also the center of the Universe.

In another passage,[210] he proclaims to be the adversay of Gregory Reisch and Mauro of Florence. On the other hand, Antonio Berga defends the view of the latter two in a work[211] in which he sharply attacks Alexander Piccolomini.

The pamphlet of Berga provokes, in its turn, a sharp retort by Giovanbattista Benedetti.[212] In this retort, Benedetti argues sharply against those who attribute to water a volume tenfold that of the earth, and in particular, takes to task Antonio Berga. Benedetti refutes their reasoning with reasons advanced by his predecessors, especially those of Copernicus. He does not mention Copernicus by name, but he had obviously studied him in depth because he often mentions him in his letters[213] and does not hesitate to put *On the Revolutions of Celestial Orbs* on the same level as the *Almagest* of Ptolemy.[214] He goes even further. Without openly declaring himself an adherent of the Copernican system, Benedetti considers it a plausible hypothesis.[215]

In his reflections on the size of land and water, Benedetti accepts without reservation the doctrine of Albert of Saxony as far as the center of gravity is concerned. He formulates this doctrine with great clarity by using the definition given by Pappus on the center of gravity as well as the definition proposed by Commandino.[216]

The ancient philosopher, he says, defined the center of gravity of individual bodies in the following manner:[217] *Centrum gravitatis uniuscujusque corporis est punctum quoddam intra positum, a quo si grave appensum mente concipiatur, dum fertur quiescit, et servat eam quam in principio habebat positionem, neque in ipsa latione circumvertitur.*

Certain modern philosophers, Benedetti says further, define it in the following manner:[218] *Centrum gravitatis uniuscujusque solidae figurae est punctum illud intra positum, circa quod undique partes aequalium momentorum consistunt; si enim per tale centrum ducatur planum, figuram quomodocumque secans, semper in partes aequeponderantes ipsam dividet.*

Still others maintain that the center of gravity of each body is the point at which the body would be united with the center of the Universe, if it were not prevented from doing so.

All agree with the proposition that the earth is united to the center of the Universe through its own center of gravity.

A few years later, Guido Ubaldo, under the influence of Benedetti, will restate just as precisely the doctrine of Albert of Saxony.

Furthermore, like Copernicus and Giuntini, Benedetti believes that the center of gravity and the geometric center of the earth are significantly different.[219]

We are certain, he says, that the spherical surface of water is at all points equidistant to the center of the Universe, which is the point towards which all heavy bodies tend. Furthermore, because of the numerous islands and different countries discovered by navigators in all regions of the world, we can be quite certain that water and earth form the same globe . . . and that the geometrical center of the earth, which coincides with its center of gravity, is located at the center of the Universe.

These statements deserve further attention. In the field of science, Benedetti is one of the most daring and intransigent reformers of the XVIth century. In many instances, he vigorously attacks the *Physics* of Aristotle. On the descent of heavy bodies, he formulates a theory which overturns the Peripatetic theory of gravity. As a great admirer of Copernicus, he is tempted to adopt the heliocentric system. On the other hand, he completely rejects all the mechanics of the Middle Ages and goes so far as to include in his rejection the most beautiful discoveries made by the School of Jordanus, such as the correct solution to the problem of the inclined plane. And yet, such blind contempt for the science of the past hesitates respectfully before a monument of XIVth century physics. That monument is the theory on the center of gravity formulated by Albert of Saxony. This theory, which appears to us today so blatantly false, survives the Copernican revolution and scientific reform with scarcely a modification. Benedetti retains it, just as Copernicus had done before him and as Guido Ubaldo and Galileo will do after him. And despite the attacks of Kepler, the theory will survive until Newton.

The *Considerations* of Benedetti did not suffice to convince those who maintained that the ocean was more voluminous than earth. This view continued to be advanced and discussed up to the beginning of the XVIIth century. In 1580, Francesco Maria Vialardi published a Latin translation of the pamphlet of Antonio Berga and of the *Considerations* of Benedetti.[220] In 1583, Agostino Michele defends again[221] the ancient view which claims that there is more water than solid earth in this world. In a long letter addressed to Horatio Muto[222] in 1584, Benedetti resumed his earlier discussion with Piccolomini and Berga and zealously refutes the arguments of Agostino Michele. The following year, Nonio Marcello Saia joined the ranks of those sharing Benedetti's views.[223]

Eventually, every sensible thinker came to join those same ranks. In 1593, the Jesuits of the University of Coïmbre, who were strict guardians of the Peripatetic tradition in physics, published their commentaries on the *On the Heavens* of Aristotle.[224] Without mentioning the name of Albert of Saxony, they expounded clearly the main points of his doctrine: the distinction between the center of gravity and the geometric center, the coincidence of the center of gravity of earth with the center of the Universe, the vaporization of the uncovered part of earth by solar heat. They conclude by stating that since the height of the mountains and the depths of the ocean are approximately the same, the earth and water form a single globe with a common center of gravity which is at the center of the Universe. Advocates as well as opponents of the Copernican system agree from now on to present the doctrine of Albert of Saxony in the same form. That form, vaguely envisioned by Albert of Saxony, was formulated by Copernicus and Benedetti. Only one difference of opinion separates the two Schools. In the eyes of the advocates of the geocentric system, the point towards which heavy bodies tend to move and where the center of gravity of the earth and the center of the surface of the oceans are located, is at the very center of the Universe. For the followers of Copernicus, this same point is but a point peculiar to the heavenly body called the earth and in each heavenly body there exists an analogous point. From the point of view of celestial mechanics, the difference is crucial. From the point of view of statics, the difference is insignificant.

At the end of the XVIth century, both Copernicus and his opponents agree on the following statement: all heavy bodies which belong to our earth have a common center. Whether this center of gravity is or is not at the center of the Universe does not change the fact that all heavy bodies tend to move towards it, and that each one of the bodies strives to unite its center of gravity with the center common to all heavy bodies. If the body is free to move, it moves in such a fashion that its center of gravity describes the *line of direction*, that is to say, the straight line which joins this point to the center common to all heavy bodies.

Such is the doctrine which ensued directly from the influence of Albert of Saxony and which we find affirmed in the works of Cardan and, even more explicitly, in those of Guido Ubaldo.

## 8. THE INFLUENCE OF ALBERT OF SAXONY AND OF LEONARDO DA VINCI: CARDAN AND GUIDO UBALDO

Leonardo da Vinci frequently quoted the following phrase: The heaviest part of a falling body guides its motion. Cardan makes this phrase more precise: when a heavy body moves naturally, whether it is free or impeded by any constraints, the center of gravity always descends.

Here is how he states this important proposition:[225]

Every part of a heavy body descending in part by natural movement alone, descends by its heaviest part with respect to the center of gravity.

Here is how Cardan comments upon this proposition, which, as can be easily seen, contains the nucleus of the Principle of Torricelli:

Let *a* be the moving body, *b* its center of gravity, *cd* the part of the body closest to the center. If a part of the body touches the earth, I claim that *cd* will descend by a natural motion, because if *a* cannot descend altogether to the center, *b* will. Indeed, the part is of the same nature as the whole. The nature of the entire earth is such that its center of gravity is the center of the whole. Thus *b* also moves to the center by the shortest path and follows *cd* which is the part nearest point *b*. But the part closest to the center of gravity is necessarily the heaviest, because this center is at the center of gravity. Thus, by natural movement, every moving body descends by its heaviest part.

As a result, if a heavy body has parts of unequal shape and substance, and if it is placed in such a way that the heaviest part is not at the bottom, it will necessarily pirouette.

This propensity of the center of gravity of a falling body or of an aggregate of falling bodies is in the eyes of Cardan the sole principle upon which are based all the phenomena of motion and immobility caused by gravity.[226]

Let us state here what is so remarkable on this subject . . . .A heavy body deprived of direction must follow a geometric rule scarcely known by learned men. There is a cause, an obvious cause, for this. Everything which is a heavy body is situated on the line emanating from the center of the Universe. If the midpoint of the [suspended] heavy body is outside of this line, it will turn towards this line which is in it because the center [of the Universe] is always on that line. Thus the unique inclination of the center of the heavy body to locate itself on the line going to the center of the earth and the center of the Universe is a sufficient explanation.

Cardan outlines the principle of his statics in the *Opus Novum*. However, he had attempted to apply it long before to a particular problem. Indeed, if we open the French translation by Richard Le Blanc of the *De Subtilitate*, we can find the following curious passage:[227]

We have discussed things which hold up more than reason seems to admit, as well as things which are mutually supportive. The time has come to show how certain matters seem to be self-supporting.

Let a flat dresser or table be AB (Fig. 98a) and the rod be CE, at the outer end of which hangs the handle of a pail filled with water GHF. Let a straight rod EF be tightly wedged between the rod CE and the bottom of the pail so as not to fall, I claim that the pail will remain suspended and will not fall.

It is clear that Cardan is completely mistaken here and that, if left to themselves, both rod and pail will fall. Nonetheless, the Milanese physician attempts to prove his claim in the following manner. Assuming that the pail has fallen, he pretends to prove that:

the center of gravity is displaced by itself away from the center of the earth. Nevertheless, assuming that it is heavy, it descends by natural movement which can only happen here through impediment. Thus the pail will not descend . . .

*Igitur*, the Latin text reads, which is much clearer than the translation by Richard Le Blanc, *centrum gravitatis elongatum est a centro Terrae sponte, igitur motu naturali grave ascendit, quod esse non potest. Non igitur situla descendit* . . .[228]

The deductions from which Cardan claims to arrive at an obviously absurd corollary, although based on a correct and fruitful principle, are completely untenable. Furthermore, it seems that the great astrologer suspected the falsity of the assertion and the illogic of the reasoning by which he was attempting to prove it. Indeed, his statement ends as follows:

It is necessary (lest the experiment not disappoint you through the mockery of the observers, for if the undertaking does not turn out as desired, the ignorant not only blame the experimenter, but also his demonstration) to pay close attention to the following: first, the surface of the dresser and table must be in equilibrium and the

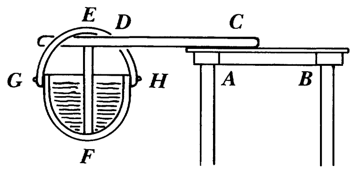


*fig. 98a.*

wood straight, and unbending. Likewise, the stick EF be straight and firmly fixed between the bottom of the pail and CE so that it can hold firmly the wood CE to the handle D. Furthermore, let the point F be the center of gravity and the pail have a round bottom.

Many people will read this, but few will understand it. However, more than is written must be understood and nothing should be omitted which appertains to perfection.

How is one to explain this strange passage of Cardan? One has the feeling that it is the distortion of an accurate argument which Cardan must have found a mutilated copy of and which he hastily inserted in his *De Subtilitate* without understanding it.

However, is it possible to discover the actual configuration of equilibrium of which Cardan has reproduced a distorted description? Several clues can put us on the right track.

This passage by Cardan which we quoted above is not included in the first edition of *De Subtilitate*. The Milanese astrologer included it only in the second edition which was the basis for the French translation by Richard Le Blanc. He retained it in all subsequent editions.

However, this addition is not the only one in Book XVII of the second edition of the *De Subtilitate*. There is another one at the end of this book. Consequently, it occurs to us that these two additions might have had the same origin.

The addition at the end of Book XVII[229] sets out to explain ". . . why man must labor so much when climbing." Among the reasons given by Cardan one finds the following:

The third cause is peculiar to the situation where the slope is very steep. Since we know that a man cannot easily stand upright if he does not stand flat-footed, when he is on a steep hill where the surface is not equidistant to the center of the earth, he is forced when climbing upright to hold himself up with great effort, because the soles of his feet do not fully rest upon the surface. Thus he is forced to do one of three things: either to support himself solely on the forward part of his feet or to bend his entire body forward or hold himself up by greatly distending and extending his muscles, which is a very difficult thing.

Can we read this passage without thinking of the reflections of Leonardo da Vinci on the various postures of the human body and without being reminded, in particular, of the following passage:[230]

A man climbing to a given point must put more weight on his forward foot then on his rear foot, that is to say, more weight to the front of the pole than to the rear?

This leads us to think that we can find in the notes of Leonardo da Vinci the configuration of equilibrium which Cardan presented in such a confused form.

Let us then peruse the notebook containing the passage we just quoted. Five pages beyond that passage we read the following:[231]

The single weight supported at the middle (center of gravity) and from which the rest is suspended, can be of any form whatsoever, because it will always establish an equilibrium on its fulcrum and sometimes the extremities will not be at equal distance from the center of the weight.

Example. Let AB (Fig. 98b) be a ruler which only rests on its extremity at A, while the other part is suspended. This is impossible to achieve if you do not first attach to this ruler the weight C which acts as a counterweight allowing A to remain at the midpoint between C and B, and this weight will come to rest on the pole A.

Similar reasoning can be applied to the device below (Fig. 98c).

Let us focus our attention on this "device below."

Leonardo da Vinci has put at C a given counterweight. If one hangs from it a pail of water, one is obviously faced with the paradoxical configuration of equilibrium which Cardan described to us in such a distorted form.

What can we conclude from this comparison? A hypothesis which we shall formulate now and which will be confirmed repeatedly throughout the remainder of this Chapter.

The sheets of the notebooks which have been in part saved are but a jumble of all of the reflections suggested to Leonardo da Vinci from his readings and meditations. But every so often, Leonardo focuses in his

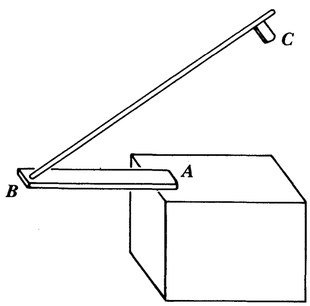


*fig. 98b.*

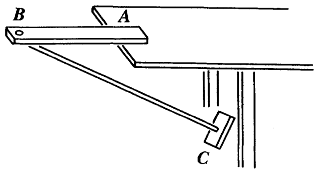


*fig. 98c.*

sketches all his thoughts on a single topic. He then transcribed them and sometimes completed them and put them in a definite order. The *Treatise on Painting* and the *Trattato del moto e misura dell' acqua* in our possession were written in that way. Others, undoubtedly, were written in a similar fashion, such as the *Treatise on Perspective* acquired by Benvenuto.

Melzi, whose intention was to enhance the reputation of Leonardo da Vinci by disseminating his ideas, had copies[232] made from these different treatises which he then put into circulation and which were then reproduced by readers in more or less complete, more or less perfect form. It is by means of such a copy that we know the *Treatise on Painting* and the *Trattato del moto e misura dell' acqua*, since the originals have been lost.

Leonardo certainly assembled in one or more treatises the properties of the center of gravity to which his meditations upon the doctrine of Albert of Saxony had led him. As we shall see in the following chapter, in his *Treatise on Painting* he quotes one of those treatises, which he had entitled a *Treatise on Local Motion.*

These treatises on the properties of the center of gravity were undoubtedly known by many mechanicians of the XVIth century. The analysis which we shall develop in the following two chapters will leave no doubt in this regard.

Was Cardan familiar with these collections? One can scarcely believe otherwise, since many passages from the *De Subtilitate*, besides the one we are presently studying, carry the mark of the influence exerted by the thoughts of Leonardo da Vinci. Moreover, in the same seventeenth book of the *De Subilitate*, Cardan quotes Leonardo twice: the first time[233] concerning his anatomical research and the second time[234] concerning his experiments in aviation. Being familiar with the diverse paths of the extraordinary intellect of the painter, Cardan must have zealously sought for the traces of that activity.[235] Thus, very probably, he possessed a copy of one of those treatises in which Leonardo had written down the properties of the center of gravity. However, even though the copy contained many errors, Cardan scrupulously reproduced even those mistakes.

Let us add one last remark, Leonardo da Vinci surrounded his two paradoxical cases of equilibrium with various confused explanations. Nevertheless, it is easy to see that he is attempting to reduce them to the following principle which he had discovered: A body is in equilibrium when its center of gravity is projected within its base which rests either on the ground or on a horizontal support.

In the *De Subtilitate*, however, it is not this principle, but the following which is invoked: A system is certainly in equilibrium if any virtual displacement raises the center of gravity.

To whom should we attribute this change in the method of demonstration? Is it to Cardan? In this matter, Cardan appears to have played more the role of an unthinking scribe than that of an inventive thinker. It would seem plausible then to conclude that this change in method was due to Leonardo da Vinci himself and that he introduced it when he transcribed two cases of equilibrium which we found in his notes.

This hypothesis does not seem at all implausible, since it would be in accord with several facts, such as the following one for example: Bernardino Baldi, as we shall see later, appears to have deduced his entire mechanics from the treatise of Leonardo da Vinci and he constantly refers to the following principle: the center of gravity of a heavy system cannot ascend by itself.

If the above hypothesis is true, it would make of Leonardo da Vinci the true discoverer of the principle of statics commonly attributed to Torricelli.

The doctrine professed by Cardan in his *Opus Novum* is precisely that which was taught in Paris in the XIVth century. We can find that same doctrine expressed with absolute precision in the work of Guido Ubaldo. After having expounded the definition of the center of gravity as given by Pappus and Frederico Commandino, the Marquis del Monte continues in the following terms:[236]

From this, one can draw the following conclusions. If a heavy body were placed at the center of the Universe, its center of gravity would also be located at the center of the Universe if one asserts that the equilibrium of this heavy body in this position requires that the various parts surrounding this point possess and maintain the same moment. Thus, when we state the proposition that because of its natural propensity any given heavy body strives to place itself at the center of the Universe, we mean nothing more than this: that the heavy body strives to unite its own center of gravity with the center of the Universe, in order to achieve a perfect state of rest. It follows that the downward motion of any given heavy body occurs along the straight line which unites the center of gravity of the heavy body with the center of the Universe. Therefore, the rectilinear fall of heavy bodies demonstrates clearly that the heavy bodies tend downwards in accordance with their center of gravity . . .

Everything we have said so far about the center of gravity leads us to conclude that a heavy body has its weight, so to speak, at its center of gravity. The very name center of gravity clearly implies this truth. The entire force, the entire gravity of the weight, is concentrated at the center of gravity. It seems to converge from all sides at this very point. Indeed, it is because of its gravity that the weight strives by nature to reach the center of the Universe. However, as we have stated, what really tends towards the center of the Universe is the center of gravity. Thus it is at its center of gravity that a body has its weight. Consequently, when a given weight is supported by a given force at its center of gravity, the weight immediately comes to a state of equilibrium and the entire gravity of that weight becomes evident to the senses. That is what happens if a weight is supported at a point in such a fashion that the line connecting this weight to its center of gravity passes through the center of the Universe. In that one case, everything happens as if the weight were supported precisely at its center of gravity. This is no longer true if the weight is supported at an arbitrary point. In that case, the weight does not come to a state of equilibrium. Before its gravity becomes evident it turns, as in the previous case, until the line which joins its point of suspension to its center of gravity extends to the center of the Universe.

... when this line is perpendicular to the horizon, it is just the same as if the weight were sustained precisely at its center of gravity, as we just said. Therefore, since the gravity of a weight only becomes perceptible at its center of gravity, it stands to reason that it is at this point that the body actually has its weight.

This doctrine, which is so clearly formulated by Guido Ubaldo del Monte, is but an updated version of the theory of gravity formulated by Albert of Saxony in the XIVth century. It is based entirely on the following hypothesis: Within every solid heavy body there exists a fixed point, the center of gravity, at which its entire gravity is concentrated. The existence of this point is not a limiting case applicable only when the verticals are considered parallel to each other, but applies even when the convergence of these lines towards the same point, the common center of heavy bodies, is taken into account.

Today, we know that this hypothesis is false. However, until the middle of the XVIIth century, it was considered admissible by geometers. Although neither Archimedes nor Pappus ever stated it explicitly, they had never formally excluded it. We shall see what a crucial role this assumption and the doctrine of gravity associated with it, will play in the evolution of statics. It will lead to important discoveries such as the discovery of the Principle of Torricelli. It will also lead to many errors which will undermine its credibility and force geometers to develop a more precise notion of the center of gravity.

The deleterious consequences deriving from this excessively general notion of the center of gravity are already evident in the works of Guido Ubaldo. They becloud even the grain of truth contained in the objections raised by Ubaldo[237] against the erroneous proposition formulated by Jordanus and Tartaglia on the stability of the balance. Montucla,[238] states quite correctly that *Guido Ubaldo, who refuted them, had himself only avoided a part of those errors because after having shown that the balance would remain inclined if the verticals are parallel, he attempted to extend the same conclusion to the case when they converge. The cause of his error was to have assumed that in the case of convergent verticals the center of gravity remained in the same position whether the balance was horizontal or inclined.*

## 9. THE INFLUENCE OF ALBERT OF SAXONY AND LEONARDO DA VINCI: J.-B. VILLALPAND AND MERSENNE.

Guido Ubaldo had not drawn from the doctrine of Albert of Saxony deductions applicable to statics. On the other hand, the theorems of J.-B. Villalpand are certainly applicable to that branch of mechanics.

Juan Battista Villalpando,[239] who was born in Cordova in 1552, entered the Society of Jesus and had for his teacher Father Jeronimo Prado, who was born in 1547 in Baeza. When Phillip II requested from him a commentary on the vision of Ezechiel, Father Prado involved his student in that project which he planned on a vast scale.[240] Villalpand was initially responsible for only the archeological part of the work. However, when Father Prado died in Rome in 1595 with his commentary unfinished, his student continued the work and wrote the third volume alone.[241] Villalpand died in Rome in 1608 without having finished the monumental exegesis of Ezechiel.

In the course of his research on the archeology of Jerusalem and the Temple, Villalpand concentrates on refuting a strange error. Certain commentators had claimed the following: Judea is such a mountainous country that its surface area is four times greater than a flat country with the same borders. In order to prove the absurdity, or better still, the uselessness of such an assumption, Villalpand undertakes to demonstrate that a mountainous area cannot hold any more people, any more animals, any more buildings and any more trees than a plain with the same boundaries. The demonstration required must be deduced from the conditions of equilibrium of a heavy body at rest on the earth.

The statics of Villalpand was undoubtedly influenced by the works of Guido Ubaldo. His development of the two definitions of the center of gravity given by Pappus and Commandino leaves no doubt in this regard. However, when he reproduces the propositions of the Marquis del Monte, Villalpand obviously strives to detach them from any assumption about a connection between the center of gravity of the individual body and the common center of all heavy bodies. The Jesuit scholar does not discuss any such connection. He declares outright that he will consider the verticals as parallel. Finally, when he reproduces the propositions expounded by Guido Ubaldo, he justifies them not by reference to the tendency of the center of gravity of each individual body to unite with the common center of all heavy bodies, but by means of deductions drawn from the very definition of the center of gravity.

Furthermore, it is not necessary to examine these deductions at length to discover their source. The properties which Villalpand attributes to the *line of direction*, that is, to the vertical passing through the center of gravity, were borrowed for the most part from Leonardo da Vinci.

The following proposition[242] could have been borrowed from Guido Ubaldo.

Every heavy body which descends without impediment, falls in such a manner that its center of gravity never deviates from the vertical.

Villalpand justifies this in the following manner:

Let C be the center of gravity in the heavy body AB (Fig. 99) and let us connect this point to the center of the Universe D by the straight line CD. I claim that when the heavy body AB descends, point C will not deviate from the line CD, which is indeed, the shortest distance. Thus, since the heavy body is not hindered by any obstacle and since point C is surrounded by parts of equal moment, nothing prevents the heavy body from traversing the shortest and avoiding the longer paths.

The demonstration sketched out by Villalpand reminds us of the following note by Leonardo:[243]

Every natural action is accomplished in the shortest way. This is the reason why the free fall of a heavy body occurs towards the center of the earth because this is the shortest distance between the moving body and the center of the Universe.

Villalpand's demonstration resembles even more closely the following passage from the *Treatise on Painting*:[244]

This is proved in the 9th [proposition] in the *Treatise on Local Motion*, where it is stated that every heavy body exerts weight in the line of its motion. Thus, when a discrete body moves towards a given point, any additional mass which is attached to it follows along the shortest line of motion of the whole, without burdening in any way the constituent parts of the body by its own weight.

In this passage, Leonardo alludes to a *Treatise on Local Motion*; he must have composed it in the same fashion as he did the *Treatise on Painting*, the *Treatise on Perspective*, the *Treatise on Water*, which have either come down to us or which we know about from other witnesses. Did Villalpand possess a copy of this treatise? Did he deduce from it his sequence of theorems on the *line of direction*? An analysis of these theorems leaves little doubt since they bear quite clearly the stamp of Leonardo da Vinci.

When we read the following proposition, how is it possible not to recall some of the fragments in Notebook A which we recently quoted:[245]

Every body resting at a point on the ground will remain in equilibrium if the vertical which passes through this point also passes through the center of gravity. The body will move as soon as the line passes outside of the center of gravity . . .

If the *line of direction* HD (Fig. 100) passes through point C, the body will remain immobile, because its parts of equal weight are equidistant from the line in question. Therefore, none of its parts can pull the part on the other side.

This proposition is followed by another[246]

A perfectly spherical heavy body, posed on a perfect plane, will continually move, if unimpeded, until it reaches the single point on the plane where it can remain at rest.

Leonardo, while jotting down on the sheets of Notebook F the thoughts suggested to him while reading Albert of Saxony, wrote[247]

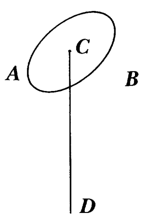


*fig. 99.*

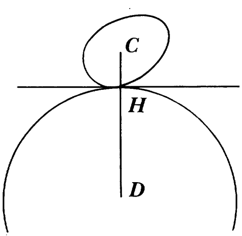


*fig. 100.*

A perfectly spherical heavy body placed at the extremity of a perfect plane will not stop, but will go immediately to the midpoint of the plane.

Villalpand's debt to Leonardo is undeniable here, all the more so because this theorem could not have been suggested to the Jesuit scholar by the problem he was attempting to solve. Nothing in this theorem is relevant to that problem.

The imprint of Leonardo is profoundly evident in the following proposition and in the demonstration which justifies it:[148]

A heavy body resting on the ground and covering a given base area will remain in equilibrium when a vertical drawn through the center of that area passes through the center of gravity [of the body] or, when a vertical drawn from the edge of that base area either passes through the center of gravity or leaves the latter inside that area. But if it leaves the center of gravity outside the base area, the heavy body will certainly fall.

. . . if line FC (Fig. 101) when prolonged leaves the center of gravity of the body (as, for example, point L) outside the area BC on which rests the heavy body, the latter will necessarily fall. Indeed, according to the definition of the center of gravity, the weight CLG is equal the weight CLA. Thus the weight of the volume CGH will exceed the weight of the volume CHA.

The heavier volume will therefore pull down the lighter. The body will thus fall on side G, because the center of gravity is on that side and, therefore, that side has the greater weight.

In meditating on the *Questions* of Albert of Saxony, Leonardo had come across a particular case of this crucial question. He had given it a justification quite similar to the one we have just read. Moreover, although his notes do not contain any formulation of the general proposition, Leonardo must have known it, since he makes continual

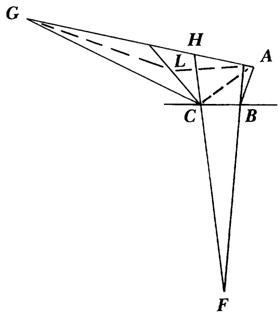


*fig. 101.*

application of it in his *Treatise on Painting*. How can one doubt that Villalpand copied it from the great painter's *Treatise on Local Motion*?

In his manuscripts as well as in his *Treatise on Painting*, Leonardo deduced from this proposition a great number of corollaries concerning the posture of humans and animals. Villalpand is especially interested in their application and borrows these too from Leonardo. Can we doubt this assertion when we read, for example, the following propositions?[249]

When a man is standing on his feet in such a way that the vertical extending from the extremity of the foot on which he is standing passes through the center of gravity, he will be unable to lift his arm on the side towards which he is leaning without falling, because this extended arm plays the role of a lever arm all the greater or heavier, the further it is from the center of the balance.

A man could not lean forward, backward or sideways unless the vertical which extends from the extreme point of the base upon which he is standing, passes through the center of gravity of his body. Or unless this center of gravity falls over this base; otherwise he will fall.

For a seated man to be able to get up, he must move his feet towards the seat and move his head forward.

Let us for a moment examine more closely the following from among the corollaries:[250]

When a bird flies, the vertical passing through the center of the wing's surface also passes through the center of gravity of the body . . . If the bird wishes to elevate the front part of its body and lower the back part, it moves its wings, that is to say, the base supporting it, forward. On the other hand, when the bird wishes to descend, it moves its wings backwards. Thus, in this way, the bird is able to alter the location of the center of gravity in its body.

This last proposition is one of those which especially fascinated Leonardo. Let us recall the way he formulated it.[251]

. . . This applies particularly to the flight of birds which move by themselves without flapping their wings or without the help of the wind. This is possible when their center of gravity is outside of the center of support, that is, outside the surface of their wings, because if the center of the two wings is further forward or backward than the center of gravity of any bird, the bird will move upwards or downwards. The further away its center of gravity from the center of its wings, the further upward or downward it will be. That is to say, the more the center of gravity is displaced from the center of the wings, the more oblique the descent of the bird. If the center of gravity is near the center of the wings, the descent of the bird will barely be oblique.

This proposition, as it appears in the work of Villalpand, bears the stamp of the great painter all the more since it is completely out of place and without any relevance to the investigation being undertaken by the Jesuit scholar. Therefore, we can unhesitatingly attribute to Leonardo the theorems of Villalpand on the center of gravity and the applications to the posture of humans and animals which the latter made of these theorems. In particular, we can attribute the following proposition to him.[252]

A quadruped remains in equilibrium when its center of gravity is located on the vertical extending from one of the extreme points of the surface which passes through its feet, or when it is located on the same side as this surface of the base in relation to this vertical.

This proposition is none other than the classical theorem on the Polygon of Sustentation, taught today in every elementary course on statics. Thus we must return to Leonardo da Vinci to find the discoverer of that law so familiar to every undergraduate. Villalpand merely transmits this to us by appropriating the discovery of the great painter.

The theorems of Villalpand which were almost literally transcribed from a notebook of Leonardo and inserted into a work on archeology, which, in turn, was appended to a book on exegesis, would have undoubtedly remained unknown to geometers, if Father Mersenne had not included them in 1626 in his *Mathematical Synopsis.* Many mechanicians of the XVIIth century must have borrowed them from the latter work in order to develop them in their treatises on statics.

However, when Father Mersenne included these propositions of Villalpand (whom he calls Villapandus) in his *Mechanicorum libri*,[253] he mixed them with many other formulations, some perhaps borrowed from Guido Ubaldo and other authors, others thought up by himself. Some of these formulations implied a more or less obvious adherence to the hypothesis according to which the center of gravity of every body tends to unite with the center common to all heavy bodies. Thus the efforts of Mersenne are oriented towards a completely different goal than those of Villalpand. The assertions of Mersenne are, however, not exempt from vacillation. This work, which is a simple compilation, reflects the divergent opinions of many authors.

Mersenne's hesitation is evident in the very first definition:[254]

Gravity is the property of a heavy body by which it tends and strives to move downwards. It seems to originate in the propensity of each heavy body towards its own self-preservation. However, others prefer to assume that the descent of heavy bodies is due to an attraction pertaining to the earth, which is either magnetic or of another nature.

The center of the Universe[255] is the point towards which all heavy bodies move in a straight line. There exists a center which is common to all heavy bodies; even though it is impossible to prove the existence of this point, it is commonly assumed to exist. It is probable that a unique center of gravity exists in each of the individual systems which make up the Universe, or in other words, in every massive body. Therefore, nothing should be flippantly asserted about the center of the Universe . . .

The influence of Copernicus is evident in this passage; that of Kepler whom we shall discuss later (Chapter XVI, Section 1) might have inspired the last line of the following passage:

We shall assume that all heavy bodies seek the center of the Universe and that they move towards it by nature along a straight line. This principle has almost universal acceptance. However, it has never been demonstrated. Who knows if the parts of a star when torn away from that star, still have gravity and reattach to the star to which they previously belonged? Similarly, who knows if stones elevated toward another heavenly body would return to earth? Would stones which, for example, were closer to the moon than the earth fall back to earth or to the moon?

Mersenne mentions all of the definitions and formulations given by Villalpand on the center of gravity and the line of direction. The properties of this line and the applications that can be made of it to the equilibrium of buildings and to the posture of humans seemed so interesting to Mersenne that he devotes to them some years later one of his *Theological, Physical, Moral, and Mathematical Questions*,[256] published at the same time as the *Preludes to Universal Harmony* and the *Mechanics* of Galileo. However, what he says, is not borrowed from Villalpand, but is at the expense of the works of Bernardino Baldi, which we shall discuss in the following section.

The strange and striking corollaries from which Albert of Saxony had deduced the sphericity of the earth were particularly well-suited to attract the attention of Father Mersenne, whose imagination delighted in paradoxical propositions. From 1625 on Mersenne included some of these corollaries in his work on *The Truth of the Sciences against the Sceptics or Pyrrhoniens*.[257] In this dialogue we observe the Sceptic attempting to embarrass the Philosopher with the following question:

Since you were so kind as to make the offer, I beg of you to tell me how much more distance a man 6 feet tall would move with his head than with his feet, if he were to go around the earth and how much closer together two ropes or two cords, suspended from the top of a tower one league high, would be at the bottom of the tower than at the top . . .

To which the Philosopher answers: "These difficulties are easily resolved" and he gives the solution. Mersenne repeats these propositions in his *Synopsis*, to which he adds yet another which is very much in the spirit of the doctrines of Albert of Saxony:[258]

If God were to remove one of the hemispheres of the earth, namely the one which defines our astronomical horizon, the plane surface remaining would have half the surface of the removed hemisphere. Nonetheless, only one man would be able to live on this surface; all other inhabitants would fall or be flung to the center, assuming, of course, that this section had not changed the center of the Universe. We would be unable to walk on the surface of the earth if it were flat, because our center of gravity would have to ascend by nature.

The thought contained at the end of this passage is expressed more clearly in the following:[259]

The center of gravity of a given body never ascends by nature; it only ascends by violence. Otherwise, half of the weight of the body or even a greater fraction thereof would ascend, which is not possible . . . a part of a body never rises unless a descending part compensates for it . . . The truth of this theorem is evident in the circumvolution of a falling sphere. Some parts of this body ascend, but the center of gravity continually descends.

The proposition stated in this fashion obviously contains the same principle of statics which will later become the Principle of Torricelli. Mersenne recognizes this principle and once it has been formulated immediately mentions two of its applications:

The truth of this theorem is evident . . . when we observe sabres, knives or other such instruments remain suspended when stuck in wooden planks set at an angle. The total weight cannot fall altogether, because it is supported on one side. On the other hand, it cannot fall in any direction because the part which falls would have to cause another part equally heavy or heavier to rise, which cannot be.

We must refer to this same principle in order to explain why a pail filled with water or any other liquid does not fall when suspended from one extremity of a stick with the other extremity supported, assuming that a second stick is wedged between the bottom of the pail and the bottom side of the first stick. Thus, if this pail or any other container were to fall, the center of gravity would have to ascend.

This second case of equilibrium mentioned by Mersenne is already well-known to us. It is the ridiculous case of equilibrium presented by

Cardan. Thus it seems that Mersenne had seen a flawed document quite similar to the one which had misled Cardan.

We can also easily recognize the first case of equilibrium mentioned by Mersenne. It is the first of the two cases of equilibrium contrived by Leonardo da Vinci illustrated in Figure 98b. The only difference is that Leonardo put at *c*, a counterweight of an indeterminate nature, while Mersenne put a knife or a sword at that point.

This last observation seems to confirm our earlier hypothesis that the document used by Cardan and then by Mersenne has its origin in the notes of Leonardo, as did the treatise used by Villalpand and the thoughts reformulated by Bernardino Baldi.

One last question: of the two cases of equilibrium contrived by Leonardo, was only the incorrect version, known to Cardan and Mersenne, in circulation? On the contrary, it seems likely to us that the change which made the second case of equilibrium absurd was the product of a secondhand copy, while other correct copies were still available.

Let us support this supposition with the following reasons: The great treatises on mechanics written by the Jesuits in the second half of the XVIIth century,[260] for example, the treatise of Father Dechales and the treatise of Father Casati, often show the direct influence of Leonardo. It therefore seems that notebooks preserving the thoughts of the great painter were still in existence at that period in the Collège Romain or in the Collège des Jésuites at Lyon.

For example, the treatise of Father Dechales presents[261] a very accurate explanation of the second case of equilibrium contrived by Leonardo. Father Dechales explains it just as Leonardo seemed to have done first of all, by remarking that the center of gravity of the system must go through the part which rests on the support. He also knows the format which Cardan and Mersenne gave to this case of equilibrium, but he correctly remarks that in this form equilibrium cannot be maintained, unless the portion of the stick which rests on the table is very long and very heavy, in which case the equilibrium loses all its paradoxical character.

It seems necessary to us to go back to Leonardo da Vinci to find the discoverer of the principle of statics which Lagrange attributed to Torricelli. In any case, this principle is clearly formulated by Cardan and by Mersenne. It is true that the applications of the principle made by these two authors are peculiar and, occasionally, incorrect; however, Bernardino Baldi and Galileo will make more interesting deductions from it.

Let us end our quotes from Mersenne with a proposition[262] which contains immediately after the statement of a truth, an error copied by Torricelli which we pointed out in section 1.[263] Perhaps we will find there an indication of an influence exerted on Torricelli either by the writings of Mersenne, or by other sources used by the latter:

If a body is suspended at one point on the *line of direction* located above the center of gravity, this point (sic) returns to its original state if displaced from it. If the body is suspended at a point located below the center of gravity, it moves away from its initial position once displaced from that position. But when the body is suspended at the center of gravity, it remains in equilibrium whatever its position . . . That is the reason why balances remain in whatever position they are placed when the point of suspension coincides with the center of gravity. They return to equilibrium when the point of suspension is above the center of gravity. Finally, they describe a complete circle when the point of suspension is below the center of gravity.

## 10. THE INFLUENCE OF LEONARDO DA VINCI ON BERNARDINO BALDI

The work of Villalpand had ceased to be relevant, when the *Exercises on the Mechanical Problems of Aristotle* by Bernardino Baldi appeared in 1621.[264] However, this publication predated the voluminous work of Fathers Prado and Villalpand. Baldi, the Abbot of Guastalla, famous for his prodigious erudition had died in 1617, four years before his work went to press. The publication of this work was carried out by Fabritio Scharloncini, who prefaced the *Exercises* with a short and interesting note about the compositions of the author. We learn in this note that Bernardino Baldi had written his *Exercises* beginning in 1582[265] when he was the friend and confidant of Guido Ubaldo del Monte.

In 1577 Guido Ubaldo had just published his *Mecanicorum liber*, which was to enjoy enormous success for the next hundred years. In 1588 when he was at the height of his greatest intellectual activity as a mechanician, he was preparing to add to it a treatise entitled: *In duos Archimedis aequiponderantium libros paraphrasis.* There can be no doubt that the doctrines on mechanics of the Marquis del Monte inflenced those expounded by Bernardino Baldi. Far from denying this influence, Baldi takes pleasure in quoting his friend's name often.

This tireless scholar's knowledge of mechanics derives from yet other sources than the *Mecanicorum liber* of Guido Ubaldo. Baldi points out some of them for us himself. First, there is the translation of the *Mechanical Problems* of Aristotle, provided with brief commentaries by Nicolas Leoniceni.[266] Furthermore, there is the important and scholarly *Paraphrase of the same Mechanical Problems* of Alexander Piccolomini which was published in 1547. Baldi even goes so far as to tell us in his preface that the fame surrounding the research of the Dutchman Simon Stevin has reached him but that he has not yet seen the work of that author. Indeed, the statics of Simon Stevin, written in Flemish, was not printed until 1586. Thus, four years before the result appeared in print, news of the research of the great geometer from Brugge had already reached Italy.

But there is a profound influence on Baldi which he neglects to mention: the influence of Leonardo da Vinci.[267] In his glosses on Aristotle, Baldi seizes every occasion to expound upon the observations suggested to him by the notes of Leonardo. He borrows from the great painter his explanation of the formation of whirlpools in running water, his theory on the resistance of materials and on the thrust of arches and vaults and, finally, many other essential points from his dynamics. But this is not the place to analyze these borrowings which we have discussed in another publication. In the present work, we will merely show how the statics of Baldi derives from that of da Vinci.

At the very outset of his mechanics, Baldi considers himself a follower of Albert of Saxony, just as his friend Guido Ubaldo del Monte had done. He states that[268]

Everything which is heavy has its weight [concetrated] at a point which is called the center of gravity.

Thus it should not surprise us to find in the *Exercises* of Baldi almost all the theorems which Villalpand had borrowed from Leonardo and which he had curiously enough included in his description of Judea.

Baldi gives some of these theorems in the chapter in which he examines the following question of Aristotle:[269] why is it that when two men carry weight suspended from a beam, the one closest to the load is carrying more weight?

This question leads him to ask why people carrying a heavy load walk bent over? And he responds by saying that they assume this posture in order to put their center of gravity on the vertical through the point of support.

He then begins to develop the notions on the different postures of humans and animals which Leonardo had sketched out in Notebook A before explaining them in a more complete fashion in the *Treatise on Painting*. Baldi continues to consider these notions in the following chapter[270] where he examines the following question of Aristotle: Why do those who are seated and wish to get up position their lower legs in such a fashion that they form an acute angle with their thighs while moving their chests closer to their thighs? This was precisely the first question[271] that Leonardo da Vinci had attempted to resolve by using the notion of the center of gravity.

Baldi explains in detail the solution given by Leonardo. In a similar way, he also explains the different postures of humans and animals and he does not fail to apply the same theory to inanimate objects. The example of the tripod[272] leads him to formulate the law of the Polygon of Sustentation. The equilibrium of leaning towers, such as the tower of Pisa or the tower of Garisendi at Bologna are treated in virtually the same way as in the book of Villalpand.

However, Baldi could not have found the theorems of Leonardo in the book by Villalpand. The work by the Abbot of Guastalla was completed well before the publication of the work by the erudite Jesuit. Nor can it be maintained that Villalpand only had indirect knowledge of the theorems of Leonardo by means of a manuscript copy of the *Exercises* of Baldi. Several passages given by Villalpand, for example, the very typical passage on the flight of birds, cannot be found in the book by Baldi. Baldi and Villalpand must have derived their knowledge from a common source which must have been either a manuscript of Leonardo or a notebook based on the notes of the great painter. Furthermore, it is possible that Villalpand knew this document through Baldi. According to Scharloncini, Baldi was also interested in the description of the Temple of Ezekiel and had composed a treatise on that subject. It would not be surprising if he had come into contact, regarding this matter, with the two Jesuit scholars who were dedicating their lives to a commentary on Ezekiel.

Baldi not only made use of the theorems on statics formulated by Villalpand, but he also formulates a great many other propositions of his own relative to that branch of mechanics. He justified almost all of these propositions by basing them on the following fundamental principle: The center of gravity of a body cannot, without violent action, move away from the center of the Universe.

It seems quite probable that Baldi, undoubtedly inspired by the notes of Leonardo da Vinci, was the first to make known after Cardan, but more formally than he, the generality of this principle of statics. He makes use[273] of it to explain certain cases of equilibrium. He goes even further and calculates as the measure of the work necessary to tip over a body, the product of the weight of a body and the height to which the center of gravity has been raised. Thus one can understand[274] the reason why, given two columns of identical shape but of different weight, the heavier column is the most difficult to topple. One also understands why a circular shape is best suited for motion:[275]

> When a circular wheel rolls across a horizontal plane, the center of gravity at no moment increases its distance from the center of the Universe; that is why this motion is so easy. It is quite different in the case of a wheel which is not of circular shape. Its motion undergoes oscillations, because as it rolls, its center of gravity is not always at the same distance from the center of the Universe.

According to Baldi, this axiom is the true basis of all mechanics. Time and time again his language betrays this view:[276]

> The demonstration of the Philosopher is true, he says, but it is not deduced from principles proper to mechanics, that is to say, from the consideration of the center of gravity.

The most interesting application of this principle by the Abbot of Guastalla is surely the discussion on the stability of the balance. While it is true that the XIIIth century geometer whom we have called the Precursor of Leonardo da Vinci had defined the principal cases of equilibrium and all of the elements of his discussion appear scattered throughout the notes of Leonardo, we find them in an orderly fashion in the *Exercises* of Baldi[277] where they are constantly related to the analysis of the displacement of the center of gravity.

A balance which has its point of suspension above the center of gravity of the beam is in stable equilibrium because any disturbance imposed on this balance causes the center of gravity to rise. If the added weight which causes this disturbance is removed, the center of gravity will return to its initial position.

On the contrary, if the point of suspension is below the center of gravity of the beam, the equilibrium of the balance is unstable, because the slightest disturbance causes the center of gravity to descend.

Finally, if the point of suspension coincides with the center of gravity of the beam, the equilibrium of the balance is indifferent. This results from the very definition of the center of gravity as given by Pappus.

To the study of the stability of the balance, Baldi adds an innovative study on the sensitivity of this instrument and shows considerable pride in his innovation. However, he still bases this study of sensitivity on the consideration of the displacement which the center of gravity undergoes when the balance is displaced from its position of stable equilibrium. What he says about it is not quite correct, but his errors can be easily eliminated. One of them seems to be simply an obvious oversight, due perhaps to a copyist.

Baldi introduces his thoughts on the sensitivity of the balance in the following terms[278]

Let us show what no one else has noted before: that balances with their point of suspension above the center of gravity of the beam are of such a nature that they are not put into motion by just any additional weight or, at least, that they do not undergo a total inclination.

Indeed, let us add a weight to one of the pans of such a balance. If the weight is able to overcome the resistance exerted by the center of gravity which is forced to rise contrary to nature, the balance will move. But if this weight is too small to overcome the resistance exerted by the center of gravity when it is close to its lowest position, the balance will not move, or, at most, it will only move very little.

Baldi adds that the resistance exerted by the balance to any displacement is greater, the closer the center of gravity is to the point of suspension.[279] Moreover, that resistance is more easily overcome by a given weight, the longer the arms of the balance are. The first of the two propositions should be replaced by its opposite. To be convinced of this, it suffices to repeat the very demonstration of Baldi himself but to leave out several inaccuracies which put it in error.

While studying balances with their point of suspension below the center of gravity of the beam, Baldi adds the following:[280]

These balances characteristically incline completely, however minimal the weight added to either one of their pans. We have seen that this does not occur in balances with their point of suspension above [the center of gravity of the beam].

Despite his claim to originality on this point, Baldi merely restates an assertion already made by Leonardo da Vinci. A rough outline of this assertion can be found in Notebook A,[281] next to remarks on the resistance of pointed and flattened arches. These remarks also influenced Baldi. Leonardo formulates them as follows:

Why the balance does not rotate beneath its pole [point of support] the heavier the weight placed at one of its extremities. — If the pole were at the center of volume of the balance and also at the center of the length, and if the center of the whole [weight] were at the center of the volume, the heavier weight would fall beneath the [center pole] of the balance.

Leonardo knows that this idiosyncrasy is peculiar to balances which have their center of gravity at the point of suspension. A few pages later,[282] he proposes a "kind of balance" with a beam in the form of an equilateral triangle pivoting about one of its vertices. The distance between the median extending from this vertex and the vertical allows us to determine the difference between the weights carried by the two other vertices.

Leonardo could, therefore, have inspired Baldi in everything the latter said about the stability and sensitivity of the balance. Leonardo most certainly furnished Baldi with his theory on the inclined plane.

We have already seen to what extent Leonardo was preoccupied with determining the apparent weight of a heavy body placed on an inclined plane. He proposed various ways to solve this problem, some leading to a correct principle, others to an erroneous formulation.

There is one particular demonstration, borrowed from Pappus, to which Leonardo returned many times.[283] Although this demonstration is obviously illogical, it leads nonetheless to a correct result, which had been found as early as the XIIIth century. Moreover, this demonstration has already attracted our attention several times.[284] The Abbot of Guastalla appropriates it in its entirety.[285] He even goes so far as to reproduce the uncertainties and the gropings in the thoughts of Leonardo. Leonardo has left us several formulations (fol. 21, verso) of his strange demonstration. In one of them he assumes that the object, made to roll on an inclined plane, is a solid wheel. In another formulation (fol. 52, recto) he assumes that it is a sphere. Baldi begins his demonstration with the following words: "Whether it is a wheel or a sphere . . ." It is obvious that Baldi was hardly concerned with erasing the stamp of the great creator whose ideas he was plagiarizing.

Like the demonstration of Pappus which undoubtedly inspired him, Leonardo's own demonstration is an attempt to reduce the problem of the inclined plane to the problem of the lever. This simplification will be provided in a correct form by Galileo (Cf. Chapter XI), then by Roberval (Cf. Chapter XIII, Section 2).

However, it is remarkable that the logical demonstration of Galileo and Roberval cannot dispense with drawing exactly the same figure or with making exactly the same calculation as contained in the unacceptable demonstration of Leonardo. Must we thus infer an influence of the latter on the former?

That Galileo knew the solution to the inclined plane given by Leonardo, is quite probable, although we are unable to prove it. At the beginning of his studies, the young geometer from Pisa is the disciple and protégé of Guido Ubaldo del Monte, himself at that time a close friend of Bernardino Baldi. If the latter had a copy of the Notebooks of Leonardo, is it not highly probable that he would have shared it with Guido Ubaldo, who, in turn, would have brought it to the attention of Galileo? Is it not possible that Galileo was able to read the *Exercitationes* of Baldi in manuscript form, long before they were published?

We can be more certain in the case of Roberval. We know of the close friendship between Roberval and Mersenne. The latter quoted Baldi,[286] in 1634 and borrowed from his *Exercitationes*.[287] Furthermore, the Bibliothèque Nationale[288] possesses in manuscript form a *Traité de Méchanique et spécialment de la conduitte et Élévation des eaux,*[289] *par M. de Roberval.* This treatise, to which we shall return in Chapter XVII, contains obvious evidence of the influence exerted on Roberval by Bernardino Baldi.[290] It is thus quite possible that the theory on the inclined plane as conceived by Leonardo da Vinci and as plagiarized by Bernardino Baldi might have suggested to Galileo, on the one hand, and to Roberval, on the other, the method by which they reduced this theory to the law of the equilibrium of the lever.

Whatever the case may be, our analysis of the works of Villalpand and Bernardino Baldi seems to necessitate some inevitable conclusions:

Through their works, a great many ideas expressed by Leonardo da Vinci on statics and dynamics circulated widely among French and Italian geometers at the end of the XVIth and the beginning of the XVIIth Century. In these works, where the ideas of Leonardo are dominant, we find more or less clearly implied a tendency to found statics on the following principle: The center of gravity of a system of heavy bodies can never rise by itself. Bernardino Baldi, in particular, seems to have clearly recognized the essential role and the full significance of this principle. It, therefore, seems likely that Leonardo was the first to consider dealing with statics in this way, a natural corollary to the doctrines of Albert of Saxony.

We shall see how the doctrines of Albert of Saxony as modified by the Copernican revolution were used by Galileo to clearly formulate this method of statics.

## 11. THE INFLUENCE OF ALBERT OF SAXONY AND GALILEO. IN WHAT WAY DID GALILEO CONTRIBUTE TO THE DISCOVERY OF THE PRINCIPLE OF TORRICELLI?

At the end of the Fourth Day of his *Discourses*, Galileo tells us that, *he applied himself to the study of centers of gravity at the insistence of the Illustrious Lord, Marquis Guidi Ubaldo del Monte, a very great mathematician of his time as demonstrated in the many works which he has published.* Thus we should not be surprised to find a great similarity between the ideas of Galileo and those of Guido Ubaldo.[291]

This similarity is evident in the following passages taken from the *Della Scienza Meccanica*:

> Definitions. We call gravity the tendency to move downwards by nature as is found in all solid bodies by reason of the more or less large quantity of mass of which they are made up . . .
>
> By definition, the center of gravity in every heavy body is the point around which are distributed parts of equal *momento*.[292]

If we imagine that such a heavy body is supported by and suspended from this point, the parts on the right are in equilibrium with those on the left and the parts in front with those behind and those above with those below. Provided that it is suspended at the center of gravity, it will remain immobile however one wishes to place or arrange it. It is also this point which tends to unite with the universal center of heavy bodies, that is to say, with the center of the earth when the body can fall freely in any given environment. We shall make the following suppositions on this point:

> Suppositions. Every heavy body . . . descends in such a way that its center of gravity never deviates from the straight line which connects the point where this center was located at the first instant of motion with the universal center of heavy bodies. This

supposition is quite obviously correct. Since, indeed, it is this center and this center only which tends to join with the common center, it is necessary that when it is not impeded, it moves to join this center by the shortest line, which is the only straight line.

Concerning this center, we can make a second supposition: Every heavy body has its weight principally at its center of gravity so that the entire *impeto*, the entire weight, in a word, the entire *momento* of that body is concentrated at that point as at its natural locus.

When Father Mersenne dedicates "to Monsieur de Reffuge, Royal Counsellor to Parliament" his translation of the *Mechanics* of Galileo, which contain the above stated suppositions, he remarks[293] that

the *Mechanics* can teach us to lead a good life by imitating heavy bodies which seek their center in the center of the earth, just as the spirit of man must always seek its center in the divine essence, which is the source of all spirit.

The doctrine of gravity as conceived by Albert of Saxony and then formulated procisely by Guido Ubaldo and Bernardino Baldi found its most precise statement in the works of Galileo. The meditations by the famous prisoner of the Holy Office will only increase the potential of this doctrine.

Crushed by the condemnation of the Church tribunal, by his solitary confinement, by his advanced age, by sickness and blindness, Galileo withdrew with the permission of the Inquisition to his villa at Arcetri, near Florence. There he was given filial care by a young man, sixteen years of age, with a precocious gift for geometry. Vincenzio Viviani was about to begin his lifelong devotion to the old master. Viviani zealously collected the teachings of Galileo.[294] He solicited from Galileo clarifications on the doubts and objections which the study of geometry had brought to the young man's mind.

These conversations between Galileo and Viviani quickly centered on the *Discourses* which had incidently just been published. Conte, in Paris, had received a copy of the final draft, finished in 1636, and brought it to the Elseviers, who then published it. In a letter written from Arcetri and dated March 6, 1638, Galileo dedicated this unexpected edition of his works to the Count of Noailles.

The novelty of the doctrine presented on the Third Day of the *Discourses*, which dealt with local motion, easily captured the attention of the young geometer without, however, completely satisfying his thirst for rigor. The entire theory was based on the following proposition: a heavy body sliding over an inclined plane acquires a velocity which depends solely on the vertical distance it falls and not at all on the inclination of the plane. Galileo postulated this proposition without any demonstration,[295] to the distress of Viviani.

The doubts and the questions raised by Viviani were to induce Galileo to return to the foundations of his work.

Let us quote the young disciple:[296]

When I read the *Discorsi* and came to the treatise on local motion, I was seized by the same doubt which others have felt, not so much on the subject of the truth of the principle upon which rests the entire theory of local motion, but rather on the subject of the necessity of assuming it to be already known. I began to search for more cogent proofs of this assumption and thus inspired Galileo to discover the geometrical-mechanical demonstration for it during the long sleepless nights which to his great distress occurred very frequently. This demonstration depended on a theory which he had established in opposition to a conclusion by Pappus and which he had developed in his early treatise on mechanics, printed by Father Mersenne. He immediately told me about the demonstration and also told his other friends who were in the habit of visiting him. Being physically blind, but mentally clairvoyant, he developed a method to lead him through the intricate paths of investigation which he knew so well, and over which I too was walking, and that method forced me to reformulate this theorem. His blindness made every explanation involving figures and symbols very difficult. Once drafted, we sent several copies to his friends in Italy and France.

On December 3, 1639, Galileo wrote to Father Castelli, Professor of Mathematics in Rome, a letter in which we read the following:[297]

Several months ago the young man who is presently my guest and my disciple raised objections to the principle which I had assumed in my treatise on accelerated motion and which he had carefully studied at that time. His objections forced me to rethink this principle in order to convince him of its truth and admissibility. To his great satisfaction and my own, these reflections, if I am not mistaken, led me to discover the conclusive demonstration. Once I had established this demonstration. I immediately told several persons about it. My disciple drafted it for me, since I am completely blind and might have committed errors in the figures and symbols necessary to my task. This draft is in the form of a dialogue and is presented as a reminiscence of Salviati so that when my *Discourses and Demonstrations* are published again, it will be possible to insert it immediately after the Scholium on the second proposition in the treatise. It will be the most essential theorem I have ever proposed for the founding of the science of motion. I am sending this demonstration by letter to Your Highness rather than to any other person. I am anxious to hear His opinion first before learning that of the friends close to His Highness, with the intention of sending other copies to our friends in Italy and in France after I have received the opinion of His Highness.

The demonstration which Viviani's questions had led Galileo to discover was inserted[298] at the point designated by Galileo when his collected works were published for the first time in Bologna in 1655. All subsequent editions have carefully retained it.

In Chapter XI we already quoted several passages from this demonstration, but we intentionally omitted the following passage to which we now turn our full attention:

It is impossible that a heavy body or an aggregate of heavy bodies can move naturally and move away from the common center toward which all heavy objects tend. Therefore, it is impossible for it to move spontaneously if as a result of its motion, its own center of gravity does not gain proximity to the common center. Consequently, on the horizon, that is to say, on a surface where all parts are at an equal distance from the same center and which has thus absolutely no inclination, the *impeto* or the *momento* of the moving body is zero.

When he returned to the thoughts he had developed long before in his treatise *Della Scienza Meccanica*, Galileo clarified all uncertainties. By the end of the year 1639 he is in full possession of these two essential theorems:

No aggregate of heavy bodies can ever move by itself, unless its motion produces a lowering of its center of gravity. When such an aggregate of heavy bodies descends in free fall and without initial velocity, its center of gravity describes a vertical.

Although Galileo gave these propositions a perfectly clear and precise form, he did not, however, create then *ab initio*. They were already stated in the XIVth Century by Albert of Saxony and implied in the following axiom, so dear to Leonardo da Vinci:

The heaviest part of a falling body guides its motion.

These propositions had also been formulated, although in a somewhat obscure way, in the *Opus novum* of Cardan and then with more vigor and precision in the *De Subtilitate*. They can be found again in the *Paraphrasis* of Guido Ubaldo and they finally assume their definitive form in the *Exercitationes* of Baldi, in the *Synopsis* of Mersenne, and in the works of Galileo.

Montucla[299] tells us that Torricelli *was studying mathematics in Rome under Castelli when he came across the treatises of Galileo on motion. He then wrote a treatise on the same subject and it was sent to Galileo and impressed him so much that he wanted to meet Torricelli and work with him. Torricelli enjoyed Galileo's esteem for only a short time, since Galileo died three months later. Torricelli subsequently enlarged the above-mentioned treatise by adding a section on the motion of fluids* *which he published together with his other works on mathematics in 1644. It is there that we find the first inkling of an ingenious principle of great use to mechanics and which reads as follows*: When two weights are joined together in such a fashion that, when placed in an arbitrary configuration and their common center of gravity neither ascends nor descends, they are in equilibrium in every situation. *It is by means of this principle that Torricelli demonstrates the ratio between weights which counterbalance one another on an inclined plane. Although he only applies this principle to this particular case, it is easy to see that it is applicable to any imaginable case in statics.*

By comparing this account by Montucla with what was previously said we come to the following conclusion: Not only did Torricelli not antedate Galileo in the discovery of the principle of statics which Montucla and Lagrange attribute to him, but it is Galileo himself who taught him that very principle. There can be no doubt about this when one knows that Galileo sent his famous scholium to Father Castelli in December of 1639 and asked him to circulate it, and that Torricelli, at that time, was a student of Father Castelli and between that moment and the time of Galileo's death (January 8th, 1642), Torricelli wrote the treatise in which the principle in question is stated in almost exactly the same terms used by Galileo.

But even if Torricelli can no longer be considered as the first author of this proposition, he is nonetheless the first to have clearly formulated it as a postulate upon which all of statics could be founded, perhaps under the influence of the *De Subtilitate* of Cardan, the *Synopsis* of Mersenne or the *Exercitationes* of Baldi. He is also the first to have shown the use of this principle by applying it to the inclined plane. Thus his contribution was important and one can understand why Galileo gave it his complete approbation.

Indeed, the determination of the [positional] gravity of a body moving on an inclined plane was, for Galileo, the crucial theorem on which his entire theory of accelerated motion was to rest. The deduction, however, which furnished him with this result was taken, more or less explicitly, from the axiom of Aristotle, or from an equivalent axiom, that is to say, from that very dynamics which the new science was about to overthrow or supplant. It was more or less apparent that one was dealing with a vicious circle. By founding the theory of the inclined plane on a postulate which seemed to him to be obvious from experimental evidence, Torricelli broke this cycle.

The more satisfying solution given by Torricelli was known to geometers much earlier than the solution proposed by Galileo but from which it was deduced. The former was actually published in 1644, while the latter was not published until 1655. As far as the manuscript copies are concerned which were supposed to have been made by Viviani and sent to the Italian and French friends of the recluse of Arcetri, we are convinced that very few of them were distributed. One of the most fervent admirers of Galileo and, perhaps, the first Frenchman to have received a copy of the *Dialogue on the Two Great Systems of the World* was Gassendi[300] and he, as late as 1645, was not familiar with the arguments by which Galileo had justified his famous postulate: The velocities acquired by bodies descending from the same elevation on planes of various inclination are equal. After Father Cazrée of the Society of Jesus had attacked this postulate, Gassendi refuted him in a letter which contains the following:[301]

While I was writing this letter, much to my astonishment I received a chance visit by the very noble Senator Pierre Carcavi, who is a man quite abreast of progress in the sciences and particularly devoted to the study of pure mathematics. After he had seen your dissertation in my possession and heard about your argumentation, he advised me that he had been given in this city a copy of a book quite recently published by Evangelista Torricelli and in which the eminent successor of Galileo had demonstrated the postulate. I obtained that work and saw that Torricelli had succeeded by using five propositions and the following premise: Two heavy bodies joined together cannot move unless their common center of gravity descends.

Through the letter of Gassendi, we can see that the treatise *De motu gravium naturaliter descendentium et projectorum*, written by Torricelli was soon known and admired in France. The following is further proof of this and is taken from the *Treatise on the Equilibrium of Liquids* of Pascal.[302] After giving two demonstrations of the fundamental Principle of Hydrostatics, Pascal adds the following:[303]

Here is yet another proof comprehensible only to geometers and which others can forego. I take as a principle that a body can never move by its weight alone without its center of gravity descending . . .

In a short *Treatise of Mechanics*, I have demonstrated by means of this method the reason for all the multiplication of force occurring in all the other devices of mechanics invented up till now. I demonstrate for all of them that the unequal weights which are held in equilibrium by means of the devices, are arranged in such a way by the construction of the device that their common center of gravity can never descend whatever position they may assume. It follows that they must remain at rest, that is to say, in equilibrium.

Although Pascal does not quote Torricelli by name, it is quite possible that he borrowed from him the principle of statics of which he makes a new application here, furthermore that the *Short Treatise on Mechanics* to which he alludes here and which has since been lost like so many of the works of the author of *The Provinciales*, was a development of the initial idea provided by the great Italian geometer. By his own testimony we know, indeed, that Pascal knew very early on of the *Opera geometrica* of Evangelista Torricelli. On August 8, 1651 he wrote to M. de Ribeyre on the subject of an experiment with "quick silver:"[304]

But since we were all anxious [around 1647 or 1648] to know who had discovered it, we wrote to Cavaliere del Posso in Rome, who instructed me, long after my work was published [in 1647], that it was in truth the great Torricelli, Professor of Mathematics to the Duke of Florence. We were delighted to learn that it came from such an illustrious genius whose works in geometry which we have already received surpass all those of Antiquity. No one knowledgeable in these matters will disagree with my evaluation.

Furthermore, Carcavi, who had drawn the attention of Gassendi to the principle of statics as formulated by Torricelli almost immediately after the publication of the book containing it, was one of the faithful friends of Pascal, whom he had chosen as a judge in the jury set up to decide upon the famous geometrical tournament on the Roulette. Carcavi must have kept Pascal informed, just as did Gassendi.

Pascal, however, can be excused for not having quoted Torricelli as the discoverer of this principle. As early as 1626, Mersenne had formulated the principle and applied it to the solution of several problems in statics in his *Synopsis*. Later on, in 1644, the same Mersenne made use of the doctrine of Albert of Saxony to explain the laws of hydrostatics.[305] In two connected vases which he assumes full of water:

the water descends until the center of gravity of the entire mass formed by the earth, the water and the vase unite together at the center of the Universe.

Pascal was therefore right in considering it as part of the common knowledge of geometers. Finally, let us add that Pascal does not quote any author by name[306] in his *Treatise on the Equilibrium of Liquids*.

Thus at the time of the first publication of the treatise which assured Galileo priority in this principle — unless it should be attributed to Leonardo da Vinci — geometers had already been accustomed to attributing its discovery to Torricelli for over ten years.

The history of the principle of Galileo and Torricelli offers us a remarkable example of the continuity of the evolution by which most often scientific ideas evolve. We have been able to follow the development of this principle in the same way a naturalist follows the development of an organism.

CHAPTER XVI

# THE DOCTRINE OF ALBERT OF SAXONY AND THE GEOSTATICIANS

## 1. HOW THE NOTION OF THE CENTER OF GRAVITY WAS REFINED. THE INFLUENCE OF KEPLER

A system is in equilibrium if any change in its configuration causes its center of gravity to rise.[1] This principle is clearly stated in the letter addressed by Galileo to Father Castelli on December 3, 1609. It is formulated no less clearly in the text on the fall of heavy bodies published soon thereafter by Torricelli. However, when we compare the forms given this same principle in the work of Galileo with that of Torricelli, we note an essential difference between them.

Not only does Galileo not neglect, in principle, the convergence of the verticals towards the center of the earth, but the consideration of the point of convergence of the verticals is an essential element in his deductions. These deductions show the clear influence of the doctrine professed by Albert of Saxony and many other Scholastics and which had been only slightly modified by Copernicus, to wit, a heavy body which is a part of the entire earth is of the same nature as the earth. The center of gravity of that body tends to unite with its counterpart which is the center of gravity of the entire earth. This sympathy between equals assures the integrity of the globe.

The language of Galileo is always in conformity with this doctrine. After having defined the center of gravity, he adds the following:

> This is also the point which tends to join the center of the Universe, that is to say of the earth, when the body can fall freely in any environment.

He assumes that

> it is this center of gravity and this center alone which tends to join with the common center.

Towards the end of his life when he is giving his principle its definitive form, he still speaks of

> the common center toward which all heavy bodies tend.

He assumes that an aggregate of heavy bodies

> cannot move spontaneously if, as a result of its motion, its own center of gravity does not gain proximity to the common center.

Our modern historical prejudices are seriously shaken when we see Galileo base the "most essential theorem," which will destroy Peripatetic dynamics, on the Scholastic theory of Albert of Saxony.

Torricelli's reasoning differs profoundly from that of Galileo. Not only does Torricelli no longer attempt to justify his principle by reference to a tendency allegedly belonging to the center of gravity of an aggregate of heavy bodies to move to the common center, but he resolutely projects this latter point to infinity and considers the verticals as being parallel to each other. The ideas which he propounds on this subject have great clarity.

> The following, he says,[2] is an objection very commonly found among serious authors: Archimedes hypothesized incorrectly when he considers as parallel the ropes supporting the two weights suspended from a balance. In reality, the directions of these two ropes converge at the center of the earth.

In order to resolve this objection, Torricelli carefully distinguishes between the actual machines constructed from real, heavy material with which one conducts experiments, and the abstract machines about which geometers reason. It is only in the latter that one can imagine heavy surfaces without thickness or ropes without weight, or verticals as being parallel lines.

> The mechanical foundation adopted by Archimedes,[3] namely, the parallelism between the ropes of the balance can be considered incorrect when the bodies suspended from the balance are real, physical bodies which tend towards the center of the earth. It is no longer incorrect when those bodies (whether they are abstract or real) tend neither towards the center of the earth nor towards any other point close to the balance, but towards some point infinitely removed.
>
> However, for the sake of brevity and facility, we shall not deviate from ordinary language. We shall continue to call the point [infinitely removed] toward which the bodies suspended from a balance tend, the center of the earth . . .

Torricelli resolutely narrows his field of deductions. He reduces it to the abstract notion of mechanics which treats gravity as having the same intensity and the same direction at all points. In this way, he transforms a principle flawed by error as stated by Galileo into a perfectly correct principle. What influences might have induced him to attempt such a transformation?

Among those influences that of Kepler must be mentioned first. The view of gravity as a desire of one mathematical point — the center of gravity of the body — to unite with another mathematical point — the center of the Universe or center of the earth — finds in Kepler a convinced opponent. The mutual attraction of two mathematical points appears to him a pure fiction. Only two bodies can attract or repel each other:[4]

The action of fire, he states, consists not in seeking the outer surface of the World but in fleeing from its center; not the center of the Universe but the center of the earth; and that center, not as a point, but to the degree that it is at the center of a body which is very much opposed to the nature of fire which itself tends to disperse. I go even further by saying that the flame does not flee, but that it is pushed by the heavier air as an inflated bladder would be by water. . . . If the immobile earth were placed at a given point and if another, larger planet moved towards it, the first would acquire gravity with respect to the second and would be attracted to it like a stone is attracted to the earth. Gravity is not an action, but is a passivity in the stone which is being pulled.

A mathematical point,[5] whether it is the center of the Universe or any other point, could not effectively move heavy bodies nor could it be the object toward which they tend. Let the physicists prove that such a force can belong to a point which is not a body and which is only conceived of in a completely relative fashion!

It is impossible that the substantial form of a stone which puts this stone in motion could seek a mathematical point as, for example, the center of the Universe, without regard for the body in which that point is located. Let the physicist then prove that natural bodies have a sympathy for that which does not exist!

Here is the true doctrine of gravity: Gravity is a corporeal, mutual affection between related bodies which tends to unite and connect them. Magnetic force is a property of the same order. It is the earth which attracts the stone, rather than the stone tending towards the earth. Even if we were to place the center of the earth at the center of the Universe, heavy bodies would not move towards this center of the Universe, but towards the center of the spherical body to which they are related, that is to say towards the center of the earth.

It does not matter, therefore, where the earth might be located. Heavy bodies will always be drawn toward it, due to the force which animates it. If the earth were not round, heavy bodies would not be drawn from all directions directly to the center of the earth. However, depending on where they come from they would be drawn to different points. If two stones were placed at a given point in the Universe close to each other and outside the sphere of force of any body related to them those stones, like two magnets, would unite at an intermediate point and the paths that they would travel to unite would be in inverse ratio to their masses.

One can easily guess the role that such claims must have played in the slow evolution which resulted in the doctrine of universal attraction.

It is not our goal here to retrace the evolution.[6] It is sufficient for our purpose to have contrasted the ideas of Kepler, who sees in gravity a mutual attraction between a heavy body and each of the parts of the terrestial globe, with the view of Albert of Saxony, Cardan, Guido Ubaldo and Galileo, who held that the center of gravity of a body aspires to coincide with the common center of all heavy bodies.

## 2. HOW THE NOTION OF THE CENTER OF GRAVITY WAS REFINED (CONTINUED). THE GEOSTATICIANS

The ideas of Kepler contributed perhaps less in refuting this opinion than the serious errors it caused many geometers, including the most well-known.

Around the year 1635, Jean Beaugrand appeared everywhere claiming that he had discovered the law according to which the weight of a body varies according to its distance from the center of the earth. Mersenne hastened to insert[7] in his *Harmonie universelle*, the statement of the law for which Beaugrand was promising a demonstration. According to this law:

> A heavy body, for example, a one pound lead ball, becomes lighter the closer it comes to the center of the earth and it has no more weight when it reaches that center, as Beaugrand concludes in his *Géostatique*[8] where he asserts that the gravity of every body diminishes by the same ratio as it approaches the center of the earth, and that even the entire earth has no weight at all.

Mersenne added:[9]

> I hope that the person who first advanced this proposition will satisfy us on this subject by removing all the difficulties involved just as he promised in his *Géostatique*.

Mersenne was not the only geometer desiring to see the demonstration Beaugrand had promised. Fermat impatiently awaited the publication of the *Géostatique*. On April 26, 1636, he wrote to the scholarly clergyman:[10]

> I would be much obliged to you if you could tell me if Beaugrand is in Paris. He is a man for whom I have great respect. He has a wonderfully inventive mind and I believe that his *Géostatique* will be something quite special.

The *Géostatique* promised for so long and so ardently awaited by the best geometers of the time, was finally published.[11] The disappointment must have been great since the arguments of Beaugrand were worth absolutely nothing.

Descartes[12] had no difficulty at all in discerning the essential flaw which negated the entire work. The arguments of Archimedes concerning the equilibrium of the lever are only true

> when one assumes that heavy bodies tend to move downwards along parallel lines without tending towards one and the same point.

Jean Beaugrand had not grasped this notion at all. After this initial error, other fallacies followed and culminated in the famous proposition so pompously promised by the author. With the usual severity so characteristic of the judgements of Descartes but with an accuracy quite often lacking, Descartes appraised the *Geostatics* in the following terms:

> Even though I have seen the circle squared many times, perpetual motion often proven and many other such demonstrations, all equally false, nonetheless, I can say in all honesty that I have never seen so many errors in one single proposition. . . . Therefore, I conclude that everything contained in this book on geostatics is so irrelevant, so ridiculous and so despicable that I am astonished that any honest man would deign to take the trouble to read it. I myself would be ashamed to have expressed my opinion on it here, if I had not done so at your request.

Such an opinion was hardly suited to assure Descartes of the friendship of Jean Beaugrand, who, in all probability, engaged in numerous maledictions against the Philosopher.[13] We know that Descartes told Mersenne[14] on July 27th 1638 not to worry about what "the geostatician" was writing against him.

Furthermore, the *Geostatics* does not seem to have been any more favorably received by the friends of Jean Beaugrand than by Descartes. This is clear from the tone of the letter written by Fermat to Mersenne on Tuesday, June 3rd 1636:[15]

> I have seen the *Geostatics* of M. de Beaugrand and I was astonished to find my views differ from his. I assume that you have already noticed this. I am sending him my honest opinion of his book and I can assure you that I respect so highly his intelligence which he has demonstrated to me so often that I have difficulty in convincing myself that I am not in the wrong by having an opinion contrary to his. However, I concede that he is my judge and I no longer challenge you.

In this letter, Fermat contrasts his own view with that of Jean

Beaugrand, because he, too, had advanced a proposition on geostatics which he had appended to a letter to Carcavi in May 1636. Today, the letter is lost, but the proposition is known.[16]

The proposition on geostatics of Fermat will be the starting point in a long and important debate in the course of which we shall see the Counselor to the Parlement of Toulouse face the greatest geometers of his time: Étienne Pascal, Roberval and finally, Descartes. We shall hear Fermat formulate theorems which seem strange to our way of thinking, accustomed as we are to modern mechanics. We shall see him develop deductions which appear to us absurd. However, let us take care not to consider this debate idle, or to think that its only result was to prove to Fermat the contradictions which are immediately so obvious to us but which run counter to his own opinions on statics. This dispute is important for an entirely different reason. But it is true that its real significance escapes us if we do not momentarily rid ourselves of the mechanical knowledge which centuries of effort have made so easy and obvious to our 20th century minds. Its significance will become clear to us if we imagine the state of mind of a geometer living in the time of Louis the XIII.

Two quite distinct doctrines vie with one another to account for the equilibrium and motion of a heavy body. One of these doctrines takes as a principle the fundamental axiom of Peripatetic dynamics. Several mechanicians, for example, Galileo, still adhere to this axiom, but the majority of geometers have more or less abandoned it. They deduce their theorems on statics from the equality between the work of the impressed forces and the work of the resisting forces formulated first by Jordanus — or from other principles related to the latter such as the principle of the impossibility of perpetual motion. In the works of Stevin and Roberval, this doctrine came to constitute a complete statics of which Descartes was soon to give an illustration, admirable for its clarity and simplicity.

The other doctrine was formulated by Albert of Saxony and it was adopted by all Scholastics. It derived from the following principle: In every heavy body there exists a point — the center of gravity — which tends to join the common center of all heavy bodies. Bernardino Baldi and Guido Ubaldo stated this doctrine with great precision while Cardan, Mersenne and Galileo deduced from it the following rule of statics: A system remains in equilibrium even should a disturbance move its center of gravity away from the common center of heavy bodies.[17]

There is, however, a contradiction between the two doctrines, between the doctrine deriving from Jordanus and the other propounded by Albert of Saxony, and the two doctrines cannot be reconciled. The useful corollaries furnished by the latter are unacceptable to those who take as a principle the equality between the work of the impressed forces and the work of the resisting forces, until a suitable correction has removed from those corollaries the trace of the unacceptable postulate which produced them.

This contradiction becomes evident, when Fermat, a fervent disciple of the theories of Albert of Saxony, pushes this theory to its extreme and unacceptable consequences. The debate which we are recounting will finally rid statics of the contradiction in it and assure the logical unity of that science.

From the very beginning of his *Propositio geostatica* it is evident that we should consider Fermat a fervent disciple of Albert of Saxony. Fermat takes as a principle "the proposition which," he says, "can easily be proven by following Archimedes and which could be immediately demonstrated should it ever be denied:

> Let B (Fig. 102) be the center of the earth, BC a terrestial radius, BA a part of the opposite radius. If the weight placed at C is to the weight at A as BA is to BC, then weights A and C will not move, they will be in equilibrium.

What a strange transposition of the laws established by Archimedes. Fermat applies the law of the lever to the case in which the two acting forces, both directed along the lever, are opposed to each other. Nevertheless, it is very clear that in order to be in equilibrium two such forces must be equal and not in the ratio of AB to BC. This is, however, the way we would express it today based on knowledge which has become so familiar to us that is seems self-evident. Let us be

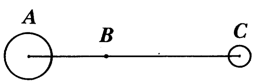


*fig. 102.*

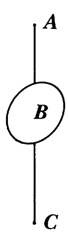


*fig. 103.*

careful, however, not to consider Fermat devoid of all intelligence and incapable of recognizing this. The proposition which he formulates and which surprises us so much, is the essential proposition of a theory supported by a great many serious thinkers, ranging from Albert of Saxony to Galileo.

It was, after all, Albert of Saxony who wrote:[18]

If the entire mass of the earth were held by violence outside its natural locus, as for example, within the concavity of the lunar orbit and if then a heavy body were dropped, this heavy body would not move towards the total mass of the earth, but would move in a straight line towards the center of the Universe. The reason for this is that it would only find its natural locus at the center of the Universe, assuming at least, that its center of gravity were at the center of the Universe.

It was also the same Albert of Saxony who attributed to the entire earth what Aristotle, Simplicius and St. Thomas had previously affirmed for any falling body and who wrote the following:[19]

The earth has its center of gravity at the center of the Universe. Indeed, all the parts of earth tend towards the center because of their gravity precisely as Aristotle had said. Furthermore, the truth of this proposition is beyond doubt. Consequently, the heavier part of the earth would push the other part until the center of gravity of the entire earth were at the center of the Universe. Thus, if these two parts had the same gravity they would remain immobile even if they did not have the same magnitude just as two weights on a balance.

And finally was it not Marsilius of Inghen who explained the theory of Albert of Saxony in the following terms:[20]

If a nail were in equilibrium at the center of the earth, only a small segment of the length of the nail would protrude beyond the center and that would be on the side with the nail's head because the head is much heavier than the rest of the nail.

Although the postulate stated by Fermat is totally inadmissible to us today, it is nothing but the conclusion drawn by Marsilius of Inghen but reformulated in a precise mathematical form.

It is by means of this principle, which is so obviously unacceptable to us, that the great geometer of Toulouse justifies the following proposition:

Let C be the center of the earth (Fig. 103), CA be a terrestial radius and B a weight placed between C and A. In order to support the weight placed at B, it would be necessary to apply to it directly a given force F. Let us assume that instead of applying this force directly at point B, we apply it along the segment AB, and further assume that the force is pulling on A. It will have to have a magnitude F′ which will be to F as BC is to AC.

This conclusion is obviously as inadmissible as the principle. The two propositions both clearly show the complete ignorance of the laws of true mechanics typical of some of the greatest geometers of the XVIIth century.

After giving the argumentation of Fermat, Father Mersenne states the following in his *Universal Harmony*:[21]

I fail to see the cogency of this demonstration.

And Descartes wrote to Father Mersenne[22]

Furthermore, I must tell you that my friend from Limoges finally arrived eight to ten days ago, and he brought for me the *Geostatics* with a letter from you in which you included the argumentation of Fermat which seeks to prove the same thing as the geostatician. But, either you omitted something in your description or the matter is over my head. At any rate, the whole thing seems incomprehensible to me, except that he seems to me to make the same mistake as the geostaticians by considering the center of the earth as if it were the center of a balance, which is a grave misunderstanding.

Fermat must have been aware of the objections raised by certain geometers against his proposition or against the obscurities which they encountered in his work. In order to remove the former and resolve the latter, Fermat wrote a text in Latin[23] which he included in a letter addressed to Mersenne on June 24, 1636.[24]

The great geometer of Toulouse complains at the outset, that his opinion has been confused with that of Beaugrand, according to whom the weight of a heavy body depends on its distance from the center of the earth.

I am of the opinion that every heavy body wherever it may be located in the Universe, outside of the center, taken absolutely in itself always has the same weight. This is a proposition which I could easily have taken as a principle, if I had not seen it questioned. Therefore, I will attempt to prove it. However, whether it is true or not, does not affect the truth of my own proposition which never considers a heavy body in itself, but always in relation to the lever. Thus I put nothing in the conclusion which cannot be found in the premises.

The distinction made here by Fermat is difficult for us today to grasp. To understand it, we must remember that Fermat is imbued with the opinions favored in the School since Albert of Saxony. He considers the total weight of a body to be invariable. However, of this constant weight one more or less large part can pass over into a state of actuality and exert an effort on the lever, while the rest remains in a potential state.

Fermat then tells us that he has long suspected that Archimedes did not exercise the necessary precision in his study of mechanics.[25] It is clear, indeed, that he considered the direction of descent of falling bodies as parallel, because without this assumption, his demonstrations have no validity. It is not that this assumption is so far from the truth. The great distance to the center of the earth allows one to consider the lines of descent of heavy bodies as parallel. However, this approximation does not satisfy those who seek both detailed and profound truths.

In order to discover such truth, one must make use of other principles than those of Archimedes. Fermat once again mentions some which he considers as entirely acceptable. Thus he asserts the following postulate which derives directly from the doctrine of Albert of Saxony:

If two equal heavy bodies joined by a straight and weightless rod were not impeded by any obstacle, they would not come to rest as long as the midpoint of the rod were not at the center of the Universe.

He also asserts another postulate and we will reproduce its formulation exactly.[26] No more convincing proof could be given of the ignorance of M. de Fermat, Counselor to the Parlement of Toulouse and outstanding geometer, on the subject of the most ancient and best-known laws of mechanics.

Let DBC be a lever (Fig. 104) which does not pass through the center of the earth; the fulcrum of this lever is at B. Its arms are BD and BC. The center of the earth is at A.

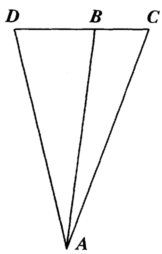


*fig. 104.*

Let one draw lines DA, BA and CA. Let one suspend heavy bodies from points D and C and let the ratio between C and D be the product of the ratio of line DA and line CA and the inverse ratio of angles CAB and BAD. I claim that the lever BDC, suspended at point B, will remain in equilibrium.

We can state that this proposition is very true. We shall demonstrate it when necessary by demonstrations taken from the purest of geometries and from Physics.

This proposition as formulated by Fermat is completely incorrect. To correct it, the ratio between angles CAB and BAD must be replaced by the ratio of their sines. For all practical purposes, these angles are so small that the error produced is negligible. Thus it is obvious that the erroneous postulate provided Fermat with results which are qualitatively correct.

Among these is the following proposition: A balance with equal arms bearing equal weights is in unstable[27] equilibrium when it is parallel to the horizon.

The error in Archimedes,[28] if we can call it that, derives from the fact that he took as his point of departure that the arms of the balance would stop, even though they were not parallel to the horizon, and I have demonstrated the contrary.

However, if the descent of the heavy bodies occurred along parallel lines, . . . in that case, the proposition of Archimedes would hold true. It is not in practice that this is far from the truth, but it is a joy to search for the most minute and subtle truths and to eliminate all possible ambiguities. This is precisely what I have done and I can assure you that although the investigation was quite difficult, I am in total control of all the demonstrations.

The deductions of Archimedes were entirely devoid of the error which Fermat claimed to eliminate from them. Guido Ubaldo alone had committed that error. And in his Latin text, Fermat is more accurate in writing the following:[29]

We shall demonstrate and refute the error of Ubaldo and other geometers who assume that the arms of the balance can remain in equilibrium, even when they are not parallel to the horizon.

It is appropriate to quote the following corollary[30] from those which Fermat deduced from this erroneous principle, because it is of crucial importance to our study:

It is evident from the preceding that all of the definitions of the center of gravity provided by the Ancients are invalid. With the exception of the sphere, there is no other body in which one can find a point such that the heavy body suspended by that point and outside the center of the earth would remain in an indifferent equilibrium.

However, instead of recognizing from this that the notion of center of gravity loses all its meaning, once the verticals are no longer considered as parallel, Fermat tries to salvage this notion at all costs and proposes[30] a new definition, a bizarre derivation from the doctrines of Albert of Saxony, Benedetti, Bernardino Baldi, Guido Ubaldo and Galileo:

Henceforth, we shall define the center of gravity in the following manner:
A point placed inside a body so that the body would remain in indifferent equilibrium if the point were united with the center of the earth. Only in this case, can one talk about centers of gravity.

Mersenne was quick to pass on the demonstration by Fermat to the many geometers whom he was corresponding with. It did not satisfy anyone and Fermat was soon aware of this:[32]

You must not doubt that my demonstration comes to a perfectly accurate conclusion, he wrote[33] to Mersenne on July 15, 1636, even though it seems that M. de Roberval did not find it precise.

Evidently, Roberval was quick to make his objections to the principles of Fermat known because during the month of August 1636, the latter wrote the following to the Professor of the Collège of France:[34]

The first objection consists in the fact that you are unwilling to admit that the midpoint of a line joining two equal weights will eventually unite with the center of the Universe in free fall. By doing so, you seem to be in violation of both empirical evidence and basic principles. The truth of my principle depends on the two weights or forces having a natural inclination towards the center of the earth and following that inclination. . . . Furthermore, no one has ever doubted that the center of a heavy body will unite with the center of the earth if it is not impeded. . . . The second objection was raised against the new ratio of the angles which I discovered but against which you had nothing more specific to say than to demonstrate that the reciprocal ratio between the weights must be explained by the sine of the angles and not by the angles themselves. Here is the demonstration of my proposition . . .

On Saturday, August 16, 1636, Étienne Pascal and Roberval wrote a long letter to Fermat.[35] In this letter, a model of courteous and thorough scientific discussion, the postulates on which the great geometer of Toulouse had founded his mechanics are submitted to an exact and rigorous examination. The thrust of Roberval and Étienne Pascal was above all to put into question the principle which had been proposed by Albert of Saxony, formulated by Bernardino Baldi and Guido

Ubaldo, recognized by Galileo and accepted by Fermat as a basic truth, as a "first principle" about which "no one ever doubted."

Sir, Étienne Pascal and Roberval wrote, the principle you demand for geostatics states that if two equal weights are joined by a straight and weightless line and can fall freely, given this configuration, they will never come to rest until the midpoint of the line (which is the center of gravity for the Ancients) joins the common center of all heavy bodies.

This principle which we considered some time ago, just as you were asked to do, appears at first sight quite plausible. But when it is a question of a principle, you know what conditions must be met in order for it to be accepted. Of such conditions a major one is lacking in the principle under consideration, to wit, we know neither the basic cause for heavy bodies to descend nor the origin of this weightiness. Thus we have no certain knowledge of what would happen either at the center to which heavy bodies tend or at the other places beyond the surface of the earth. Since we live on it, we have some direct experience of the latter upon which to base our principles.

It could be that gravity is a quality residing in the falling body itself. But perhaps it is in another body which attracts the falling body like the earth. It is also possible and it is quite probable that it is a mutual attraction or a natural desire which bodies have to unite, as is evident from iron and a magnet, which are of such a nature that if the magnet is at rest, the iron will seek it out if not impeded and vice versa. If both are unimpeded in their motion, they will move towards each other in such a way that the more powerful of the two will move the shortest distance.

When reading these lines written by Étienne Pascal and Roberval, one cannot help but recognize the influence exerted by Kepler on these two geometers. This should not come as a surprise to us since an analysis of the famous treatise *Aristarchi Samii de mundi systemate*,[36] written by Roberval, indicates the extent to which the Professor of the Collège de France had meditated upon the thoughts of the great astronomer, just as Descartes had done.

However, Étienne Pascal and Roberval continue, of these three possible causes of gravity, the results are quite different which we shall show by examining them here successively.

First of all, if the first cause is true, in accordance with general opinion, we fail to see how your principle could stand, because common sense tells us on this subject that a body always has the same weight regardless of its location, since it always has the same quality which causes it to have weight. Furthermore, common sense tells us that a body will rest at the common center of heavy bodies when the parts of the body which are on either side of the same center are of equal weight and counterbalance each other without taking into account their distance from the center.

And it serves no purpose to propose a center of gravity for the body AB, which center, according to the Ancients, is at the midpoint C. This center has only been shown to exist when the descent of the weights occurs along parallel lines, which is not the case. And even if such a point existed — which cannot be the case for bodies which tend toward the same common center — it has not yet been demonstrated and would in no way prove that it is the particular point at which the body would unite at the common center. Even if it were so, it still runs counter to our common knowledge in several points for the reasons stated previously.

In any case, we fail to see why the common center for the Ancients should be considered to exist anywhere but in the bodies which are suspended or supported outside of the place towards which they tend to move.

. . . If either the second or third possible cause of the gravity of bodies is true, it seems to us that one can draw conclusions from them.

Étienne Pascal and Roberval, in fact, attempt to determine how the weight of a body varies with the distance of this body from the center of the earth when the weight of a falling body is considered as the resultant of attractions exerted by the various parts of the globe. Their analysis is oversimplified because they do not seem to take into account the influence which the distance between two objects undoubtedly exerts on the magnitude of their mutual attraction. It constitutes, nevertheless, a curious attempt at continuing and developing the ideas of Kepler. Moreover, it leads the two authors to perspicacious reflections concerning developments in geostatics, such as:

Of these three possible causes for gravity, we do not know which one is correct. We cannot even be sure if any one of the three is correct. There could be another possible cause from which completely different conclusions would be drawn. Therefore, it seems to us that in this matter we cannot admit principles other than those of which we are certain through continual experiments guided by sound judgement.

As far as we are concerned, we consider as equally or unequally heavy bodies those which have an equal or unequal force to move towards the common center. And the same body is said to have the same weight, if it always has the same force. If this force increases or decreases, even though it is in the same body, we can no longer consider it as having the same weight. What we would very much like to know is whether this occurs in bodies approaching or departing from the center. However, since we have found nothing which satisfies us on this subject, we leave this question open and continue to base our reasoning solely on what we, together with the Ancients, have been able to discover up to now as being true.

Étienne Pascal and Roberval possess accurate knowledge of the laws concerning the composition of forces. Therefore, they can easily uncover the serious errors committed by Fermat in his deduction.

They say to Fermat that:[37]

After taking the arc of a circle EBD (Fig. 105) with its center A coinciding with the center of the earth, you assume that the weight placed in its entirety at point B, will

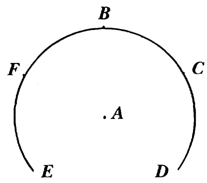


*fig. 105.*

exert the same weight on support B as if it were distributed in parts to points E, F, B, C and D. That is far from the truth since often instead of exerting weight on support B in the direction of A, on the contrary, it will exert weight on the same support in order to move away from A.

This will happen, for example, if the arc ED is greater than half a circumference and if the load rests entirely at the two points E and D.

And yet, being entirely concentrated at point B, it will always weigh with full force on support B so as to move the lever towards A, and in general, when it is distributed, it will always exert less weight on the support than when concentrated at point B. All these things, even though contrary to your assumption, are demonstrated by following our principle.

These same principles led Mersenne to recognize that the total weight of a body of finite extension should decrease progressively to the extent that the body moves away from the center of the earth. And this is true even though, contrary to the view of Beaugrand, the weight of each part of the body remains invariable.

Those individuals, says the erudite priest,[38] who argue for a particular center of gravity in each part of a given body and who ascribe a particular propensity to each point in that body to descend to the center of all heavy bodies (which is assumed to be the same as that of the earth) prove, in another way which seems to me better, that bodies become lighter or exert less weight when approaching the center, but not in proportion to their distance from it. . . . But because the other, different weight derives from the different angles formed by each point of the given body (by reason of the straight line through which it wishes to descend to the center of the earth) with the line passing through the center of gravity of the body, or one which is parallel to it, it follows that if the body is considered as a point, that is to say a point having weight, it will always have the same gravity, no matter how near or how far it is from the center of the earth. This does not occur in the other view,[39] according to which this point becomes lighter in proportion to its distance from the center, as is the case with a heavy body.

The remark by Mersenne in this passage seems to express the view held commonly in the Schools at that time. However, we have already encountered it[40] in a very precise form in the *Tractatus de ponderibus* by Master Blasius of Parma. And even Albert of Saxony, whose influence on Blasius of Parm is undeniable,[41] stressed the principle:[42]

It is true that distance causes the different parts of a heavy body to tend to their natural locus by different paths. However, it will never restrain the tendency of a body towards its natural locus.

As we have seen, in writing these lines, Albert of Saxony had in mind an argument of Roger Bacon. This gives us another opportunity to confirm the persistence among the mechanicians of the XVIIth century, of traditions which owed their origins to the School of Jordanus and to the commentaries of Albert of Saxony and his followers.

It was with great surprise that Fermat received the criticisms by which Étienne Pascal and Roberval claimed to destroy the principle of Albert of Saxony. This painful surprise is apparent in the letter he addressed to Mersenne on Tuesday, September 2, 1636:[43]

As far as the *Geostatical Proposition* is concerned, he says, it is founded entirely on the sole principle that two equally heavy bodies, joined by a continuous line and left unimpeded, will join the center of the earth at the point which divides equally the line connecting them, that is to say, that this point of division will unite with the center of the earth. After Messrs. Pascal and Roberval had recognized that my reasoning was based on this and that allowing this principle, my proposition presented no difficulty, they denied this principle of mine which I took as the clearest and the most evident axiom possible. Please let me know if you share their opinion. In any case, I have recently demonstrated it again by new principles deduced from indisputable experiments which I shall send to you as soon as possible.

Since it was undoubtedly limited to the ideas which had been developed in the Schools in accordance with the archaic teachings of Albert of Saxony, Fermat's knowledge of mechanics contained enormous lacunae. The geometer of Toulouse obviously did not know how the equilibrium of a lever loaded by forces of different inclination, depended on the moments of these forces with respect to the point of support. He also had doubts about the argumentation of Roberval, where this rule was put to use:[44]

I would be much obliged to you, he writes to the Professor of the Collège of France, if you would send me the demonstration of your proposition in which you pursue the view that heavy bodies remain in a reciprocal ratio to the perpendiculars drawn from the center of the lever to the two pendants. I shall continue to doubt this opinion until I have seen your demonstration. However, I can assure you that I shall continue to stand by my view.

Giving in to the request of Fermat, Roberval wrote to him on October 11, 1636:[45]

I am sending you the demonstration of the fundamental proposition of our mechanics as I promised you.

By giving in detail the definition of the terms and axioms he used, he explains with great care the laws of the equilibrium of both straight and bent levers required to explain forces of different inclination. The sequence of his presentation resembles precisely the reasonings of Giovanbattista Benedetti. Furthermore, it can hardly be doubted that Roberval knew the *Diversarum speculationum* of that author. One year later, in fact, Mersenne explains in the *Seconde partie de l'Harmonie universelle*[46] how the convergence of verticals modifies the law of the equilibrium of the balance. The rule which he points out is that of Fermat but corrected by the amendment made by Roberval. In order to justify this amendment, Mersenne refers to the treatise of Benedetti:

It would not be inappropriate to add here a particular remark which has been made concerning the arms of a balance whose weights are in a reciprocal ratio to the length of the arms in accordance to the proposition of Archimedes because it assumed that the two pendants of balances descend along parallel lines instead of inclining towards the center of the earth where they would unite if they were both 1145 leagues long. From this, those who consider the balance in a more precise fashion conclude that the weights mentioned above are in a reciprocal ratio to the perpendicular lines drawn from the center of each weight to the line which connects the center of the earth to the balance, or that they are in a reciprocal ratio[47] computed as the ratio of the inclined lines or the ratio of the angles formed at the center of the earth by the line joining the center of the earth to the center of the balance and the lines which are inclined, that is to say, of the inclination or of the direction of the pendants towards the center of the earth; or rather, that they are formed in a reciprocal ratio of the perpendicular lines drawn from the center of the balance to the inclined lines of the pendants, as Benedetti does in his third chapter on mechanics and which many excellent geometers consider as true.

This very accurate theory which Benedetti undoubtedly borrowed from Leonardo da Vinci and which Roberval, in turn, borrows from him is useless in the face of the obstinacy with which Fermat defends his point of view. He tries[48] to make Roberval contradict himself and believes he is able to do so by concluding from the principles contained in his final letter that a heavy sphere, placed on a plane tangential to the terrestial globe, will move unless it is located at the point of contact. We recognize in this conclusion a proposition of Albert of Saxony which Leonardo da Vinci, Villalpand and Mersenne carefully preserved. Fermat fails to see that if the theory of the inclined plane requires the immobility of such a sphere placed on a horizontal plane, it is due precisely to the fact that this theory does not take into consideration the convergence of the verticals.

The obstinacy of the geometer of Toulouse, who refuses to give in to the laws of solid mechanics is evident in several other instances:[49] the cause for this obstinacy is understandable. The view held by Albert of Saxony which states that the weight of a body is the tendency of the center of gravity of that body to unite with the center of the earth, has been dealt a crushing defeat.

With his usual good fortune, Descartes enters the fray at a moment when he can only gain the laurels of victory. The inexhaustible curiosity of Mersenne prompted him to ask the opinion of the great philosopher on the geostatical problem which pitted Fermat against Roberval and Étienne Pascal. Descartes responded to this inquiry and on July 13, 1638, he sent to Mersenne an *Examen de la question sçavoir si un corps pèse plus ou moins, estant proche du centre de la terre qu'en estant éloigné*.[50]

This examination contains a discussion of Cartesian statics which differs little from that which Constantin Huygens had received some time before. He added to this discussion which we discussed in Chapter XIV several remarks concerning the debate in question here.

We have seen that Descartes knew from Mersenne the propositions advanced by Fermat. After reading the following passages it is obvious that Descartes also knew of the letter in which Étienne Pascal and Roberval refuted these propositions.

> . . . we need to determine what is understood by absolute weight. Most people take it to be a property or an internal quality in each of the objects called heavy which causes them to tend towards the center of the earth.
>
> According to some, this property depends upon the form of the body: according to others on its matter only. According to these two views, of which the first is the most widely accepted in the schools while the second one is acknowledged primarily by those who think they know more than the common man, it is clear that the absolute weight of

objects is always the same intrinsically and that it does not change at all in relation to their varying distance from the center of the earth.

There is yet a third opinion, namely, that held by those who think that all weight is relative and that the force or the property which causes bodies we call heavy to descend is not at all intrinsic, but exists at the center of the earth, either in its entire mass which attracts them to it as a magnet attracts iron, or in some other fashion. And those who hold this third opinion, believe that just as a magnet and all other natural agents which have some degree of activity are always more active at close range than at a distance, so one must also admit that the same body weighs more the closer it is to the center of the earth.

In my own view, Descartes adds, I conceive of the nature of weight in a way quite different from these views. However, I could only clarify my view if I deduce several other propositions which I have no intention of doing here. All I can say is that my view does not teach me anything related to the proposed question, except that it deals with facts alone, that is to say, that it can only be determined by humans to the extent that they can conduct experiments on it. And even then, experiments which can be conducted here on the surface of the earth will not tell us how things are much further below towards the center of the earth, or far above, beyond the clouds, because if an increase or decrease of weight occurs, it is improbable that it happens everywhere in the same proportion.

Furthermore, Descartes is looking at previous experiments in order to determine whether one of them might give information on the variations in weight. The facts seem to prove to him that weight decreases when one rises above the surface of the earth, but the proofs which he gives to this assertion are unusual. He cites "the flight of birds, these paper dragons which children fly, and even, to trust in Mersenne, cannonballs, shot straight upward and which do not [appear to] fall back to earth." Among the arguments he uses, we find one which is of great interest for the universal history of weight:

Another experiment which has already been done and which seems to convince me very strongly that bodies removed from the center of the earth do not weigh as much as those which are closer, concerns the planets which do not have their own luminosity, such as the moon, Venus, Mercury, etc., which are most likely of the same matter as the earth. Since in the judgement of almost all the astronomers of this century, the heavens are liquid, it appears that those planets should be heavy and fall towards the earth, were it not for the great distance preventing them from doing so.

Nevertheless, Descartes does not think that the experiment is advanced enough to permit a geometrical demonstration on variable weight. In his arguments, he assumes it to be constant.

Furthermore, we shall assume that each part of the same heavy body always maintains in itself the same force or propensity to descend, regardless of whether it is approaching or departing from the center of the earth, and regardless of its location. Even though, as I have said before, this might not be true, nevertheless, assume it in order to make our calculations manageable.

Once this invariability of absolute weight has been assumed, we can demonstrate that the relative weight in all solid bodies — when considered in open air and without support — is slightly less when they are closer to the center of the earth than when they are further from the center, even though it is not the same for liquid bodies. On the contrary we can demonstrate that when two equal bodies are placed opposite each other on a perfectly accurate balance, but with the arms of this balance not parallel to the horizon, the body closest to the center of the earth will weigh more and by precisely as much as it is closer to that center. It follows that when there is no balance between the equal parts of the same body, the higher parts weigh less than the lower to the extent that they are further removed from the center of the earth, so that the center of gravity cannot be an immobile center in any body, even when it is spherical.

The first proposition stated by Descartes was the one previously formulated by Blasius of Parma and then rediscovered by Mersenne. Étienne Pascal and Roberval had stated similar propositions in opposition to Fermat. The rules on the composition of forces allow one to easily demonstrate this proposition. However, as we have seen, Descartes does not seem to have ever had an exact knowledge of these laws. And thus, when he proposes[51] to give a "demonstration explaining why a body can be said to weigh less when close to the center of the earth, than when far from it," he makes use of a rather bizarre and unconvincing artifice to deduce this demonstration from the laws of the inclined plane.

The proposition concerning the stability of a balance where the verticals are considered convergent, becomes the object[52] of "another demonstration explaining how the same object can be said to weigh more, when it is close to the center of the earth, than when it is far from it." This demonstration did not require of Descartes a high degree of invention. Étienne Pascal, Roberval and Mersenne had already shown how to correct the reasoning of Fermat by following the principles given by Benedetti.[53] Descartes makes use of the deduction corrected in this manner.

Fermat had already concluded from his incorrect deduction the corollary that a body does not have a center of gravity independent of its position. Descartes proves this corollary again by a correct reasoning:[54]

From this it is evident that the center of gravity of the two weights B and D (Fig. 106) joined by the line BD, is not at point C, but between C and D, for example, at point R where I assume the line which divides the angle BAD into two equal parts falls. . . . Thus the weights B and D must be supported at point R in order to remain in equilibrium in the position which they are in. But if the line BD is considered as being ever so slightly inclined to the horizon, or if these weights are at a different distance from the center of the earth, they must be supported by another point to be in equilibrium, and thus their center of gravity is not always at the same point.

Fermat thought he could assert the invariability of the center of gravity at least for a sphere. Descartes proves that even this exception can not be allowed:[55]

Thus it clearly follows that the center of gravity of this whole sphere is not at the point which is at its geometrical center, but somewhat lower on the straight line which joins this center to that of the earth. This seems truly paradoxical, unless you consider the reason for it. However, as soon as you do so, you can see that it is a well-established mathematical truth.

The statement of Descartes summarizes and settles the debate between Beaugrand, Fermat, Mersenne, Roberval and Pascal. Today, his conclusion is clear and certain. The idea of a center of gravity which is invariably connected to each solid body only makes sense as long as the verticals are considered parallel to each other. Thus it is an absurdity to attribute to this point a tendency to join the center of the earth. It is enough to reflect on the center of the earth to render illegitimate any concept of the center of gravity. This is the important result produced by the dispute among the geostaticians.

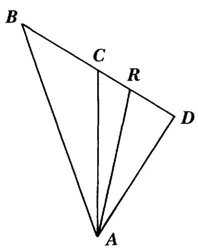


*fig. 106.*

Did Torricelli know about this dispute? Could his research have possibly come under the influence of the ideas discussed among French geometers? There is no doubt in our mind on this point.

We have seen that Torricelli spent a great part of his life in Rome, close to his master, Father Castelli. Only three months before the death of Galileo (January 8, 1642), he left his first master in order to be with the great geometer at Arcetri.

In the midst of the dispute on geostatics, Father Castelli was in contact with Jean Beaugrand. He had heard of the proposition of Fermat on the variability of the center of gravity and had undertaken similar research. We know this from the following letter for which we unfortunately know neither the date, nor the addressee:[56]

I have read the very subtle thoughts expressed by Fermat on the subject of the center of gravity. I readily confess that I found them splendid and quite worthy of that keen intelligence which Beaugrand praised so highly during his stay in Rome. I hope that he has a rigorous demonstration for them. Beaugrand told me that he had obtained a similar proposition, to wit, that the same heavy body, placed at various distances from the center of the earth, weighs differently and that the weight varies in the same ratio as the distance from the center of the earth. So I began to reflect on this matter and I thought at the time that I had found a demonstration. However, since then I have encountered several difficulties and my enthusiasm for such speculations has cooled considerably. I still recall having deduced the same result as Fermat: to wit, a heavy body with a center of gravity coinciding with the center of the earth would have no weight and, furthermore, that the entire earth is devoid of weight.

In addition, I had found that a heavy body which descends towards the center of the earth not only changes weight from instant to instant, but, even more amazingly, the center of gravity continually changes place within the mass of the body. Moreover, if a heavy body rotates in place, its center of gravity changes constantly. Thus I readily agree with Fermat on the following point: That the nature of the center of gravity is not at all as commonly described by the mechanicians.

Thus Torricelli was aware of the errors and contradictions involved each time one considered the center of gravity without considering the verticals to be parallel. From this one can easily understand why he was careful to formulate the latter hypothesis with such great precision. By doing do, he profoundly transformed the principle of statics which he had taken from Galileo. He removed every trace of the erroneous doctrine to which this principle owed its existence. Like so many principles in physics it is by denying its own origin that the Principle of Torricelli became an irrefutable truth. But, by breaking all ties with the error which had produced it, it also lost its apparent self-evidence which caused its wide acceptance. It thus appeared as it really was: a pure postulate, justified solely by having its deductions agree with reality.

CHAPTER XVII

# THE SYSTEMATIZATION OF THE LAWS OF STATICS

## 1. F. MARIN MERSENNE (1588–1648), BLAISE PASCAL (1623–1662), F. ZUCCHI (1586–1670), F. HONORÉ FABRI (1606–1688)

When the XVIIth century reached its midpoint, the work in statics undertaken by Stevin, Galileo, Roberval, Descartes and Torricelli had been completed. But at the beginning of the XVIth century, both the mechanicians of the School of Jordanus as well as of Leonardo da Vinci, had already anticipated the most important truths of statics. Then these truths were obscured and the narrow-minded and biased criticism of geometers cast them into oblivion, just as the fog sometimes breaks and allows us to see the brilliant snow-capped peaks, before another cloud hides them again from view. Today, the most important propositions among those comprising the science of equilibrium have been precisely formulated and the silhouettes of the major peaks stand out clearly. But the science of statics is far from being finalized. A scientific theory is not a compilation of several important truths which are unrelated to each other. It is a system in which these truths are meshed with each other, a systematic classification, whose order demonstrates the natural affinities between diverse principles. No mechanician at this point has a clear vision of this interconnection. Although the principle peaks are already illuminated in brilliant light, the foothills which unite them into a monolithic chain are still buried in shadows.

Occasionally, one summit will obscure the view of a neighboring peak. Descartes, who is able to delineate so clearly the Principle of Virtual Displacements, has only a very vague and imprecise notion of the law of the Composition of Forces.

In order for statics to become a finished discipline, one important task remained to be done. All of the various laws previously discovered had to be grouped into a single, coherent system, in order to show how they were in accord with each other, how they derived from each other and how in each circumstance, they defined the sufficient and necessary conditions to ensure equilibrium.

No one wished more ardently to accomplish this work of systematization and coordination than Father Mersenne, and no one worked more actively to bring it about. Unfortunately, the energetic brother of the Minims was not suited to accomplish the task which he had set for himself. To organize in a harmonious theory all of these disparate and incongruous propositions required a clear and profound understanding of the principles, an extreme rigor in deduction and a very sure and finely honed critical mind. Mersenne was endowed only with the boundless curiosity of a collector and the exuberant imagination of an artist. Thus, instead of the logical system which should have been constructed, he achieved but a compilation.

It was, to be sure, a very complete compilation for which Mersenne had drawn from the works of almost all contemporary mechanicians. As early as 1626, Mersenne had published his *Synopsis mathematica*,[1] a long list of propositions drawn from either the geometers of Antiquity or from contemporary authors. Besides the theorems comprising the treatises of Archimedes, Mersenne reproduced the major formulations contained in the works of Commandino and Luca Valerio. He also added many excerpts borrowed from Simon Stevin, Guido Ubaldo, Villalpand, and many others. Up to the end of the XVIIth century, the *Mechanicorum libri* provided the material for many treatises on statics, especially after Mersenne reedited his *Synopsis*[2] in 1644.

In 1634 the *Mechanics* of Galileo was published. The indefatigable compiler was not satisfied with merely translating the works of the great geometer of Pisa. He also added "several rare and new additions, useful for architects, well-drillers, philosophers, and craftsmen." Among these additions several are borrowed from the *Mechanics* of Guido Ubaldo.[3]

The year 1636 witnessed the publication of the *Harmonicorum liber*, where Mersenne reports on the early works of Galileo on the accelerated fall of heavy bodies. Statics plays here a minor role but there is mention of a study[4] on the manner of the variation of a weight of a heavy body suspended from the extremity of a lever when the lever is rotated around the point of support. The influence of Benedetti, whom Mersenne does not quote here by name although he will mention him in another work, is very evident.

In the same year, 1636, Mersenne published in French his *Harmonie universelle*, which contains the *Treatise on Mechanics, on weights supported by forces on planes which are inclined to the horizon; on the forces which support a weight suspended from two ropes*, by G. de

Roberval. The energetic Brother of the Minims, however, not only adds the work of Roberval to that part of his work entitled: *A Treatise on the Nature of Sound and Motion of all kinds of Bodies. Second Book. On the Motion of all kinds of Bodies.* After having presented in this same part the theory of Galileo on the fall of bodies and criticizing the hypothesis of that great physicist on the inclined plane — hypotheses which were not in accord with Mersenne's own experiments — Mersenne examines,[5] "Proposition 9 of Book Eight of the *Mathematical Collections* of Pappus, which asks what force is required to support a given weight on a level plane inclined towards the horizon at a given angle which I have already discussed in great detail in the Fourth Addition to the *Mechanics* of Galileo. This is why I am adding here only the demonstration given on this by M. de Roberval, one of the most outstanding geometers of this century."

Roberval is not the only mechanician whose work was analyzed in the *Harmonie universelle*. A little further on, after the above quoted passage, Mersenne shows us[6] that the law of the inclined plane as formulated by Cardan in the *De proportionibus*, is not correct and furthermore, that "Cardan, Tartaglia and Guido Ubaldo all failed in their analysis of the balance."[7]

In another passage[8], Mersenne turns his attention to the decrease in weight in a body as its distance from the earth increases. This same concern can also be found in several passages of the *Nouvelles observations physiques et mathématiques*, which were to be added at the end of the *Harmonie universelle*. Here we see Mersenne[9] concerned with the objection raised by Fermat to the theory of the lever of Archimedes. He takes into account the convergence of verticals by using the theorem of moments — "as Benedetti does in his third chapter on mechanics and which several excellent geometers consider to be correct." On this same topic, he had previously restated the strange argumentation of "M. Fermat, Counselor to the Parlement of Toulouse and eminent geometer" but he also added: "I fail to see the cogency of this demonstration."[10] He had also announced the upcoming publication of the *Géostatique* of Beaugrand.

The *Harmonie universelle* dealt with a great many questions on mechanics. However, these questions were scattered throughout the different parts of the work and were not sufficiently coherent to provide a treatise on mechanics. Several years later, Mersenne did attempt to write such a treatise and added it to one of those obtuse and rambling works which he was wont to publish on the most diverse topics. The *Tractatus mechanicus, theoricus et practicus*, published in Paris in 1644 with Antoine Bertier, formed the second part of the *Cogitata physico-mathematica*.[11]

This *Tractatus mechanicus*[12] is in reality only a rather unsystematic compilation of information acquired by Father Marin Mersenne on statics. The *Praeludium*[13] which begins it contains several drawings[14] concerning the lever and the inclined plane. The *Mechanical Problems* of the Stagirite provided the inspiration for the demonstration of the law of the lever, deduced from the velocities of its extremities. Propositions II and V[15] dealing with the notion of moment were obviously influenced by Benedetti, while Proposition VI[16] restates the argumentation of Guido Ubaldo who seems unfamiliar with this notion. Proposition X[17] is a development of arguments borrowed from Galileo on the application of pulleys. However, Mersenne owes the most to Descartes and Roberval.

Our energetic compiler quotes almost in its entirety the letter which Descartes, that "Vir clarissimus,"[18] had written him[19] on July 13, 1638 and which he discussed in Chapter XIV.[20] This letter provides him with the theory of the lever[21] and the inclined plane,[22] to which Mersenne applied the axiom of Descartes and, finally, the apparent variation of the weight of a body as it moves away from the center of the earth.[23]

The calculations of the force which is required to act along a parallel or oblique path on an inclined plane in order to maintain a heavy body in equilibrium on that plane is taken[24] from the *Traité de Méchanique* written by Roberval and included in the *Harmonie universelle*.

Moreover, it is not in the *Tractatus mechanicus* that Mersenne reproduces the theory of Roberval on the parallelogram of forces but he gives it only in the *Ballistica et Acontismologia* in Propositions V and VI.[25]

Finally, let us add that the influence of Stevin is not entirely absent from the statics of Mersenne, although it is less evident than that of the authors previously discussed. Stevin's influence is more evident in other parts of the *Cogitata physico-mathematica*. The term *antisacoma*[26] used by Mersenne in a certain passage[27] is indubitable evidence of that influence, which is especially clear and important in hydrostatics, for Mersenne borrows this science almost entirely from the geometer of Brugge.

The treatise on mechanics is similar and, in addition, is composed of fragments taken from the most diverse texts and put together in a primitive mosaic where there are no smooth transitions between the jagged edges of the ill-fitted pieces. Mersenne was obviously not the man to harmonize works of such dissimilar nature, containing so many apparently contradictory principles.

Everything that Mersenne lacked to be able to reduce statics to a coherent system of propositions, namely, a profound understanding of principles, a rigor in logical deduction, a sharp critical mind — all these qualities were possessed to the highest degree by Pascal, who was supremely well-prepared for the task at hand. It appears, indeed, that he attempted that task, but unfortunately, his undertaking has not come down to us, but we know of its existence from a passage in the *Treatise on the Equilibrium of Liquids*, which Périer published in Paris in 1663, one year after the death of his brother-in-law. We read the following in Chapter II which is entitled: Why liquids exert weight according to their depth:

> Here is another proof which will only be understood by geometers and which others can ignore.
>
> I take as a principle that no body ever moves by its own weight without its center of gravity descending. . . .
>
> In a short *Treatise on Mechanics* I have demonstrated by using this method the reason for all of the multiplications of forces present in all the other mechanical instruments invented till now. I demonstrate for all of them that the unequal weights which are in equilibrium by means of machines are, because of the construction of the machine, so arranged that their common center of gravity could never descend, no matter what position the weights take. It follows that they must remain at rest, that is to say, in equilibrium.

The principle adopted here by Pascal in his short *Treatise on Mechanics* is therefore the principle formulated by Torricelli.

Moreover, Pascal did not fail to recognize the value of the axiom formulated by Descartes. We read the following in this same Chapter II of the *Treatise on the Equilibrium of Liquids*:

> We must admire that fact that this new machine retains the same working principle to be found in all the Ancient machines, such as the lever, the press, the endless screw, etc. This principle is that displacement increases in the same proportion as force . . . so that the displacement is to the displacement as the force is to the force. What can be taken as the true cause of this effect is that it is evident that it is the same thing to move one hundred pounds of water by one inch, as to move one pound of water by one hundred inches; And that, when one pound of water is equilibrated with one hundred

pounds of water, the hundred pounds could not move by one inch without moving the one pound weight by one hundred inches. They must remain in equilibrium, since one pound has as much power to move one hundred pounds by one inch, as one hundred pounds has to move one pound by one hundred inches.

Thus Pascal accepted both the axiom of Descartes and that of Torricelli. However, we neither know whether he was able to show why these two principles produce the same results nor whether this question even interested him.

A talent for critical analysis was surely the gift most valued by Father Zucchi. It is with great incisiveness and subtlety that he reveals in his *Nouvelle philosophie des machines*[28] everything inadmissible in the statements which Aristotle made in the first chapters of his *Mechanical Problems*. He shows no less insight when attempting to elucidate the implicit but not self-evident postulates which Archimedes used in order to demonstrate the law of the lever.

However, his talent for critical analysis was not sharp and certain enough to keep Father Zucchi from error and fault in working through various principles of statics proposed by contemporary geometers. Confronting these incongruous axioms, he vacillates and, faced with ill-defined concepts, he is confused.

In one of his axioms,[29] for example, he uses the word *virtus* to mean what Descartes meant by *force* and which we designate today as *work*. However, in the following axiom, the term *virtus* has now assumed the same meaning for what we call today *force*. The first of these postulates seems to imply that the author intends to base his entire statics on Cartesian principles:

What is sufficient to lift a given weight to a given height, is also sufficient to lift a weight K times larger to a height K times smaller.

However, the reasoning takes an unexpected turn and he soon finds himself led to the Peripatetic principle:

What is sufficient to move a given weight at a given velocity is also sufficient to move a weight K times larger at a velocity K times smaller.

Nevertheless, after generalizing the remark made by Galileo concerning the inclined plane, Zucchi is careful to correct the Peripatetic axiom:[30]

The velocity or the slowness of the motion must be calculated considering the line of inclination of the motor or resisting force. In the particular case of weights, it must be calculated with respect to the vertical because the tendency of these weights toward downward motion or their resistance to upward motion is directed along this line

This quote shows how easily the contemporaries of Descartes extended to forces in any direction what they knew to be true about weights. Thus it should not surprise us to see in Section 3 how Wallis makes a similar generalization of the axiom of statics formulated by the great French philosopher.

At the moment when the first edition of the *Nova de machinis philosophia* of Zucchi was published in Paris, another Jesuit scholar was attempting to present dynamics in a completely logical form where the mathematical laws of that science would be deduced clearly from the principles of natural philosophy. This Jesuit was Father Honoré Fabri. Born in the Bugey in 1606 or 1607, Father Fabri was a Professor at the Jesuit college in Lyon before he became Grand Inquisitor of the Holy Office. He died at Rome on March 9, 1688. At the beginning of his scientific career he was frequently in contact with Father Mersenne.

Father Fabri did not publish under his own name the results of his reflections on local motion. The work which contains those results was published under the name of a friend of Father Fabri, Pierre Mousnier, Doctor of Medicine.[31]

The work published by Pierre Mousnier is mainly a treatise on dynamics and is of the greatest interest for the history of this science. However, since statics is in the final analysis only a very special case of dynamics, it comes as no surprise that this treatise also deals with statics.

Book V, entitled: *De motu in diversis planis*[32] explains the theory of the motion of a heavy body placed on an inclined plane. This theory presupposes the previous determination of the apparent weight of such a heavy body.

Father Fabri bases this determination on the following axiom:[33]

A heavy body only moves spontaneously when it descends.

From this postulate, he deduces the following corollary from which follows the complete theory of the inclined plane:[34]

The motion of a heavy body is hindered according to the ratio between the distance it must traverse to attain a given height or to increase its distance from the center by a given amount and this vertical height.

Don't we recognize in this formulation the old axiom of Jordanus: *Gravius in descendendo quando ejusdem motus ad medium rectior?*[35]

This is not the only trace of medieval science contained in the work of Father Fabri. On the subject of the convergence of the verticals, it contains, for example, all of the paradoxes conceived by Albert of Saxony and his School, and which Villalpand, Bernardino Baldi and Mersenne had assembled.[36]

Furthermore, Father Fabri, or his interpreter Pierre Mousnier, is not satisfied with the brief reference to statics contained in the book devoted to the inclined plane. There is also an appendix[37] dealing specifically with the study of machines designed to lift heavy bodies. The fundamental laws which govern the use of these machines are related to the principles on which the erudite Jesuit based his dynamics.

The statics of Father Fabri is more clearly based on the statics of Galileo than that of Father Zucchi. That is to say, in the final analysis it is based upon the statics of Aristotle, but modified by taking into consideration the inclined plane. This is clearly evident from several axioms stated at the beginning of his statics:

The same force will produce a small movement more easily than a large movement in the same moving body. — A motion is all the smaller, the slower it is, that is to say, the more time it requires to traverse a given distance. — A weight equal to a second weight cannot move it by an equal motion. — A weight equal to a second weight cannot move it with a smaller motion. — A weight moves more easily along a oblique line than along a vertical in so far as the oblique line is longer than the vertical. — One weight can move a larger weight, provided that the displacement of the latter is smaller than the displacement of the former and that the ratio of the displacement is less than the ratio between the weights. — In order for one weight to pull a smaller weight with a greater motion than its own, it is necessary that the ratio between the weights be greater than the ratio of the motions.

After having stated these axioms, the author formulates in the following terms the "most universal problem" of statics:

To move a given weight by means of an arbitrary force.

He then furnishes the general solution:

To cause the motion of the weight to be less than the motion of the force and the ratio between the motions to be greater than the ratio between the weights.

To this solution he add the following "most universal corollary:"

It follows that any activity which has as its goal to move large weights consists in making their displacement increasingly slow. You can increase the weight being moved proportional to the decrease in displacement.

A few very brief remarks hint at the application of this principle to the lever, the block and tackle, the winch, the screw, the cogged wheel and the inclined plane.

The influence of Descartes, so evident in certain parts of the dynamics formulated by Father Fabri, is totally absent here. The entire statics of the erudite Jesuit is based on the notion of "momento" as it was conceived by Galileo.

## 2. THE *TRAITÉ DE MÉCHANIQUE* OF ROBERVAL

It is only in an off-hand way, that is, in an appendix to the theory of local motion, that Father Fabri dealt with mechanics. He had only presented in a very generalized and concise form the principle which justifies its applications. His lessons, published by Pierre Mousnier, could in no way assume the status of a treatise on statics. Nor could this claim be made by the work of Father Zucchi, because his treatise was far from being a complete treatise on statics. It was rather a critical essay on the principles of mechanics.

Roberval, on the contrary, set out to write a complete treatise on mechanics. The friends of the Professor of the Collège de France anxiously awaited the publication of this work. When Mersenne restated in his *Cogitata physico-mathematica*, the theorems of Roberval on the inclined plane, he hoped to stimulate:[38]

those devoted to the study of mechanics to ask of our great geometer, the equal of Archimedes, an account of the other parts of this science and to demand it so insistently that they will indeed obtain it, to the greater honor of learning.

These demands were not sufficiently strong to overcome the aversion which Roberval apparently felt towards the publication of his works.

However, the treatise on mechanics of this great geometer had not remained a mere draft, but, according to a letter addressed in 1650 by the author to Hevelius,[39] had been brought to completion. This letter even informs us of the titles of the eight books which the work was to comprise.

We have constructed, says Roberval, a new mechanics from the foundation up; except for a few, all the blocks of Antiquity, with which it had been previously constructed, have been discarded. It is complete in eight stories, which correspond to the books of the same number.

The first book deals in a general way with the virtual center of force.[40] We will attempt to see if such a center exists, to which force it applies and to which forces it does not apply.

The second treatise concerns the balance and examines the weights which form equilibrium.

The third deals, in detail, with the virtual center of force.

The fourth discusses an extraordinary case of plagiarism.

The subject of the fifth is instruments and machines.

The sixth deals with forces which act within certain media; it concentrates on floating bodies.

The seventh considers compound motion.

The eighth, finally, deals with the center of percussion of moving forces.

This treatise has not come down to us.

Long after the death of Roberval a brief fragment of a text called: *Projet d'un livre de Mécanique traitant des mouvemens composés*[41] was published[42] as a supplement to his treatise on geometry which was entitled: *Observations sur la composition des mouvemens*.[43] This fragment, to which we shall turn our attention in Section 4, can be considered an introductory essay for the seventh book of the treatise on mechanics. However, this introduction is limited to what would have been contained in the first pages of that book.

Other fragments of Roberval on various topics in mechanics, almost all of them unpublished, can be found in a manuscript notebook kept at the Bibliothèque Nationale.[44]

Among these fragments there are several which must certainly be considered as drafts for one of the books of the *Treatise on Mechanics*, mentioned in the letter to Hevelius.

The *Tractatus mechanicus* at the beginning of the manuscript notebook seems to be nothing other than the beginnings of the first book of this treatise. It is indeed precisely the virtual center of force which Roberval introduces as the purpose of his deductions.

Roberval defines what he means by *puissance* (*virtus seu potentia*). He gives this word exactly the same meaning which we give to the word force. That is the meaning, incidently, he gave as early as 1636 in the letter to Fermat which we already mentioned in the preceding chapter:

In general, we use the term force to designate the quality by which any object tends or aspires to move toward another location, either downwards, upwards or sideways, and it does not matter whether this quality is naturally inherent in the body or if it is imparted to it from some other source. From this definition, it follows that every weight is a kind of force, since it is a quality by which bodies aspire to lower positions. Often, we call by the term force the things in which the force is inherent, as for example, a heavy body can be called a weight.

In today's language we would say that it is the composition of forces applied to a solid object which Roberval intended to study in the first book of his *Treatise on Mechanics*, and of which the *Tractatus mechanicus* of 1645 undoubtedly forms the beginning.

Right at the beginning the problem is posed in very general terms. A body can be a point, a line or a surface. It can be extended in all directions and the forces can be arbitrary. But these general terms soon become restricted, explicitly and implicitly. In fact, Roberval asserts that the force inherent in each of the elements of a solid body has an invariable magnitude. He further asserts that it either has a fixed direction or that it is directed towards a fixed center.

These restrictions legitimize the fundamental Postulate ranked as third in importance by Roberval and which he states in the following way in his letter of 1636:

If a weight is suspended from or attached to a flexible weightless line attached at one end to some support, so that it sustains the force which unimpededly pulls on this line, the force and the line will assume a position in which they will remain at rest, and the line will be of necessity straight. Let the given line be called the pendant or the line of direction of the force.

The *Tractatus mechanicus* of 1645 examines only a very special case of the already restricted problem quoted above. This is the case in which all of the forces acting upon the solid body are parallel to each other and to a fixed direction. This special case is, by the way, studied with great logical rigor. By a method inspired by both Archimedes and Pappus, the existence and the properties of the center of parallel forces are established.

The beginning of an analysis of the composition of forces applied to solid bodies concludes this fragment, without however completing it. The largest and most innovative part of the first book of the *Traité de Mécanique* mentioned to Hevelius, a book whose overall structure we can guess at from the letter addressed to Fermat in 1636, is missing.

The second book of this treatise deals with the balance. The *Demonstratio mechanica* preserved in manuscript in the Bibliothèque Nationale was undoubtedly meant to be part of this second book. This "Mechanical Demonstration" is the demonstration of the law of the lever. In form, it imitates the rigorous deductions of the Greek geometers. In substance, it resembles the deduction adopted by Stevin and Galileo.

According to the letter addressed to Hevelius, Roberval dealt in his third book with "virtual centers of forces, in particular." What did he mean by that? He was undoubtedly referring to his geometrical analysis of the centers of gravity of various figures, because a good part of his talent as a geometer was devoted to such analysis. We are probably dealing with part of the material used in writing this book when we read in the manuscript kept in the Bibliothèque Nationale a *Proposition of M. de Roberval for determining centers of gravity* and a *Lemma Marvelously Suited for the Determination of Centers of Gravity by M.* de Roberval, 1645.

The proposition, which forms the main topic of these two texts, formulates the fundamental definition of the center of gravity for an arbitrary number of material points. The moment of the total mass of points with respect to any given plane with their centers of gravity joined is equal to the algebraic sum of the moments of these points with respect to the same plane. This theorem formed the implicit basis for all the research on centers of gravity, as well as the research undertaken in Antiquity by Archimedes or Pappus, and also that research undertaken in modern times by Commandino, Maurolico, Guido Ubaldo, Stevin and Luca Valerio. To be more precise, this research used a particular case of this theorem. The case is where the plane chosen passes through the center of gravity. However, in our opinion, it had never been stated or demonstrated in its entire generality.

The demonstration of Roberval is carried out with the extremely complicated deductive approach characteristic of our geometer. In the Latin edition of the *Theorema lemmaticum*,[45] this complexity is indeed excessive and one could only wish for more brevity and simplicity. Interesting applications of the lemma demonstrated there are added to this Latin edition and concentrate on the determination of the centers of gravity for the semi-circle, the arc partial circumference, the trochoid[46] and the curve associated with the trochoid and triangle.

The title of the fourth book: *Quartus, de fure mira continet*[47] confirms for us that the third book promised to Hevelius was concerned with the determination of particular centers of gravity. Roberval, undoubtedly, wished to relate the strange theft by Torricelli which victimized him. In his *Histoire de la Roulette*,[48] Pascal told us of this shameless case of plagiarism.[49]

A fragment dealing with floating bodies: *Proposition fondamentale pour les corps flottants sur l'eau*[50] concludes the manuscript notebook kept in the Bibliothèque Nationale. It most likely was to serve as a basis for the composition of the sixth book of the *Traité de Méchanique.*

Our manuscript contains nothing about compound motion with which the seventh book was supposed to deal. The *Projet d'un livre de mécanique traitant des mouvemens composés*,[51] published in 1693, seems to be, as we have indicated before, a draft for the beginning of the seventh book.

The subject of the eighth book was the center of percussion of mobile forces, which had caused a lively dispute between Roberval and Descartes. The manuscript kept at the Bibliothèque National does not contain anything pertaining to this subject.

If we leave aside a rudimentary treatise which we shall discuss later, we find nothing in our manuscript which could have influenced the composition of the fifth book, which deals "with instruments and machines". This deficiency is most unfortunate, since Roberval most certainly provided in that book the complete demonstrations which are only sketched out in the *Traité de Méchanique*, which was inserted by Mersenne[52] in the *Universal Harmony.*

Thus we do not have in our possession the *Traité de Méchanique* in the form which Roberval mentioned in his letter to Hevelius. The manuscript notebook kept in the Bibliothèque Nationale only furnished us with several fragments which Roberval apparently had had collected and classified so that he might use them in composing this great work.

However incomplete and incongruous this material available to us is, it is enough to allow us to sense the scope and the basic structure of the finished work. The loss of this work seems beyond recovery and should be deeply regretted. It is most certain that the *Treatise on Mechanics* of Roberval was a grandiose and powerful achievement in which the doctrines elaborated at the beginning of the XVIIth century were ordered and classified. The concern for a rigorous deduction even in the most minute detail certainly made this work verbose and complicated, However, those geometers who wished for a perfectly clear development of the science of equilibrium, were entirely satisfied in their wishes.

Roberval had not only taken into account the aspirations of the geometers so fond of erudite and rigorous deductions, but he had also had in mind the needs of practicing artisans. The latter lack the intelligence and the leisure to follow the arguments by which it is possible to deduce methodically from a small number of simple and general postulates the various laws of mechanics. However, because they need to make use of such laws, they also need to have a clear, precise and solid knowledge of them. It was undoubtedly with that purpose in mind that Roberval composed the *Traité de Méchanique et specialment de la conduitte et elevation des eaux, by M. de Roberval,*[53] a treatise which has unfortunately remained unfinished, but which fills the major part of the manuscript at the Bibliothèque Nationale.

Although this *Treatise on Mechanics* is not dated, a passage in it contains information indicating its date of composition. When Roberval discusses the raising of water by means of the "*Siphon*" he expresses himself in the following terms:[54]

> And even though it appears possible to transport water by this means over a high mountain, one should remember that such a conveyance is impossible above a height of 32 French feet. Even slightly below 32 feet, it is quite uncertain, for two reasons: the first is that it is very difficult to obtain a siphon, which is so perfectly welded that it is air tight. And once air enters into the siphon, water will no longer flow. The second reason is that at a great height one needs too long a siphon and runs the risk of it breaking.

The experiment of Torricelli showed that the atmospheric pressure is the true reason for the phenomena mentioned by Roberval. It is clear that at the time he was writing the *Treatise on Mechanics* Roberval had no knowledge of this famous experiment. However, it is in 1644, upon returning from a trip to Italy, that Mersenne repeated in Paris the experiment of Torricelli and "made it known throughout France to the great admiration of all scholars and interested parties."[55] As a close acquaintance of Mersenne, Roberval must have been one of the first to know about the important "Italian experiment." Since there is no trace of it in the *Treatise on Mechanics*, the latter must have been written before 1644.

In this *Treatise on Mechanics* there are no definitions, postulates or deductions, but it is a very clear and simple account, totally devoid of abstruse scientific pretension and presents the major lessons of mechanics. When reading this short work, one is reminded of the two masterpieces of Pascal, the *Treatise on the Equilibrium of Liquids* and the *Treatise on the Weight of Air.* Roberval's *Treatise on Mechanics* is written in the same spirit. Dynamics and the mechanics of fluids form the main body of this work.

But, at the outset, the author says, we shall furnish enough information on the instruments of mechanics as is necessary to construct those needed to pump and raise water.

That is the reason why the treatise begins with a study of the "five major types of common instruments whose forces are known: to wit, the balance, the lever, the wheel and axle, the pulley or the block and tackle and the inclined plane, to which the wedge and the screw may be reduced."

In this text one can find evidence of the influence of Bernardino Baldi on Roberval. We have pointed out such evidence elsewhere.[56] Let us only mention here the discussion concerning the stability and sensitivity of the balance. Not only does Roberval restate very accurately what Baldi had said on this subject,[57] but he even transforms into an obvious error an obscure passage written by the Abbot of Guastalla. In his discussion on the balance with the center of gravity of the beam below the axis of rotation, Roberval expresses himself in the following terms:[58]

The third type is subject to error when the center of gravity is below the center of movement.

We can not expect to find any new truths on statics in this rudimentary treatise. Roberval merely formulates with clarity and simplicity laws already known through the work of his predecessors or of his contemporaries. Thus the properties of the inclined plane are presented with great care. Let us merely quote the following passage,[59] on the equality between motor work and the work of the resistance in machines. It scarcely differs from what we read in the *De Subtilitate* of Cardan or in *The Explanation of Moving Forces* of Salomon of Caus:

Finally, it is necessary to note that what is true for the lever is also true for all other instruments with respect to the displacement and the path traversed by the weights and the force which moves them by means of the instrument, namely, that if they act over equal arms or over equal displacements they traverse equal paths. If they act over unequal displacements, the one acting through the larger, traverses the longer path in the same proportion as its displacement is longer. Therefore, it follows that the smaller of the two — whether the force or the weight — must be the one with the longer arm or the greater displacement for compensation and will also be the one which will traverse the longest path. It also follows that, proportionately, the weight acting with the longer arm will need more time to cause the other to move, and vice versa. If, for example, we take a small force for the purpose of moving a large weight, this small force will need, proportionately, a longer arm and, therefore it needs to traverse a great distance and therefore, requires a lot of time while the weight will traverse a much shorter path, namely, if the arm of the force is ten times longer [than that of the weight],[60] it must travel through ten feet to move the weight one foot. In this way, the weight moves very slowly and requires a great amount of time to move a short distance. What we have just said should serve as a warning that one should not hope to be able to save time and force simultaneously, or to produce a large effect with a small amount of force, except over a long period of time. This is the common mistake of the ignorant and causes other ignorant people to mock them but also science undeservedly as well.

Therefore, Roberval dedicated a large part of his effort in constructing a vast and rigorous treatise on mechanics intended for the use of geometers and in composing a rudimentary account of the same science for the convenience of practicing artisans. But in accordance with a strange custom of his, he did not have the two works printed. The first one is lost while the second one is still unpublished. Thus both *Treatises on Mechanics* remained unknown and were unable to satisfy the ever increasing urgent needs of both geometers and artisans for a complete and systematic statics.

## 3. JOHN WALLIS (1616–1703)

If not the artisans, the geometers soon saw their wish fulfilled with the publication of the monumental treatise of John Wallis.[61] Indeed, the three volumes which the great Englishman devoted to statics, dynamics and hydrostatics form a true monument to mechanics and, moreover, present the most inclusive and systematic work written since Stevin.

Furthermore, the statics of Wallis is not dissimilar to the statics of Stevin. It contains the same concern, sometimes exaggerated, for geometrical rigor, the same desire to admit no supposition, however clear it might be, and no corollary, however evident it might seem, without a formal and precise statement pointing it out. It must be admitted that it is equally tiresome to read these two works because of their excessively complicated logical apparatus.

Upon which hypothesis should all of statics be based? In any given machine, two forces (*potentiae*) oppose each other and must precisely counterbalance each other in order to establish equilibrium. One is the motor force (*vis motrix*), the other is the resistance (*resistentia*). How can the efficacy of each of them be evaluated, either to determine the displacement of the machine or the prevention of that displacement?

We have two possible solutions. The first is that which Galileo deduced from ancient Peripatetic dynamics. In order to measure the efficacy of a weight, whether motor or resistant, its *momento* must be calculated, that is to say, one must multiply the weight by the velocity of the motion at its point of application, or even better, by the projection of this velocity onto the vertical.

The second solution is that which derives from the School of Jordanus and was adopted by Hérigone and Roberval and then formulated with precision and defended with vigor by Descartes. In order to measure the efficacy of a weight, one must multiply the weight by the path described by its point of application, or to be more precise, by the projection of this path onto the vertical.

Wallis vacillates[62] between the two solutions and, instead of unequivocally choosing one of them, he wavers and espouses a bizarre compromise.

The efficacy of the motor force can be measured by its *impedimentum*. So, while the *momentum* will be the product of the motor force and the velocity of the point of application, the *impedimentum* can be determined by multiplying the resistance by the path traversed by the point of application.

> Momentum appello, id quod motui efficiendo conducit. Impedimentum, id quod motui obstat, vel eum impedit. Momentum eadem ratione a verbo moveo descendit, atque Impedimentum ab impedio . . . Ad momentum refero vim motricem et celeritatem.[63] Quae, quo majora sunt, eo magis efficitur motus. Ad impedimentum refero resistentiam et distantiam. Quae, quo majora sunt, eo magis motus impeditur.[64]

One cannot claim that equilibrium is the result of the equality between the *momentum* and *impedimentum*. These differ in kind, and equality between them is impossible. A *momentum* which exactly counterblances an *impedimentum* is not equal to it. In Wallis' terms, it is *equipollent* to it.

This awkward articulation between the Galilean and the Cartesian doctrines can not but uselessly complicate the propositions of statics. It makes the first chapter in Wallis' mechanics seem very awkward and crude. This great geometer undoubtly did realize this later because he abandoned this bizarre compromise and beginning with the second chapter[65] he became a resloute Cartesian.

A heavy body, he says, tends to descend, as long as it is unimpeded. It only descends by as much as it approaches the center of the earth. It only rises by as much as it moves away from this center. Its propensity for a given motion is measured by the *magnitude of its descent* in its motion. Its resistence to a given displacement [motion] is measured by the magnitude of its ascent within this displacement [motion]. The magnitude of the descent of a body is the product of its weight by the distance it falls. The magnitude of the ascent is, likewise, the product of the weight times the distance it rises.

When dealing with a system of several heavy bodies, it is possible to calculate, on the one hand, the sum of all the descents, and on the other hand, the sum of all the ascents. If the first sum exceeds the second, that excess respresents the magnitude of the total descent. If the first sum exceeds the second, that excess represents the magnitude of the total ascent. Between these two possibilities is a third where the sum of the descents is exactly equal to the sum of the ascents.

In the first case, the system tends to move in the direction anticipated by the calculation of the partial descents and ascents. In the second case, it tends to move in neither direction and remains in equilibrium.

Such are the principles formulated by Wallis which gave a very general form to the Cartesian axiom. But the great English geometer will generalize that axiom even further. In almost every case Descartes had assumed that the forces in balance were weights and had limited the statement of his principle of statics to this particular case. We remarked in Chapter XIV how easy it was to generalize the principle making it applicable to every kind of force. Although the great philosopher was probably aware of the possibility of such a generalization, he, nevertheless, neglected to formulate it. Wallis will not only call attention to this generalization, but will emphasize it.

He begins by remarking,[66] as Descartes had done before him, that the fundamental principle of statics does not imply any hypothesis concerning the nature of gravity. It makes no difference whether one considers it an innate quality in all heavy bodies, or an attraction similar to electric or magnetic actions exerted by the earth, or finally, a pressure pushing heavy bodies towards the center of the globe. It suffices to understand by the term, gravity, the force which is obvious to our senses and which moves heavy bodies downwards, regardless of its inherent nature.

However, if the laws of statics concerning gravity contain nothing which depends on the particular nature of this force, they must apply, *mutatis mutandis*, to every kind of force:

What we have said on the subject of gravity and the center of the earth can also be said about any given motor force and the goal towards which it strives. The descent of a heavy body is measured[67] by the amount by which it moves towards the center of the earth. Its ascent is measured by the amount by which it moves away from the center. Thus, in a very general sense, the forward motion due to a motor force is measured by the displacement effected in the direction of that force, and the backward motion is measured by the displacement in the opposite direction.

The magnitudes[68] of the descents of the various heavy bodies are in the same proportion to each other as the products of the weights and the distance of their fall. The ascent can be calculated in a similar way. . . . Thus, in a very general sense, the motion forward or backward under the action of any motor force can be obtained by calculating the product of the force by the distance of their forward or backward motion taken along the line of direction of the forces.

The principle is thus quite clear and allows one to generalize from the consideration of weight to the consideration of any given force. It is now easy for Wallis to lay the foundations for a universal statics. All he must do is add the following words to the statements where he formulates the hypothesis on which the statics of heavy bodies rests:[69] *Idem intellige, mutatis mutandis, de quacumque vi motrice.*

Thus it is in this way that the English geometer formulates the fundamental principle of his statics and demonstrates a profound grasp of the works of his predecessors, be they Torricelli,[71] Jordanus,[72] Tartaglia or Guido Ubaldo.

What effort was required of Wallis to generalize this principle as formulated by Descartes? Almost none. All he had to do was to render more explicit several statements implicit in the undertaking of the great philosopher, and to produce certain generalizations which were obviously necessary at the very outset.

On the other hand, when Jean Bernoulli set out to state the Principle of Virtual Displacements, what modifications did he have to make to this postulate of Wallis? Again, almost none. What Wallis takes into consideration when he sets out to determine the tendency of a force to produce a given motion, was later called the *virtual work* of that force. He defines equilibrium as equality between the sum of the positive virtual moments and the sum of the negative virtual moments.

It is true that Wallis in his formulations is only considering virtual displacements, which he assumes to be rectilinear. Furthermore, he assumes the forces to be constant in magnitude and direction. But, he also foresees the infinitesimal procedures which will obviate these limitations. Like Descartes, he recognizes[73] that a curvilinear trajectory may be replaced by its tangents, and a curved surface on which a weight is resting by its tangential plane. He also recognizes[74] the analogous artifice which will allow one to take into consideration forces of variable magnitude and direction.

When Jean Bernoulli is ready to give his definitive formulation to the Principle of Virtual Displacements, he only needs to unify the statements scattered throughout the treatise of Wallis and to reformulate them on an infinitesimal basis. Thus the principle of Wallis and that of Descartes differ only slightly. There is an even less perceptible difference between the formulation of Jean Bernoulli and that of Wallis. And yet, thirty-two years pass between the letter of Descartes to Constantin Huygens and the statics of Wallis, and forty-eight years separate the publication of the latter's statics and the letter which Bernoulli will write to Varignon. So slow and laborious is the progress towards truth in the human sciences!

## 4. THE GREAT TREATISES OF STATICS FROM THE JESUIT SCHOOL — F. DECHALES (1621—1678) AND F. PAOLO CASATI (1617—1707)

Written in accordance with an excessively complicated academic logic and limited to the study of extremely simple machines, the treatise of Wallis was not able to fulfill the needs of most physicists and artisans.

The treatises, Father Pardies wrote[75] in 1637, which have been published on the laws of motion, on the resistance of bodies, on the force of percussion, on the equilibrium of liquids, on the hardness, on weight, and many other matters, are certainly works which testify to their author's subtlety and to the refinement of the times. However, one cannot consider them taken together a complete science of mechanics. They represent elegant parts of it, but they do not form a whole, since they are the products of diverse authors of diverse views who did not collaborate with the same goal in mind and who even worked from diverse principles.

I had always hoped that this lengthy work of M. Wallis, which we awaited for so long, would include everything ever wished for about this topic.

I was almost certain that this had happened when I first saw the three large volumes in quarto with the titles *Mechanics and the Science of Motion*. However, I discovered that this excellent and admirable work is much better suited to satisfy those who are already experts in this science, than to instruct those wishing to learn it. Besides the fact that it is far from being inclusive it is written in such a scholarly and mathematical style that very few people are capable of understanding it.

At the time Father Pardies wrote these lines, the desire for a treatise on mechanics which would be both simple and comprehensive was so widespread and so strong that it aroused the support of Louis XIV and Colbert. In 1675, they called upon the Académie des Sciences and urged them to find a solution to this problem.[76]

The King wished for the Académie to start working without delay on a treatise on mechanics in which theory and application would be explained in a clear fashion, readily comprehensible by everybody. However, one was supposed to separate from theory anything too closely connected with physics and anything which might give cause for argument was to be included in some kind of introduction to the entire work. The work itself was supposed to include the machines used in the practical arts in France as well as abroad.

That is the message which M. Colbert had conveyed through M. Perrault to the Académie on June 19 of the same year. During several meetings, the Académie discussed the request and M. DuHamel was charged with giving M. Colbert an account of the reflections of each of the members. Messers. Picard, Huygens, Mariotte and Blondel worked together on the preliminaries. Messers. de Roberval and Römer also dealt with the same subject individually. M. Buot was charged with drawing up the catalogue of machines and to have drawings of them made. As assistants he was given M. Couplet, and Messers. Pasquier and Du Vivier.

As far as I know, the work requested of the Académie never appeared, but treatises on mechanics written by individual members were published in ever greater number.

Unfortunately, these treatises were not only quite numerous, but often also quite mediocre. The authors can be divided into two groups. The first group, obsessed with including everything and unconcerned with unity, accumulated — pell mell — and uncritically everything ever written on statics. The second group, on the contrary, given to a malicious and punctilious criticism rejected even the most undubitable truths and the most fecund principles.

*The Course or Mathematical Universe*[77] of Father Claude François Milliet Dechales or De Challes has, to be sure, an imposing and antique aspect. Its deductions and discussions are carried out in the slow, stringent and rigorous manner of the Scholastics.

In these discussions, written in a Peripatetic form, one continually senses the influence of very ancient authors. Besides references to the *Synopsis of* Mersenne,[78] there are constant allusions to the treatise of the Precursor of Leonardo da Vinci and to the *Jordani Opusculum de ponderositate*, edited by Curtius Trojanus. At one point,[79] the Jesuit scholar rejects the opinion of that author as concerns the influence exerted by the environment upon the motion of projectiles. At another point,[80] he borrows from him the demonstration of the law of the lever and certain propositions[81] concerning the balance.

It is true that Father Dechales has a rather strange way of modernizing his borrowings from the Ancient mechanicians. He has no qualms about attributing what he takes from them to certain contemporaries who are either colleagues or friends.

Thus the law of the lever which Stevin and Galileo borrowed from an argument known since the XIIIth century is presented[82] by Father Dechales as deriving from Father Léotaud (1595—1672), a fellow member of the Society of Jesus. The reduction of the problem of the inclined plane to the problem of the lever which Galileo had accomplished in his early works, which Roberval had used and which Descartes repeated again in the opposite direction, derives,[83] according to Father Dechales "from my friend M. Reynaud, a man well-versed in mathematics."

The principle of statics adopted by Father Dechales is exactly the same one postulated by Aristotle in his *Mechanical Problems*. However, in the course of its development, this principle is gradually transformed just as it was transformed in the writings of Galileo.

In order to assess the mechanical effect of a weight, its quantity of motion must be known.

This quantity of motion can be obtained[84] by multiplying the number of parts of the weight by the velocity, and since we neither know nor can measure the velocity other than by the space traversed in a given time, in order to know the quantity of motion, we must multiply the number of parts of the weight by the space traversed . . .

If, in a machine, two weights oppose each other:

in such a manner so that there is the same quantity of motion in each of them there is equilibrium.

Thus two moving bodies are equal in force,[85] when their magnitudes are in inverse proportion to their velocities.

Thus no machine can increase the force of their potential.[85]

If the forces of the power can be applied to a larger weight,[87] it is because the quantity of motion is diminished in the weight or increased in the force. Thus the more the forces of the power are increased by the machine, the more the ratio between the motion of the power and the motion of the weight.[88]

The principle so stated is faulty, unless it is modified. It is not the velocity in itself of a weight which must be included in the determination of the resistance of the weight, but only the vertical component of the velocity. The most simple observation calls for such a correction. For example, the fact that the same force normal to the same lever supports a lighter weight when the lever is horizontal than when it is oblique.[89] It seems[90] that our author became aware of the correction required by the Axiom of Aristotle while studying above all the first deduction in which that axiom was ever used, i.e., the demonstration of the law of the lever given by Jordanus de Nemore. Moreover, Father Dechales also adopts this demonstration for his theory of the balance.[91]

Moreover, Father Dechales remains faithful to Peripatetic dynamics in considering the velocity of the ascent or descent of a heavy body and not the distance it moves upwards or downwards. Thus his theory of the inclined plane is that of Galileo,[92] not that of Descartes.

This theory begins with a curious proposition,[93] which is difficult to reconcile with those which follow. Father Dechales attempts to explain why a sphere rolls more slowly, the less the plane is inclined. He thinks he has found the reason in the counterweight formed by a part of the sphere. His reasoning calls to mind the deductions of Pappus and, even more, those of Leonardo da Vinci and Bernardino Baldi.

The method by which he treats[94] the composition of concurrent forces also very closely resembles the method employed for a time by Leonardo. Dechales assumes that two concurrent ropes support a weight and he sets out to calculate the tension in each of them. To do this, he replaces one rope, for which he is not calculating the tension, by a rigid rod capable of rotating around one of its endpoints. The solution to the problem is then immediate.

Just as Guido Ubaldo, Villalpand and Mersenne, our author claims[95] that "the center of gravity for any body cannot ascend, unless violently." He applies this principle to the very same example that Mersenne cites and which came from Leonardo.

He justifies this principle by means of reasonings similar to those used by Villalpand, without however, referring to the attraction of the center of gravity towards the common center of heavy bodies.

Although it had been clearly refuted for over half a century, such an attraction does not seem absurd to him, as the following passage shows:[96]

*In every body there exists a definite center of gravity* . . . Father Léotaud attempted to prove this proposition by taking as his point of departure the following view accepted by all Peripatetics: The center of the Universe, or if one wishes, the center of the earth — it doesn't matter which one — is the center of all heavy bodies. They are all drawn to it by their weight and they remain at rest there. Demonstration: every heavy body tends with all of its effort towards the center of the Universe so that if all obstacles were removed, it would move towards this center and then remain there. But it could never remain at rest if it were not for a certain point or center of gravity within the body so that the body stops moving, once this point coincides with the center of the Universe . . . This demonstration is acceptable but . . . we shall see whether it is possible to say something even more convincing.

Father Dechales, does indeed have some doubts about the properties which the Ancients attributed to the center of the Universe. He thinks[97] that heavy bodies might very well attempt in their descent to join not the center of the earth itself, but an interior nucleus, which, in itself, is devoid of any weight. How naive and old-fashioned this hypothesis seems to us, when we recall that at the time it is stated by our author, Newton had already laid the foundations for the system of universal gravitation!

The same tone of old-fashioned naivete is prevalent in everything Father Dechales wrote about statics. No recent discoveries, no new ideas find their way into his system. Although everything he writes reeks of old age, he at least is able to preserve what is valuable in the older traditions. The powerful thoughts expressed by Descartes and Wallis on the Method of Virtual Displacements remained for him a dead letter. He only took from this approach what Galileo had written about it. A heavy body is in equilibrium when the center of gravity is at its lowest possible point. He fails to give this principle the precise form which Torricelli and Pascal had given it. He merely states it in the same fashion as Cardan, Villalpand and Mersenne. The extreme respect of our author for tradition renders him almost imcapable of accepting new truths, but makes him the jealous guardian of ancient truths.

If there is a place where respect for tradition is always present, it surely is within a tightly knit religious order. And Father Dechales is a Jesuit and his work can be included among the long series of works by which the Society of Jesus attempted throughout the XVIIth century to give logical order to statics.

The treatises of Father Zucchi and Father Honoré Fabri mark the beginning of that attempt. Their treatises as well as the teachings of their authors, whether at the Collegium Romanum or at the Collège run by the Society of Jesus in Lyon, were highly influential on all of the works of Jesuits on statics.

Father Zucchi and Father Fabri took as the fundamental principle of statics the Principle of Virtual Velocities in the form which Galileo had given it. In their eyes, this form offered a unique advantage. It allowed them to meld the laws discovered by modern mechanicians with the principle of Peripatetic mechanics. We know how much the Jesuits of the XVIth and XVIIth centuries valued a synthesis capable of carefully maintaining the major principles of the physics of Aristotle while enriching them with the discoveries of the new science.

Father Dechales also shared the desire to be both a loyal Peripatetic and a mechanician, well informed on the science of his day. That desire had inspired him to base his statics on the principle adopted by Father Zucchi and Father Honoré Fabri. Father Paolo Casati was also inspired by the same desire and took up the same cause.

Father Paolo Casati of Plaisance (1617—1707) began writing on mechanics in 1655 in a curious work entitled: *Terra machinis mota*.[98] A second, more complete edition of this work was published in 1658.[99]

In this work, three interlocutors called by Father Casati Galileo, Mersenne and Guldin comment upon the well known statement of Archimedes:[100]

Give me a place to stand and I shall move the earth.

They attempt to prove that this sentence was far from being arrogant boasting.

Stevin had already expressed a similar opinion. Furthermore, the influence of Stevin is evident in this strange dialogue written by Father Casati where the windlass is called a *pancratium*, the exact same name proposed by Stevin when he discusses the proposition attributed to Archimedes.

There exists yet another influence in several passages of the *Terra machinis mota* which we could delineate if we had the time. It is the influence of Leonardo da Vinci. It is true that the doctrines on mechanics which the Jesuits taught in their colleges contained numerous borrowings from the notes of the great painter. Our analysis of the *Cursus mathematicus* of Father Dechales has already revealed some of these borrowings. We could point out many more in the *Terra machinis mota* concerning certain theories on hydrostatics, and others will become apparent later on.

The dialogues entitled *Terra machinis mota* have hardly any importance for the organization of the principles of statics. It is in another book that Father Casati worked on such a synthesis. This new book was not printed until 1684,[101] but in the preface to the reader we learn from the author that as early as 1655 he had distributed to his students at the Collegium Romanum a handwritten summary. Thus the work of Father Casati appears to antedate the one by Father Dechales. Moreover, there are many similarities between the two works. They not only derive from the same mentality, but they often make use of the same demonstrations.

The first book[102] of the work by Casati deals with the center of gravity and is to a large degree borrowed from Bernardino Baldi, Villalpand and Mersenne, that is to say in the final analysis, from Leonardo da Vinci. Furthermore, it sometimes seems that Father Casati is submitting to the direct influence of Leonardo in his *Mecanicorum libri* as well as in his earlier works. He seems to have borrowed almost literally from the notes of the great painter[103] a pulley arrangement which facilitates the tolling of a heavy bell.[104] His study of the posture of animals copied from those who were inspired by Leonardo[105] gives the author an opportunity to formulate the law of the polygon of sustentation. It even appears that Father Casati was the first mechanician ever to use the term.

It is in this same book that the author deals[106] with the apparent weight of a heavy body placed upon an inclined plane. In order to determine the apparent weight he uses almost exactly the same reasoning as Father Honoré Fabri:

> . . . the weight of a body on an inclined plane is to its weight in the vertical plane as the resistance which it experiences in order to move upwards in one of those planes, is to the resistance which it experiences to move upwards in the other one; but these resistances are to each other as the violences experienced by the body during these displacements.

And these violent actions are inversely proportional to the distances which the body must traverse in these two planes in order to ascend by an equal amount.

Moreover, Casati distinguishes between the apparent weight of a body placed upon an inclined plane (*gravitatio in plano inclinato*) and the pressure which it exerts upon the plane (*gravitatio in planum inclinatum*). The analysis done by Stevin would have allowed him to determine precisely this latter force, but Casati does not resort to that analysis. He repeats an error committed by Descartes when he formulates [107] the following proposition:

From the preceding chapter, we know the force of the weight of a body placed upon an inclined plane. The difference between the weight of the body in the vertical plane and the weight of the same body placed on an inclined plane measures the resistance to motion of the body by the adjacent plane. Thus it also measures the pressure exerted by the body on the plane.

The calculation of the moment of a weight attached to one extremity of a lever arm with the other being free to rotate about the point of support can be reduced [108] to the problem of the inclined plane. This moment is equal to the apparent weight which the same body would have when placed on a plane normal to the arm of a lever. This artifice which allows one to move from one problem to another is identical to the one which Descartes had used and which Galileo and Roberval had used in an inverted fashion.

Once this problem is solved, Casati [109] goes on to determine the tensions in two ropes which sustain a weight. He obtains it by using exactly the same method as Dechales.

The solutions to the different questions on statics analysed in Book 1 were deduced from the postulates concerning the properties of the center of gravity. These postulates were not reduced to the general laws on motion. In his second book,[110] Casati attempts to deduce from the principles of dynamics the theory of diverse machines.

The principles of dynamics used by our author closely resemble those formulated by Father Fabri. They are founded [111] entirely upon the consideration of an *impetus* proportional to the product of the weight of a body set in motion and the velocity of that motion.

This notion plays an essential role in the formulation of the principle which provides the basis for all machines. Casati borrowed this statement almost word for word from Fabri:[112]

The central stratagem of mechanics consists in positioning its instruments in such a way and placing the force and the load at such points that the force moves faster than the load. If one takes into account the relation between their displacements, one will be able to determine the force required to move a given load or the load a given force will move. To make the motion possible, it is necessary that the ratio between the force and the weight of the load exceed the ratio between the displacement of the load and the displacement of the force. The machine does not increase the capacity of the force nor does it diminish the weight of the load. It merely accommodates the resistance of the weight to the capacity of the force.

This law has a physical cause. To move a load equal to the force at the same velocity as that force, the *impetus* produced by the force would have too great an intensity. It has a lesser intensity when a heavier load is moved more slowly, but this lesser intensity is sufficient because of the smaller resistance . . .

One can thus see that a kind of justice always rules between the capacity of the force, the weight of the load, the distances traversed during the motion and the duration of these motions. Wherever the capacity of the force decreases, or the weight of the load increases, the distances traversed by the load become shorter and the duration of these paths longer. On the other hand, the distances traversed by the force become larger, because this weaker force must move more rapidly than the load. Thus, if one wishes to lift a heavier load, one must increase the force or, if one wishes to maintain the same force, one must either decrease the displacement of the load, or increase the displacement of the force. With a small force, it is impossible to move a large weight rapidly.

These various passages expound the statics of Aristotle, not of Galileo. However, Father Casati is aware of the modification which the study of the inclined plane had forced the geometer of Pisa to make of the Peripatetic principle. We have seen him restate an accurate solution to this problem of the inclined plane. Moreover, in all of his calculations he does not introduce the actual velocity of the weight put into motion, but the projection of this velocity on the vertical.

The mechanicians of the Jesuit School — Fathers Zucchi, Honoré Fabri, Dechales and Casati — were certainly well-acquainted with the work of Descartes. Nevertheless, they did not adopt the method which the great philosopher wished to be applied in statics. It is easy to understand why they refused to adopt this method. Its main goal was to bring about the definite break between the recently established statics and the basic law of Peripatetic dynamics. The clear intention of the Jesuit geometers consisted, on the contrary, of fusing the modern science of equilibrium with the principle of Aristotelian mechanics. How could they avoid being attracted to the method of Galileo which derived so directly from the axioms postulated in the *Physics*, in the *On the Heavens*, and in the *Mechanical Problems*, but, which, in practice, yielded exactly the same corollaries as the Cartesian method, and by using the same calculations?

Although they misunderstood the notion of *work*, whose nature and importance had become ever more apparent from Jordanus to Descartes, nonetheless, they preserved in its entirety the Method of Virtual Velocities, which originated in the *Physics* of Aristotle and had been transformed by Galileo under the influence of the discoveries made by the School of Jordanus. Thus the Jesuit school of mechanics was able to safeguard a great part of the fertile ideas produced by the venerable Science of Weights.

## 5. THE REACTION AGAINST THE METHODS OF VIRTUAL VELOCITIES AND VIRTUAL WORK: JACQUES ROHAULT (1620–1675), F. PARDIES (1636–1673), THE TREATISES OF F. LAMY, THE *DE MOTU ANIMALIUM* OF BORELLI

We now shall see how the truths of the Ancients will be misunderstood and brutally expelled from the field of statics. We have already witnessed the violent rejection on the part of Guido Ubaldo, Benedetti and Stevin during the XVIth century of the rich ideas which were to develop from the teachings of the School of Jordanus. This same rejection reoccurs at the end of the XVIIth century and is as radical in expelling ideas as was the XVIth century. However, it is much less justified because the School of Descartes is now the enemy in place of the School of Jordanus.

No one was more fanatical than Jacques Rohault in rejecting any demonstration which used the Method of Virtual Displacements or any comparison between the work of the motor force and the work of the resisting force. One would have to go as far back as Benedetti to find an author who refused so stringently any consideration of this nature.

Rohault was a student and friend of Cyrano de Bergerac and had induced him to break with the system of Gassendi and to adopt the Cartesian cosmology. In Cyrano's papers, Rohault had found a plan[113] for various chapters for a treatise on physics. Based on that plan, he wrote and published a complete treatise,[114] which enjoyed great success and remained a classic until the middle of the XVIIIth century.

During his lifetime, Rohault published nothing related to statics, but he lectured on it in his very popular courses, which he taught in a clear and elegant fashion by making use of cleverly devised experimental demonstrations.[115]

The public lectures which he gave once a week were attended by persons of every rank and condition — prelates, abbots, doctors, courtesans, physicists, philosophers, geometers, students, regents, provincials, foreigners, artisans, in a word, persons of every age, sex and profession. During his lectures he delivered almost as many oracles as he gave answers to problems which had been submitted to him by all sorts of persons. In this way, he achieved such a great reputation that we know of many people who left their homeland and undertook long voyages in order to hear him speak. Some did so out of curiosity, while others were moved by jealousy for they wanted to criticize and combat his teachings.

Through these lectures the method by which Rohault taught statics was soon well-known. The influence of this method is evident in works which appeared several years before the publication of the method itself.

We have this method today in the *Oeuvres posthumes* of Rohault which his father-in-law, Clerselier, published in 1682.[116]

We have said before that one could look in vain in Rohault for a reference to the Method of Virtual Displacements either in the modified form which developed from Aristotle to Galileo, or in the form which developed between Jordanus and Descartes and Wallis. Furthermore, no mention is to be found of the principle of the center of gravity which had been so accurately formulated by Torricelli and Pascal. Nor is there any trace of the postulate on the impossibility of perpetual motion so skillfully used by Stevin. The law of the lever, established by the method which Stevin and Galileo undoubtedly took from the Middle Ages if not from Antiquity, was the sole source from which derived all of the laws of "mechanics." The logical order of the presentation, the rigor and clarity of the deductions, cannot hide its underlying arid sterility, devoid of any possibility of bearing fruit.

However, the same author who so completely ignored the ideas of Descartes on "mechanics" was, as far as physics is concerned, a fervent Cartesian. It was he who wrote the following in the *Preface* to his *Treatise on Physics:*

The man who contributed the most to the composition of this work and whose name cannot be found anywhere in it because it would have been repeated too many times, is none other than the famous Descartes, whose merit has been increasingly recognized in many of the most important states and who will cause everyone to admit that France is at least as fortunate as Ancient Greece in producing and nourishing great men in all professions.

He goes further. In his *Treatise on Physics*, Jacques Rohault defined[117] the notion of quantity of motion and uses almost the same words which Dechales was to adopt just a few years later to show how the equality of the quantities of motion produced the equilibrium between the motor force and the resisting force:

Motion has always been considered a quantity which on the one hand can be calculated by the length of the line which the moving body traverses . . . and on the other hand, can be calculated by how much or how little matter is moved at one time. . . . From this, it follows that for two unequal bodies to have equal quantities of motion, the paths which they traverse must be to each other in inverse ratio to their masses. If a body is three times larger than another, the path which it traverses can only be one third of the path traversed by the other body.

When two bodies which are attached at the extremities of a balance or a lever are to each other in inverse proportion to their distances from the fixed point, they must while in motion, describe paths which are to each other in reciprocal proportion to their mass. . . . Thus we must assume that they will be in perfect equilibrium. This result could serve as a foundation for mechanics . . .

Why did Rohault, when he wrote his *Treatise on Mechanics,* give it a completely different basis and not even mention other possibilities such as the one above? We can't answer this question. The fact is that his treatise, as it was, was consistent with the fashion of the times.

The most fervent Cartesians, like Rohault for example, had passed over in silence the principle on which Descartes wished to found statics. The opponents of the great philosopher went even further, and openly fought this and other similar principles.

Father Ignatius Gaston Pardies of the Society of Jesus was an impassioned opponent of Descartes. In his *Discours de la Connaissance de Bêtes.*[118] published in Paris by Mabre-Cramoisy in 1672, he fought against the automatism which the great philosopher attributed to animals. In his *Discours du movement local,*[119] published by the same editor, first in 1670, then in 1673, he rejects the principles of Cartesian dynamics. Thus it is not surprising to see him reject the foundations upon which Descartes meant to build statics. The *Statique*[120] of Father Pardies is a rather unoriginal book, even though it had some success at the time. The beginning is borrowed almost word for word from Villalpand. The law of the lever which is so pompously introduced in the following words:

Here now is the most important proposition of statics, is established through the demonstration adopted by Galileo and Stevin and taken up again by Rohault and Dechales. In the passage containing this reasoning, Pardies sounds, incidentally, as if he were presenting a new invention:[121]

Those who know what the interpreters and commentators of Archimedes say about this subject, will realize that the demonstration which I have furnished is devoid of any of the shortcomings which are usually associated with this demonstration.

The equilibrium of the bent lever is treated[122] in a form which recalls the reasonings used by Benedetti. All simple machines, such as pulleys, inclined planes, the assemblage of two ropes to sustain a weight, are reduced to the straight or bent lever. The tensions of these ropes are determined by the same artifice used by Dechales and Casati.[123]

Incidentally, Father Pardies writes:[124]

In the case of all of these moving forces one can observe that the perpendicular displacement of the weights while moving either upwards or downwards in the same interval of time is always reciprocally proportional to these same weights.

In support of this proposition, he cites the example of the lever and reproduces the figure which Dechales had copied in almost every detail from the treatise of Jordanus de Nemore.

However, Father Pardies is very careful not to make of this proposition the foundation of statics. He wants it to be founded on entirely different principles and this proposition to be reduced to the role of a corollary:[125]

Thus, he says, some people have labelled it a principle in order to account for all the moving forces. Furthermore, it seems quite obvious that it does not take more or less force to raise a one hundred pound weight one foot than to raise a one pound weight one hundred feet. Thus a one pound weight descending by one hundred feet will counterbalance a one hundred pound weight descending by one foot. This principle does not sufficiently satisfy our intellect so to provide clear demonstrations. It is, nonetheless, quite correct. And in view of the demonstrations which I have just made on moving forces, one can accept it without hesitation as being beyond doubt.

If Father Pardies refuses to follow Descartes and to make of the proposition of Jordanus the essential postulate of statics, he comprehends clearly, nevertheless, the connection of this proposition with the impossibility of perpetual motion. It is true that what he says[126] in order to show that "perpetual motion in mechanics is impossible" is but a clear and accurate commentary on what Cardan had written in the *De Subtilitate:*

From this we can see that those who search for the means to produce perpetual motion in statics are wasting their time. To produce such motion, certain bodies would necessarily have to descend and others ascend in such a fashion that the same bodies, once they had moved upwards would descend again and perpetuate motion in a continuous cycle. But it is obvious in these instances that whatever moves downwards must also move upwards. If the weight which must ascend is only equal to the weight which must descend over the same interval of time, it is impossible for the motion to occur by itself, since a weight cannot in this fashion overcome another equal weight. If the weight descending is larger than the weight ascending over the same interval of time, the velocity of the descending weight must of necessity be proportionately smaller so that the descending weight is to the ascending weight as the velocity of the ascending weight is to the velocity of the descending weight. Moreover, the cycle could not be perpetual so that either more bodies would rise than descend, or, on the contrary, more bodies would descend than rise. Thus the machine would soon stop. If the velocity of the descending body is to the velocity of the rising body in an inverse ratio to the weights of the individual bodies, there will be equilibrium and nothing will move.

The statics of Father Lamy, a priest of the Order of Orators, is hardly original.[127] Lamy's treatise calls to mind the treatise of Dechales perhaps even more than the treatise of Father Pardies did. Just as the former, it begins with the theorems of Villalpand and gives for the law of the lever the same demonstration used by Archimedes and Stevin.

However, the criticism of Father Lamy extends even further than that of Father Pardies. Neither the postulate of Aristotle or Galileo nor the postulate of Descartes seems to the punctilious Father a sufficient foundation for statics. They are merely corollaries to the laws of equilibrium and not at all their *raison d'être.*

What is gained in force by a lever, he says,[129] is lost in time and space.

He substantiates this remark by using the ancient method of Aristotle, i.e., by taking into account the actual length of the path traversed by each of the weights and not the projection of this path on the vertical. He continues:[129]

One need not look beyond what we have proposed for any further cause of the equilibrium of two bodies of different weights, suspended from a balance. As we have proven, it is obvious that such equilibrium occurs because the balance is being affected equally on both sides of the fulcrum. However, some people have assigned a different cause to this equilibrium, to wit, the law of nature which we have just demonstrated in the preceding proposition . . . Several reasons have kept me from sharing that view.

First of all, when considering two bodies in equilibrium, I cannot see how a displacement which they do not have and can only have when they are not at rest, can be the cause of that very rest . . .

There are machines in which this law of nature, i.e., whatever is gained in force is lost in time – is valid. However, we shall demonstrate geometrically that the force of those machines has a cause other than this law. Thus it is not an admissible conclusion that it is the cause of the force of the lever and that it is at the same time one of its effects . . . There is no need for me to mention[130] that the law according to which one loses in distance and time what one gains in force is not the cause of the force of pulleys, but rather a consequence of their configuration. These function as levers, as we have seen . . . Thus one need look no further for another cause for the effects of these machines.

According to Father Lamy, the axiom invoked so often since Aristotle and Galileo, does not merit its lofty status, but should be relegated to the more humble level of a corollary.

Our author does not show any more respect for the axiom of Jordanus and Descartes:[131]

Descartes proposes the following principle and claims that it is the cause of the equilibrium of the lever. It is the same thing, he says, to lift a one hundred pound load to a height of ten feet as to lift a ten pound load to a height of one hundred feet. . . . This seems to me to contain a fallacy because this principle can only be true if one can lift separately the parts of the total load. For example, no more force is needed to lift ten stones separately to a height of one foot, than to lift one of those stones to a height of ten feet. If I can lift one stone those ten feet, I will certainly be able to lift all of them to a height of one foot. However, it is evident, that this cannot be done unless I take the stones one by one, because even though I can lift a load of one pound to a height of one thousand feet, I cannot lift a weight of one thousand pounds to the height of the one thousandth part of a foot.

Father Lamy repeats Stevin's objections to the axiom of Aristotle. These objections lose their validity as soon as one recalls that the Method of Virtual Velocities is a method of demonstrating *per absurdum*. He makes the same criticism of the axiom of Descartes that Mersenne had made before him. The confusion of force and work which was due to an inaccurate terminology explains their criticism. Nor does the axiom of Stevin, deduced from the impossibility of perpetual motion, find any favor with the harshly critical and punctilious priest. While discussing the theory of the inclined plane, he finds the occasion to attack that axiom.

The major preoccupation of Lamy in his theory of the inclined plane is to determine the fraction of the total weight of the body supported by the plane, since:[132]

A heavy body exerts only a part of its weight on the plane upon which it rests, when that plane is inclined.

That fraction is what we call today the component of the weight perpendicular to the plane. The "arithmetical" excess[133] of the entire weight to the component upon this plane is according to the expression of Lamy that which is " borne by the air."[134] Lamy seems here to be submitting to the unfortunate influence of Father Casati.

Furthermore, when Lamy attempts to calculate the part of the weight supported by the inclined plane, he makes use of a rather bizarre demonstration clearly copied from Leonardo da Vinci and Bernardino Baldi. He assumes that the body supported by the inclined plane has the shape of a sphere (Fig. 107) and he claims that[135]

the inclined plane does not bear the total weight of X, but . . . only bears that portion of the weight someone would feel while supporting lever LG at point E. Therefore, the rest is borne by the air.

This reasoning furnishes Lamy with the following erroneous theorem:[136]

When a body is placed on an inclined plane, the part of the weight of that body which bears on this plane is to that part which does not bear on it as the length of the plane is to its height.

Although Lamy continues to apply equally strange reasonings, he is more fortunate in the following proposition:[137]

When a sphere is pulled over a plane along a line parallel to the maximum inclination of this plane, the portion of this sphere bearing on the plane is to the "portion which does not bear" on it as the inclination[138] of the plane is to its height.

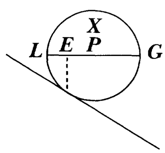


*fig. 107.*

In this formulation, "the portion the plane does not bear," means the component of the weight of the body parallel to the inclined plane.

This theorem leads our author to another one[139] which is also accurate[140]

When two heavy bodies rest on two planes of equal height, if the part borne by one of the two planes is to the part borne by the other as the inclination (i.e., length) of one plane is to the other, then these two bodies will be in equilibrium.

This proposition and the theory of the inclined plane as formulated by Stevin are in perfect agreement. Perhaps motivated by a need to criticize the great geometer of Brugge, Lamy modifies his own theorem in order to bring it into disagreement with classical teachings on the inclined plane.

Although it is commonly believed, he says,[141] that when the actual weights of the two heavy bodies arranged on the two planes, as in the figure accompanying the preceding proposition, are to each other as the planes upon which they rest, they must be in equilibrium, that is not what we just observed. It is not necessary that the actual weights be to each other as the [lengths of these] planes, but the portion of those weights which bear on those planes. I have seen an author use this so-called demonstration, which I reject . . .

After having given the demonstration by Stevin, Lamy adds:[142]

But since the demonstration assumes the impossibility of perpetual motion, which has not been demonstrated, it is no good. Furthermore, he has not noticed that the spheres E, F, and G, (Fig. 108) cannot descend and cause the spheres O, and N to rise since they incline more on plane AC than on plane AB.

The criticism has no basis. The string of spheres forms a perfectly symmetrical wreath which inclines equally on side AB and side AC.

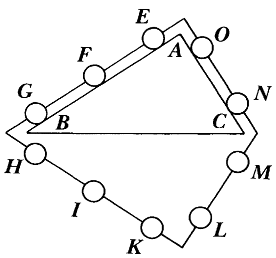


*fig. 108.*

However, we have to admit that Stevin, usually so unsparing in taking useless precautions for his lengthy demonstrations, would have been well-advised to state this explicitly and include some justification for such a statement.

The *Statics* of Father Pardies as well as the *Treatise on Mechanics* of Father Lamy are very mediocre works. Like those of Rohault and Dechales, these two works are testimony to the miserable state of the science of equilibrium around the year 1680. The same impression is made by another work,[143] written during the same period, although its author is none other than the renowned Giovanni Borelli. The numerous editions of this work testify to its great popularity.

The study of the forces exerted by the muscles determining the movements of animals, forces Borelli to study the tensions in ropes which impede a resistance. An entire chapter[144] is dedicated to these lemmas on the Composition of Forces. The methods by which the demonstration of these propositions is shown to be based on the properties of the lever are quite artificial. They are only clever devices but their results are hardly convincing.

These results are, of course, those which had been known since Stevin. However, Borelli finds it legitimate to criticize the demonstrations of Stevin and Hérigone,[145] whom he mentions by name as well as the reasonings of a certain "insignis Geometra neotericus"[146] whom he does not name, but whose device is the very one employed by Dechales, Casati and Pardies. Borelli goes even further. He thinks he has discovered an error in the statements by Stevin and Hérigone. He agrees with them that two oblique and concurrent forces transmitted by two ropes will hold a weight in equilibrium if each of the tensions is to that weight as the side of the parallelogram of forces is to the diagonal of that quadrilateral. But, he claims that the converse of this theorem is not correct. Varignon[147] will have no difficulty in proving to him, by using Borelli's own lemmas, that he is quite wrong.

Furthermore, Borelli refuses to allude to any of the general principals of statics, or to the Principle of Virtual Velocities constantly used from Aristotle to Galileo, or to the Principle of Virtual Displacements which Jordanus, Descartes and Wallis had continued to expand upon and refine. For Borelli as well as for Rohault, Pardies and Lamy, the law of the lever is "the most important proposition in statics." All other propositions can be reduced to it. The narrow-mindedness of these authors resembles that of Guido Ubaldo.

It is clear, in fact, that around the year 1680 most geometers had a very superficial knowledge of statics. The great and fecund principles to which this science owes its most marvelous discoveries are not only either unknown, relegated to the status of mere corollaries, passed over in silence or considered incorrect, but some of the most indisputable theorems are questioned or are simply misunderstood. Among such theorems is the Law of the Composition of Concurrent Forces. All of a sudden this law is no longer considered as one of the many theorems of statics, but as the most fundamental proposition from which the entire science can be derived and as the sole principle which enables the geometer to see with absolute clarity and certainty the reason for the most diverse cases of equilibrium.

## 6. THE PARALLELOGRAM OF FORCES AND DYNAMICS. THE *OBSERVATIONS* OF ROBERVAL. VARIGNON (1654–1722) – THE *LETTER* OF F. LAMY. THE *PRINCIPIA* OF NEWTON – THE *NEO-STATICS* OF F. SACCHERI

In spite of the quite unjustified criticism of Borelli, the Law of Composition of Forces will soon appear to mechanicians as a principle which can unravel all of the questions of statics. From now on, the stature of this principle will require that it be made independent of all other laws relative to equilibrium and that it be disassociated from any consideration of the lever or the inclined plane from which it had been derived up till then. It must now be arrived at directly from the fundamental laws of motion.

The Law of the Composition of Forces will find its direct justification through the principles of dynamics by returning to its very origins — the reasonings in the *Mechanical Problems.*

Aristotle, or whoever might have been the author of the *Mechanical Problems*, was quite familiar with the Law of the Composition of Velocities. We have said before[148] that in the eyes of this author, to know the Law of the Composition of Velocities meant knowing the Law of the Composition of Forces, because by virtue of the fundamental axiom of Peripatetic dynamics, a constant force produces a uniform motion with a velocity proportional to the force which produced it. Thus it can be asserted that the Law of the Composition of Forces had been known since Antiquity. If modern authors, such as

Leonardo da Vinci, Stevin and Roberval devoted so much time to the demonstration of this law, it is because they wanted reasons purely within the domain of statics and proofs which were not based on the ratio between the moving force and the velocity of the moving force. The reason for these efforts is very clear to Stevin, who considered Peripatetic dynamics as useless, without, however, knowing what form of dynamics should replace it.

We have seen that Descartes, like Stevin, thought that the dynamics of the Ancients must be redone and that a new dynamics had not been attained. Consequently,it was important to at least temporarily base the science of equilibrium upon autonomous postulates, upon axioms which would not rely for their certainty upon the laws of motion.

Roberval also harbored some doubt concerning the Peripatetic principle, which asserts a proportionality between force and velocity. This is clear from the following passage in the unpublished *Treatise on Mechanics.*[149]

> And although the force or impression increases and, consequently, the velocity as well, one should not conclude that this velocity increases proportionately. For example, one should not conclude that a two-fold increase in force or impression will cause a two-fold increase in velocity in the body, even though all other conditions remain the same. On the contrary, in order to produce a two-fold increase in velocity, one often needs more than a two-fold increase in force without, however, knowing the increase of one in proportion to the other, which is very difficult to ascertain.

The doubts expressed in this passage constitute, unfortunately, an isolated occurence within the work of our geometer. Everywhere else Roberval reasons like a Peripatetic.

As we have seen[150] before, this author is the first to have published correct demonstrations in statics of the Composition of Forces. He gives two of them. The second, deduced from the axiom which Descartes was to formulate in its general form, is quite elegant. And although he supported the idea that the Law of the Parallelogram of Forces should be justified by methods solely within statics, and although he helped assure the success of this idea, he did not consider it necessary to abandon the Aristotelian approach to problems.

When Roberval died in 1675, he left in manuscript his *Observations sur la composition des mouvemens, et sur le moyen de trouver les touchantes des lignes courbes,*[151] which is one of his claims to glory as a geometer. Mechanics is hardly treated at all in this work and when it is, it has a distinct Peripatetic form.

Power, says Roberval[152] is a moving force; Impression is the action of that power. The line of direction of the power is that in which the power moves the body . . . We have also defined power insofar as it can help us consider the different types of motion. However, this does not prevent us in other speculations from understanding by the word power a force capable of supporting a weight or any other effect.

A little further on, Roberval considers:[153]

two kinds of forces within bodies capable of making them move. The first one propels them violently from one place to another. A racket, for example, imparts such a force to a ball, or a bowstring to an arrow, etc. The second one occurs through attractions between bodies, whether this attraction is reciprocal or not . . .

There is no doubt that Roberval does include weight, the virtue of the magnet,[154] and the other forces among the "powers" whose "impressions" he is analyzing.

Generally speaking,[155] we shall consider in this treatise two things concerning motion, direction and velocity.

It is clear from the definition that our geometer has given to the expression "line of direction" that the direction of motion coincides with the line of direction of the force which produces it. This is also indisputably clear from propositions such as the following:[156]

The direction of a power moving a body which in its motion describes the circumference of a circle, is the perpendicular to the extremity of the diameter where the moving body is located.

The proposition conforms so closely to Peripatetic dynamics that it is evident that Roberval has accepted its basic axiom. Namely, proportionality between the "impression" of a "power" and the velocity of the uniform motion produced by it. Despite the doubt expressed in his *Treatise on Mechanics*, this axiom seems so self-evident to the Professor of the Collège de France that it never enters his mind to require that it be accepted. He invokes it in the clearest fashion precisely when he needs to identify the problem of the composition of forces with the problem of the composition of velocities.

We can now consider[157] a motion to be composed of several motions when the moving body representing the motion is moved by several impressions . . .

But we shall observe[158] that in this first composition as well as, in general, in all other motions, we can consider six factors. Namely, three directions, of which two are simple and one composite, and three impressions, of which two are simple and one composite.

When these three directions are given, the three impressions are also given, that is to say, the proportions of the velocities of the three motions.

Thus, in his *Observations sur la composition des mouvemens,*[159] Roberval applies the law of the Composition of Forces to dynamics, to be sure, but to Peripatetic dynamics. His work is a logical continuation of the *Mechanical Problems* and the *Causes* of Charistion.

To the *Observations sur la composition des mouvemens* the *Projet d'un livre de Méchanique traitant des mouvemens composez* is added.[160] Although we only possess two pages from the preface of the former work, it was most certainly written in the same Peripatetic spirit as the *Observations.*

The *Observations* of Roberval were only printed once, long after the death of their author, in 1693. However, the doctrine on composite motion contained in the *Observations* and the method of "drawing the tangents to the curved lines" which was taken from it, must have been known much earlier, either as oral tradition deriving from the lectures of Roberval at the Collège de France or in manuscript form. The ideas contained in this work seem to have exerted a profound influence on the research of Varignon.[161]

As soon as Varignon had discovered that composite motion provided a simple explanation for the use of forces in machines and yielded the exact ratio of those forces no matter what direction they took, his method had an advantage over all previous methods and he bagan to apply it to simple machines. In 1685, in the *Histoire de la Republique des Lettres,*[162] he wrote a paper on the block and tackle in which he used composite motion to answer all questions on this kind of machine.[163]

In 1687, Varignon became known to the public through his *Projet d'une nouvelle Méchanique*[164] dedicated to the Académie des Sciences. He continued throughout his life to work on his treatise on statics of which the *Projet* had been the blueprint. However, this treatise[165] was not published until three years after his death, through the efforts of Beaufort and Father Camus.

The *Project for a New Mechanics* begins with a preface in which Varignon familiarizes the reader with the various stages by which his mind had reached a clear understanding of the laws of equilibrium. The author undoubtedly thinks that by confiding in us, he will gain our admiration for the originality of his insights and the unique depth of his reflections. However, he is not completely successful because we soon recognize in his reflections a train of thought routinely encountered in every treatise on mechanics written around this time. Thus what strikes us in this geometer's work is not so much the strength and novelty of its thought but rather the clarity and fidelity with which it reflects the ideas of its contemporaries.[166]

At the beginning of the second volume of the *Letters* of M. Descartes, says Varignon, I came across a passage in Number 24 in which he states that it is ridiculous to want to use the concept of the lever for the pulley. This reflection led me to another one: namely, does it make more sense to imagine a lever as a weight set on an inclined plane rather than as a pulley. After having given it some thought it seemed to me that these two machines, being at least as simple as the lever, should not in any way depend on the lever, and that those who relate them to the lever do so because they are forced to it by their principles which are not general enough to demonstrate the independent characteristics of these machines . . .

This might have been what caused Messrs. Descartes and Wallis to take another approach. Whatever the case may be, they were successful because the approach they took also leads to the knowledge of the application of each of these machines, without compelling us to make them depend on each other. Moreover, it had led Wallis further in this matter than any other author I know of.

The comparison which I made of these two kinds of principles gave me the feeling that the principles of Archimedes were far from being as general and as convincing as those of Messrs. Descartes and Wallis. However, I felt that none of them enlightened me much. When I looked for the reason, the deficiencies of those principles seemed to stem from the fact that their authors were much more concerned with proving the necessity of equilibrium than with showing how it comes about.

This persuaded me to start spying on nature and, by following it step by step, to see if I could understand how it causes two powers, whether equal or unequal, to remain in equilibrium. Finally, I attempted to locate equilibrium at its very source or more precisely in its genesis.

Varignon then proceeds to give an example of this method, which allows him to discover the very genesis of equilibrium. He analyzes the equilibrium of a body placed on an inclined plane. He shows how the tension in the rope holding the body and the weight of its mass have a resultant which is precisely perpendicular to the plane. He says nothing in this matter which cannot be found in Stevin or which has not been repeated on many occasions by Mersenne, Hérigone, Wallis and by all who have written on the subject of statics.

Thus, after having found the way which equilibrium occurs on an inclined plane, I shall now set out in the same way to determine how weights supported by ropes alone or which are attached to pulleys or to levers maintain equilibrium between each other or between the forces which support them. I also noticed that all of this came about by means of composite motion. It occurred with such uniformity that I could not help but believe that this was the actual path which nature followed in the case of the congruence of the action of two weights or of two powers. Nature achieves this uniformity by causing the individual impressions — whatever the ratio — to unite in a single impression which discharges in its entirety at the point where the equilibrium arises. So that the physical explanation of the effects most admired in machines seemed to me precisely composite motion.

Such far-reaching insights surprised me and the self-evidence of the details of all of this, independent of their general application, further confirmed my opinion that one must go into the genesis of equilibrium to see its true nature and to recognize the characteristics which all other principles can only prove, at the very best, as necessary deductions.

How did Varignon arrive at the opinion "that all physical explanation of the effects most admired in machines is precisely composite motion?" There can be no doubt. He arrived at this opinion in the very same way as Roberval in his *Observations.* He was led to it by the principles of Peripatetic dynamics, which he seems never to have questioned in any of his works on statics.

Varignon not only does not question the fundamental axiom of Aristotle's dynamics, but he even formulates it explicitly[167] and makes it the principle axiom from which all of his deductions will be derived: *The distance, he says, traversed by the same body, or by equal bodies during equal times, are to each other as the forces which move them. And, inversely, when those distances are to each other as these forces, they cause the same body or equal bodies to traverse these distances in equal times.*

One might raise the objection that the similarity between the axiom of Aristotle and that of Varignon is only apparent, and that the proposition stated by Varignon would be in agreement with modern dynamics provided that the bodies under consideration start from a rest position, and that this qualification was undoubtedly obvious to Varignon, but he merely neglected to formulate it.

If the view which we stated were challenged by such doubts, one could easily confirm it by reading the beginning of the *New Mechanics.*

After having stated[168] that weight is a force, that "it ordinarily serves as the standard for calculating all other lesser known forces . . . so that one can call any given force . . . one pound, or three, etc.," Varignon states his axioms and in the list of postulates which he enumerates we find the following:

(I) Effects are always proportional to their causes or their productive forces, since the latter are the causes of the former only to the extent that the former are the effects of the latter and only by reason of what the latter cause.

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(VI) The velocities of the same body or bodies of equal mass are like the motor forces which are exerted on them, that is to say, those which cause their velocities; inversely, when the velocities are in the same ratio, they are those of the same body, or of bodies of equal mass.

(VII) The distances covered with uniform velocity in the same time by any given bodies are to each other as these same velocities and inversely, when these distances are in this ratio, they have been traversed in equal times.

(VIII) The distances traversed in equal times by the same body, or by bodies of equal mass are like the forces which cause them to traverse these distances. And, inversely, when these distances are in this ratio they have been traversed in equal time by the same body or by bodies of equal mass. This axiom is a corollary to the two preceding ones, axioms VI and VII.

From now on the word velocity will always mean uniform velocity, unless otherwise specified.

It is impossible to formulate more precisely the axiom of dynamics constantly referred to in the *Physics* and in the *On the Heavens*, an axiom presupposed in the *Mechanical Problems*. It is stunning to realize that the person who confirms this axiom in such a clear and explicit manner is a famous mechanician and a contemporary of Newton. The error is deep rooted and it will be a long and arduous task to eradicate it completely. From a root stock long believed dead, new buds continue to sprout unexpectedly. The views professed by Varignon in his dynamics are a striking example of the vitality of an erroneous idea.

Since Varignon accepts the principle of Aristotelian dynamics, the Law of the Composition of Forces seems self-evident to him. It can be reduced to the Law of Composition of Velocities and can be obtained by the same methods used by Roberval.[169]

Once the principle of the Composition of Forces has been established in this fashion, Varignon can reduce all of the possible cases of equilibrium in machines to that principle. In all these cases, the resultant forces are reacted by the supports. We do not need to point out in detail how ingenious but often artificial the processes of reduction are. Many of these processes became classical and are still taught in school.

It is only in the *New Mechanics*[170] that Varignon produces the famous theorem which is stated today in the following form:

With respect to any given point in the common plane, the moment of the resultant of two forces is equal to the algebraic sum of the moments of their components.

Thanks to this beautiful theorem, his name is known today to every beginning student of mechanics. However, it took little effort for him to discover it.

Leonardo da Vinci had already grasped the truth of this proposition in the case where the point about which one sums the moments is taken in the direction of one of the three forces. One of the moments is then equal to zero. Stevin found it in this form and published it. After him, Roberval, Hérigone, Wallis, Dechales, Casati, Pardies and Borelli reproduced it. A simple generalization was all that was needed to produce the theorem stated in the *New Mechanics*. However, today's student, who recognizes the name of Varignon, knows nothing of Simon Stevin.

The systematic reduction of statics to the Law of the Composition of Concurrent Forces occurred not only to Varignon. At the same time, it is also present in the reflections of Father Lamy, who, in 1687, stated his ideas in a letter.[171] addressed "to M. Dieulamant, Engineer to the King in Grenoble." Let us quote a few passages from this letter:

(1) When two forces pull the body Z (Fig. 109) along lines AC and BC, called the *lines of direction* of these forces, it is obvious that the body Z will not move along either line AC or BC, but along another line which lies between AC and BC, which I shall call line X and which will be the path traversed by Z.

(2) If the path X were blocked, then the body Z which is destined to traverse this path, would remain immobile, and the forces would remain in equilibrium . . .

(4) Force means that which is capable of producing motion. One can only measure the motions by the distances they traverse. Therefore, let us assume that force A is to force B as 6 is to 2. Thus, if A, in the first instant were to pull the body Z towards itself up to point E, then B at the same instant, would only have pulled it to point

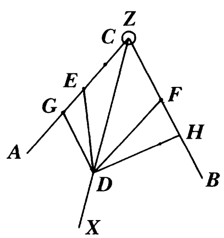


*fig. 109.*

F. I assume that CF is only one third of CE. We have seen that Z is incapable of moving along AC or BC. Thus, in this first instant, it must move to D where it corresponds to E and F. That is to way that it has traversed the value of CE and FC. Everyone agrees on this . . .

(6) This line X has such a ratio to the lines of direction of the two forces A and B that at whatever point one draws from it two perpendiculars to these lines, they are inversely proportional to each other as the forces, or as DE is to DF.

After having shown that the Composition of Forces applies to the inclined plane, the winch, as well as to the rod supported by two ropes, etc., Father Lamy adds:

I therefore, cannot believe that one could possibly wish for a simpler and more efficient principle for resolving all the problems in mechanics, and for determining precisely the force in all machines, regardless of how one applies the forces which are needed to move them.

The similarity was very great between the ideas stated by Varignon in his *Project for a New Mechanics* and those outlined simultaneously by Father Lamy in his letter to M. Dieulamant. Therefore, in 1688, in his *Histoire des Ouvrages des Sçavans,*[172] Basnage accuses Father Lamy of plagiarizing Varignon:

It appears, he says, that Father Lamy owes his discovery of the new principles of mechanics to Varignon.

Father Lamy defended[173] himself vigorously against this accusation and asserted the indubitable independence of his discovery from the research of Varignon.

Father Lamy would have been justified in calling attention to the difference between his demonstration of the Law of the Parallelogram of Forces and that demonstrated by Varignon, and he might well have taken pride in this difference. However, this difference was quite minimal on the surface and amounted to no more than the introduction of these few words:

In the first instant.

But, in reality, the difference was quite significant, because it transformed the reasoning which linked the Laws of the Composition of Forces to Peripatetic dynamics into a reasoning which linked the same law to modern dynamics. According to the principles of modern dynamics, it is true, indeed, that if diverse forces, either constant or variable, act successively upon the same moving body no longer at rest, the velocities which these forces impart to the body after an infinitely small interval of time — which is the same for all of them — are proportional to the intensities of these forces.

At the same time that he was undertaking to reduce all of statics to a single principle, represented by the Law of the Composition of Forces, Father Lamy succeeded in deducing this law of forces from the laws of an accurate dynamics. However, at the same time he was addressing his letter to Dieulamant, Newton was publishing his immortal work on the *Mathematical Principles of Natural Philosophy.*[174] The great geometer was also setting out to deduce a justification for the Law of the Composition of Forces from the principles upon which rests the science of motion. He succeeded in doing so by following precisely the same path taken by Father Lamy. He might even have indicated the path which he had followed in a less clear way than had the scholar from the Order of Orators.

According to Newton,[175] to each force there corresponds something which could be called an *instantaneous force* (sic) and which he calls *vis impressa*. He gives the following explication of this *vis impressa:*

Consistit haec vis in actione sola, neque post actionem permanet in corpore.[176]

It seems that the following idea must be inferred from this formulation which is too concise to be clear: The *vis impressa* is the effect produced by a force which acts upon a moving body during an infinitely small interval of time, chosen once and for all.

The *vis impressa* thus causes the moving body to move in a straight line with a uniform motion which for a given body has a velocity which is proportional to the intensity of the force applied for an instant. From this, Newton easily deduces the demonstration of the Law of the Parallelogram of Forces.[177]

Today, when we compare the method by which Newton and Father Lamy arrived at the Law of the Composition of Concurrent Forces with that by which Varignon obtained the same result, we can make a very clear distinction between the two. Varignon arrived at the Law of the Parallelogram of Forces by utilizing the Law of the Composition of Velocities and the following axiom: A force is in the same direction as the velocity of motion which it produces and it is proportional to that velocity. On the contrary, Newton and Father Lamy use the Law of the Composition of Accelerations as well as the following postulate: The acceleration of a moving body is in the same direction as the force which produces its motion and it is proportional to that force. Of these two principles, we consider the first a grave error and the second an essential truth.

The geometers of the XVIIth and XVIIIth centuries do not seem to have attached the slightest importance to this distinction. After two millenia the propositions of Peripatetic dynamics had become second nature in the minds of physicists. Physicists continued to invoke them without hesitation as long as their implications did not blatantly contradict the discoveries of the new dynamics.

Is it not evident that the works of Varignon provide a striking example of what we have just said? When Varignon publishes his *Project for a New Mechanics* in 1687, he takes as the point of departure for his deductions axioms which appear to be borrowed from the *Physics* or from the *On the Heavens*. However, at the very same time, Lamy and Newton are demonstrating that the same results can be deduced by means of an exact dynamics. Varignon certainly knew of the *Letter* by Father Lamy and it would be highly improbable that he did not know of the *Principia* of Newton. These works would have enabled him to correct his own reasoning and to rid it of any trace of an antiquated physics. But did he bother to do so? In no way. For thirty-five years he spent all of his efforts developing the germinal ideas contained in the *Project*. And the *New Mechanics*, the fruit of such persistent labor, is even more profoundly permeated by Peripatetic dynamics than the initial outline. The same can be said about the *Neó-Statique* of Father Saccheri.

Father Saccheri was a native of San Remo, but the year of his birth is unknown. He died in Milan on October 5, 1733. That same year, he published a book on geometry entitled *Euclides ab omni naevo vindicatus.*[178]

This work alone proves that Father Saccheri was a powerful and original thinker. He had the honor of being considered by Beltrami[179] as a precursor of Legendre and Lobatchewsky. M. P. Mansion said the following about this work:[180]

> Despite its defects, the *Euclides ab omni naevo vindicatus* is the most remarkable book ever written on the *Elements* prior to Lobatchewsky and Bolyai.

Such a geometer would seem to be particularly capable of avoiding paralogisms when dealing with the principles of mechanics so that one ought to expect his *Neó-Statique*[181] published in 1703, to be free of all contradiction.

A work by a fellow cleric, Father Ceva,[182] had called Father Saccheri's attention to certain remarkable properties of a weight supposedly capable of attracting the constituent parts of diverse bodies towards a fixed center and which supposedly had an intensity proportional to the distance of the part attracted toward the common center of falling bodies. This law of gravity is precisely that which Jean de Beaugrand, *the geostatician*, had proposed and which Fermat accepted with some minor changes.

On the topic of a weight obeying such a law, Saccheri attempts to demonstrate two propositions which are incidently quite accurate. The first of these propositions, which seems to condense the scattered truths contained in the erroneous views of Fermat, can be stated in the following way: If gravity follows such a law, the resulting weight of a body always passes through a point (center of gravity) which occupies in that body a position which is totally fixed and independent of the orientation of the body.

The second of these propositions asserts that a point mass, released without any initial velocity and descending in free fall, will always take the same time to reach the common center of falling bodies, regardless of the initial distance from this common center at the outset of motion.

Of these two propositions which Saccheri attempts to establish, the first is derived from statics while the second is derived from dynamics. Thus we are able to recognize the principles used by the Jesuit scholar in these two branches of mechanics.

Saccheri puts the notion of *momentum*[183] at the very beginning of his deductions. This notion, which is similar to what Galileo called *momento* and which is identical to the Cartesian *quantity of motion*, can be obtained by multiplying the *mass*[184] of the moving body by the *velocity* which it has. Generally, Saccheri refers to this as *impetus*.[185]

The composition and decomposition of the *momento* or the *impetus* is nothing but the composition and decomposition of velocities. It is easy for Saccheri to formulate the solution to this problem known since Aristotle. However, we soon see[186] that the propositions arrived at in this fashion undergo an imperceptible change and what had previously been proven about the *impetus* is now applied to the *vis motrix* and the laws of kinematics concerning the composition of velocity are transformed into laws of statics on the composition of forces, apparently without the author himself being aware of this change, which the reader too has difficulty in recognizing.

It is by such a transposition of *force* to *impetus* that the apparent weight of a heavy body on an inclined plane is evaluated.[187] It is true that in this evaluation we are dealing with a velocity from a state of rest (*impetus ex quiete*) and one could see in this an indication that the forces must be measured by the velocity they impart to the moving body which started from a position of rest after an infinitely short interval of time. If this were so, the reasoning of Saccheri would be similar to that of Lamy and Newton and would be correct. Yet no further explanation of the word *ex quiete* indicates that it should be given such importance in this passage. It plays no role in the ideas on statics developed by Saccheri and seems to be a mere subterfuge to render less blatant the obvious contradictions between the statics and dynamics of the same author.

Can we doubt for even a moment that Saccheri considers the *vis motrix* proportional to the *impetus* and identical to the *momentum*, when we read the following definition of the center of gravity:[188]

By center of gravity we mean the point in every heavy body through which the natural direction of the composite *impetus* passes which tends towards the common center of all heavy bodies. This direction must be understood as resulting from all of the natural *impetus* by which the different parts of the falling body tend towards the same center.

It is clear that the statics of Saccheri is based entirely upon the assumption that force is proportional to the *impetus*, that is to say, to the velocity. Similar to the statics of Varignon, the statics of Saccheri takes all of its principles from the dynamics of Aristotle.

But when Saccheri addresses the problems of motion, he invokes the dynamics of Newton. When he considers a point mass which describes a given trajectory,[189] he considers the *impetus vivus* of this body, that is to say,[190] the velocity directed along the tangent to the trajectory. He also considers the *impetus subnascens* along any given direction D. Following what he affirmed constantly in his first two books, this quantity is identical to the quotient of the mass of the body by the component of the weight in the direction D. If Saccheri had consistently followed the principles which he deduced from his statics, he would have considered as equal the *impetus subnascens* in the direction D and the component of the *impetus vivus* in the same direction. However, he is not consistent. He equates the increase (*incrementum*) of the component along D of the *impetus vivus* with the *impetus subnascens.* Expressed in modern terms, he equates the quotient of the mass of the moving body by the component of the weight along a given direction with the component of the acceleration in this same direction. Such an equivalence is the basic principle of Newtonian dynamics.

Thus we can see how Saccheri, a very skilled geometer and an equally subtle logician, makes use of propositions of statics which he has established by following the methods of Aristotle to deal with the problems of Newtonian dynamics. Likewise when the great Euler explains in an admirable treatise[191] the mechanics which derive from the works of Newton, he also adopts *in toto* the laws of statics which Varignon had founded upon Peripatetic principles.

These examples suffice to show how slowly and uneasily Aristotelian dynamics gave way to modern dynamics. The reason lay, of course, in the fact that Aristotelian dynamics offered a much more obvious explanation of the most common experiences. Modern dynamics is infinitely more abstract and is the result of a prodigious effort of reflection and analysis. It took centuries to wean the human mind from Aristotelian dynamics and to accustom it to the modern approach.

## 7. THE LETTER OF JEAN BERNOULLI TO VARIGNON (1717). THE DEFINITIVE FORMULATION OF THE PRINCIPLE OF VIRTUAL DISPLACEMENTS

In the year 1687, it appeared that mechanics had abandoned forever the Method of Virtual Displacements developed by Jordanus, Descartes and Wallis, as well as the Method of Virtual Velocities of Aristotle, Charistion and Galileo. With the exception of Casati and Dechales, everyone who wrote on statics after Wallis either ignored these methods or declared that the human mind was unable to find sufficient certainty in them for the foundation of statics. At best the authors agreed to treat them as corollary to propositions constructed upon other hypotheses.

After attempting to base the entire science of statics upon the principle of the lever, the authors finally recognized in the Law of the Composition of Concurrent Forces an axiom from which the rules of equilibrium for all machines could easily be deduced. By relating this law directly to the essential principles of the theory of motion, they gave it a clarity and certainty perfectly suited to a hypothesis which must support an entire doctrine.

Henceforth, statics seemed finally to be headed in the direction which Varignon had proposed for it in his *Project for a New Mechanics* and which Father Lamy had mentioned in his letter to Dieulamant. All statics needed to do was to progress in the direction which those authors had proposed for it. Varignon dedicated the rest of his life to this end and attempted to bring statics to the goal he had pointed out. His efforts resulted in the *New Mechanics or Statics,* which was published shortly after its author's death and was to remain a classic work for a long time.

As far as the Method of Virtual Displacements was concerned which we have followed in its continuous development from Jordanus to Descartes and Wallis, it now seemed that it was irrevocably rejected and was about to be consigned to oblivion forever.

When one follows the slow and complicated process by which a science develops, one often observes an idea which for a time shines brilliantly but then gradually grows dim again and disappears from sight. It seems to have been extinguished forever. However, quite often this disappearance, which had been taken as a final extinction, is but a temporary eclipse. The moment when the idea disappears from view is soon followed by the moment when it reappears, more brilliant than ever, as if it had hidden for awhile in order to rest and gather new force and brilliance.

We already have seen how the Method of Virtual Displacements,[192] so influential in the works of Jordanus, the Precursor of Leonardo da Vinci and in Leonardo and Cardan themselves, had been ignored or rejected by Guido Ubaldo, Benedetti and Stevin. However, at the very moment at which it appears to have been completely abandoned, Roberval and particularly, Descartes return to it. Henceforth, this principle is autonomous and devoid of any connection with the postulate of virtual velocities or with the dynamics of Aristotle.

As we shall see, the Method of Virtual Displacements undergoes a very similar resurrection. In the very same book — the *New Mechanics* of Varignon — which seems to signal the definite eclipse of this method and usher in the final triumph of a statics founded upon the composition of forces, we find the principle from which this method derives in its definitive form.

It is, indeed, in his *New Mechanics* where Varignon inserts a letter which Jean Bernoulli had sent him from Basel on February 26, 1717. The letter contains the following passage:

Imagine several different forces which are acting through different tendencies or directions to maintain in equilibrium a point, a line, a surface or a body. Further imagine that a small displacement is being imparted to the entire system of these forces and that this can either be a rectilinear displacement in any direction or a rotational displacement about any fixed point. It is easy to see that with such motion each of these forces will move forwards or backwards in its direction unless one or several of the forces have their displacements perpendicular to the direction of the small displacement. In this case, this or these forces will neither move forward nor backwards any distance at all because these progressions or retrogressions which I call *virtual velocities*[194] are nothing but the augmentation or diminution of each line of direction after the small displacement has been imparted. These augmentations and diminutions can be calculated if one draws a perpendicular to the extremity of the line of direction of any force. This perpendicular will carve out of the same line of direction, which has moved to an adjacent position because of the small displacement, a small segment which will be the measure of the virtual velocity of that force.

Let P, (Fig. 110) for example, be a point within the system of forces which maintain one another in equilibrium. Let F be one of these forces which pushes or pulls point P along the direction FP or PF. Let Pp be a short straight line which point P describes in a small displacement by which the force FP takes the direction fp, which will either be exactly parallel to FP, if the small displacement of the system occurs parallel to a given straight line,[195] or it will form, when prolonged with FP, an infinitely small angle, if the small displacement of the system occurs about a fixed point. Draw PC perpendicular to fp and you will have Cp for the *virtual velocity* of force F, so that F x Cp equals what I call energy. Notice that Cp is either positive or negative in relation to the other displacements. It is positive if point P is pushed by force F and the angle FPp is obtuse. It is negative if the angle FPp is acute. But if, on the contrary, point P is being pulled, Cp will be negative when the angle FPp is obtuse. It will be positive when the angle is acute.

Once the above has been understood, I can formulate the following general proposition: any time there is a state of equilibrium of given forces, however they are applied or regardless of their direction, whether they act upon each other directly or indirectly, then, the sum of the positive energies will be equal to the sum of the negative energies taken positively.

It is in these terms that Bernoulli formulates the principle which, henceforth, is complete and from which all the laws of equilibrium can be derived.

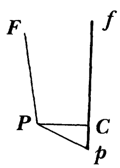


*fig. 110.*

How did Jean Bernoulli come to an understanding of this general axiom? What Varignon communicates to us from Bernoulli's letter gives us no information on this point. However, it is not hard to guess what we cannot prove by documentary evidence.

The difference is slight between the form which Wallis had given to the Principle of Virtual Displacements and the form which this axiom has assumed since then. All that is needed to pass from one formulation to the other is to openly state what Wallis had suspected all along, and to consider precisely an infinitesimal displacement, i.e., infinitely small quantities of work. This transformation presented no difficulty to a geometer experienced in infinitesimal analysis. Thus it appears very likely that Jean Bernoulli arrived at his statement of the Principle of Virtual Displacements by coordinating and perfecting the diverse statements scattered throughout the work of Wallis. It is through Wallis and Descartes that his work is linked to the efforts of Jordanus and the mechanicians of his School.

It is not true that the Method of Virtual Displacements for which Bernoulli had just furnished a general and precise statement, won universal approval nor did the mechanicianis acknowledge in it the principle from which all of statics must be derived. Varignon himself, who publicizes the discovery of the great geometer of Basel, fails to recognize in the method a principle. He only sees in it "a general corollary of the theory" which he founded upon the Law of the Parallelogram of Forces.[196]

This proposition seemed to me so general and so beautiful, says Varignon, that after I saw that I could easily deduce it from the preceding theory, he granted my request to include it here with the demonstration that this theory had furnished me, but which he had not yet sent me. I now give it in its individual application to all the above mentioned machines.

And then Varignon proceeds tirelessly to prove in fifty pages that all of the machines for which he has deduced the conditions of equilibrium from the Law of the Composition of Forces verify the equality posited by Bernoulli. Guido Ubaldo had done the same with the axiom of Aristotle and so had Father Pardies with the axiom of Descartes. They had refused to bestow the title of principle to these rich and far-reaching postulates and relegated them to the rank of corollaries.

We are now approaching the conclusion of our history of statics. The *New Mechanics* of Varignon and the *Letter* of Jean Bernoulli close the period of the development of statics which truly deserves to be called the *Origins*. Henceforth, the *Classical Period* begins. We set out to search out the sources of a river. We described its tumultuous beginnings and then its torrential passage through sinuous gorges. The river has now reached a gently undulating plain through which it pursues its peaceful course.

At the moment we take leave of this river, it has divided into two branches, with its current taking two different directions. It seems to be guided by the two impulses which statics received in its origins. In the first, we recognized the influence of Archimedes, in the second, that of Aristotle.

From Archimedes to Varignon, the mechanicians never ceased to pursue the same ideal. They continue this pursuit from Varignon to Poinsot and from Poinsot to our own time. They dream of constructing a statics on the model of Euclid's *Elements of Geometry*. By means of a thorough and ingenious analysis they hope to reduce the most complicated cases of equilibrium in the most diverse systems until they can see clearly simple and elementary instances of equilibrium. Furthermore, in these simple and elementary instances they want equilibrium to be as self-evident and certain as those truths of common sense to which Euclid appealed. The goal of Archimedes in his treatise *On the Equilibrium of Planes* was to provide statics with principles which would be acknowledged as just as clear and certain as the axioms of geometry. Such was the desire of Daniel Bernoulli and then of Poisson, when they attempted to establish the Law of the Parallelogram of Forces without reference to the general principles of dynamics.

While a great many mechanicians follow this first current of thought, others follow the direction which Aristotle had given to statics. They are not attempting analysis which breaks down the most complex laws of equilibrium and reduces them to clear and self-evident elementary propositions. Rather their efforts tend towards a broad synthesis. They are attempting to reunite in a single and universal principle all natural or artificially produced cases of equilibrium. It is clear that they deduce this principle from a few simple and evident observations. However, the extreme generalization which takes them from a few individual experiences to such an all-encompassing law, denies to this law any claim to self-evidence. The more science becomes aware of the logical processes which it puts into play as it develops, and the more it realizes that the certainty of such a general hypothesis can not be contained in the few facts which suggested the hypothesis, the better it sees that what confirms the hypothesis and gives it its value is the ease with which the hypothesis can classify the multitude of diverse laws discovered by experience, or the certainty with which it portends new laws to be discovered.

It is this latter tendency which had led geometers, from Jordanus and his disciples to Roberval and Descartes, from Descartes to Wallis and Jean Bernoulli, to continually refine and extend the Principle of Virtual Displacements. Each of the two tendencies attempts to dominate statics and the conflict between them is incessant. However, an impartial observer of this struggle easily recognizes the advantages in both methods. It is true that the analytical mind, by means of meticulous criticism, helps remove every trace or error from the truths discovered by the process of synthesis. However, its own discoveries are rare and meager and only serve to demonstrate its sterility. Fecundity is the prerequisite of the spirit of synthesis. It is the Method of Virtual Displacements which continues to expand the domain of statics.

The exclusive use of this latter method characterizes the *Mécanique analytique* of Lagrange. The work of Lagrange represents the confluence of all the currents which bore statics through history and is the culmination of all of the tendencies which have guided its evolution.

During the different periods of its development, statics has taken for the basis of its deductions first the principle of the lever, then the properties of the inclined plane and finally the Law of the Composition of Forces. All of these principles are equivalent to each other, and their equivalence stems from the fact that they all derive directly from the Principle of Virtual Displacements. Thus the science of equilibrium is brought to perfect unity by Lagrange. Henceforth, it is condensed into a single formula.

Varignon, in developing an idea which Albert of Saxony and Guido Ubaldo had outlined, attempted to discover the explanation for all of the cases of equilibrium in the forces exerted by moving bodies upon their supports. Lagrange deduces from the Method of Virtual Displacements a process which is as simple as it is accurate for defining and determining those forces which are negated by the contacts between the body and its supports.

The doctrine of Albert of Saxony, which states that the center of gravity of any heavy body tends to join the common center of heavy bodies, furnished a principle of statics which Galileo and Torricelli state in the following terms:

A system is in equilibrium when any change in its configuration would cause its center of gravity to rise.

For a long time, this principle remained separated from the principle of equality between the motor work and the work of the resistance and from the principle of Jordanus, Descartes, Wallis, and Jean Bernoulli. Lagrange discloses the close tie between these two principles.

The Principle of Torricelli is not the exact equivalent of the principle of Jean Bernoulli. The latter applies to all cases of equilibrium, while the former excludes some. It is only because of the general theory of stability, as established by Lagrange, that we are able to distinguish the cases of equilibrium which the Principle of Torricelli defines and to demonstrate that they are the only stable states of equilibrium.

The physicists attempted to deduce the fundamental principle of statics from the laws of dynamics. In this way, Roberval and Varignon deduced the Law of the Parallelogram of Forces from Peripatetic dynamics, i.e., from the proportionality between force and velocity. Father Lamy and Newton more correctly deduced it from the proportionality between force and acceleration. D'Alembert, in a certain sense, inverted the question by showing how any problem of motion could be reduced to a problem of equilibrium. Lagrange, in turn, used the Method of Virtual Displacements to develop a formula which reduces every problem of motion to an algebraic equation.

An arrangement of solid bodies is, by the way, not the only system where equilibrium depends on the Principle of Virtual Displacements. The statics of deforming bodies and, in particular, of fluids, derives entirely from this principle. The various methods proposed by Newton, Bouguer, Clairaut and Euler for dealing with hydrostatics can all be reduced to this general method.

By means of the Method of Virtual Displacements Lagrange establishes a magnificently unified and organized statics. In it all laws of equilibrium for solid or fluid bodies can be classified in perfect order, and all the legitimate aspirations of those who had promoted the science of equilibrium are finally realized.

After Lagrange, the Method of Virtual Displacements remains the most precise and most general method to which mechanicians can resort any time an obscure point needs clarification or any time an embarrassing difficulty requires a solution.

It is true that Navier did discover the general equations of elastic equilibrium without the help of the above method. However, when he needed to complete his work by adding to these general equations the boundary conditions which complete the definition of the problem, he approaches this problem by using the Method of Virtual Displacements.

Poisson believes that the elasticity of a solid body generally depends on only 15 coefficients. Cauchy and Lamé increase this number to 36. However, by using the procedures of Lagrange, Green is able to settle the controversy by proving that the exact number of coefficients is 21.

It is through the principle of the equilibrium of channels, which Clairaut had conceived and from which Lagrange had deduced the Principle of Virtual Displacements, that Laplace derived the equation of the capillary surface. However, his demonstrations are weak when he tries to establish the laws governing the interface between the liquid and the container. The invariability of the angle of contact is postulated, but not demonstrated. In a work which contains one of the most beautiful examples of the method of Lagrange, Gauss demonstrated with absolute precision all the laws of capillarity.

The theory of equilibrium of elastic plates seems to pose a hopeless enigma for geometers. Cauchy and Poisson are not in agreement in their formulation of the boundary conditions which must be met at the edges of a plate, and the conditions which they propose are more than necessary. Once more it is the Method of Virtual Displacements which allows Kirchhoff to solve the enigma by lisiting, without any omission or repetition, all of the boundary conditions required along the edge of an elastic plate.[197]

Indeed, the Method of Virtual Displacements can be proud of the domain which it conquered and of the clear laws and perfect order it imposed. However, suddenly at the end of the XIXth century, new areas, prodigiously rich and vast, increase its already existing empire. It is no longer only mechanical equilibrium which is governed by its laws. Henceforth, with sovereign authority, it will decree the conditions for those states of equilibrium which occur in electrified and magnetized systems. The minute seed planted by Jordanus not only produced the *Mécanique analytique* of Lagrange but the chemical and electrical mechanics of Gibbs and Helmholtz as well.

## CONCLUSION

After the traveller has crossed the arid limestone plateau of Larzac[198] with its grayish rounded hillocks and its maze of rocks resembling the ruins of a deserted city, he approaches the plain washed by the Mediterranean. The path which he must now follow is formed by steep ravines which are the traces of ancient streams or dried-out riverbeds and which, with the passing of time, cut deeper and deeper into the limestone plateau. Soon these ravines join in a single defile. Sheer walls, topped with ominous parapets of crumbling stones, now border the bed where once a beautiful river flowed deep and wild. Today, this riverbed is nothing but a chaos of worn and broken blocks. No spring flows from its rocky walls and no pool of water wets its gravel. Among the mass of stones, no plant can grow. The Vissec[199] is the name given to this parched river of death by those who live here in the Cévennes.[200]

The traveller, who can only advance with extreme exertion through the mass of fallen stones, occasionally hears a faraway rumble, like the roll of distant thunder. As he presses on, this rumble grows louder, before it bursts forth with a loud crashing sound. This is the great voice of the Foux.

In the limestone wall, a dark gaping cavern opens up like a giant maw. From this maw, white torrents of water gush forth and fall back thunderously in a mixture of crystal-clear droplets and bubbling white foam. The fissures in the distant limestone plateau have gathered this water in an underground lake.

Suddenly a river is born and henceforth, the clear and cold waters of the Vis flow between the white shores and the silvery oyster-beds. Its cheerful mumuring arouses in response the clicking of the mills and the deep ringing laughter of the Cévennes villages, while a glorious sunbeam glides over the notched edge of the plateau and slips down towards the bottom of the gorge, painting the poplar branches with a gold lining.

When traditional history, falsified by prejudice and distorted by deliberate simplifications, attempts to recount the development of the exact sciences, the image which it calls to mind resembles that of the course of the Vis River.

In this traditional view, Greek science flooded vast areas with its abundant and fertile waters. At that time the world witnessed the great discoveries of the likes of Aristotle and Archimedes germinate and grow, to be admired forever. Then the source of Greek thought dried up and the river to which it had given birth ceased to deliver its life-giving force to the Middle Ages. The barbaric science of those days was nothing but chaos in which the unrecognizeable remnants of Hellenic wisdom were piled up helter-skelter, arid and sterile scraps to which clung, like parasitical, gnawing lichen, the puerile and conceited glosses of the commentators. All of a sudden, a great clamour shook this scholastic desert. Powerful minds cut through the rocks which had hidden for centuries the pure water from the ancient sources. Set free by these efforts, the waters gushed forth, happily and abundantly. Wherever they flowed, they brought about the rebirth of the sciences, of literature and the arts. The human mind simultaneously recovered both its vitality and its freedom. Soon thereafter, the great doctrines were born which, as the centuries passed, grew ever deepening roots and spread ever more impressively their branches and foliage.

But this traditional view is sheer nonsense. During its evolution, human science has had very few instances of sudden births or rebirths, just as the Foux is an exception among the sources of rivers.

A river cannot suddenly fill a large river bed. Before a river flows majestically, it is at first a mere stream, and a thousand other similar streams had to become its tributaries. At times those tributaries become numerous and abundant and the river rises rapidly. But when the tributaries are but few, mere trickles of water, the river hardly rises. Sometimes even, a cleft in the porous soil engulfs part of its waters and diminishes its flow. But through it all, its flow varies only gradually and does not disappear completely or spring out of nowhere.

Nor does science, in its progressive advance, have any sudden changes. It grows, but by increments. It advances, but by steps. No individual human intelligence, whatever its force or originality, will ever be able to produce a completely new doctrine at one stroke. The historian enamoured of simple and superficial views, delights in the brilliant discoveries which illumined the dark night of ignorance and error with the bright daylight of truth. Yet, anyone who is willing to analyse deeply and carefully what first appears to be a unique and unexpected discovery must soon conclude that it was the result of a great many imperceptible efforts and a conjunction of an infinite number of hidden tendencies. Each phase of the evolution which moves science slowly toward its goal has two characteristics: continuity and complexity. These characteristics are very clearly evident to the student of the origins of statics.

The historian enamoured of over-simplification will mention only one work produced on statics by the Ancients: the work of Archimedes. Such a historian will present the work as an isolated colossus, towering above the ignorance surrounding it. But to appreciate the greatness of this work, it is not necessary to distort it by presenting it in total isolation. The statics of the geometer of Syracuse is characterized by research of impeccable rigor in its deduction. His research is a subtle analysis applied to complicated problems and it presents wonderfully clever solutions to problems of which no one but a geometer understands the importance. All of those characteristics are the hallmark of a refined science and are far from the groping hesitations of a nascent doctrine.

It is obvious that Archimedes had precursors, who before him and by methods different from his own, had understood the laws of the equilibrium of the lever which Archimedes was to develop so magnificently.

History has retained traces of some of these precursors. The *Mechanical Problems* may not be by Aristotle, as tradition would have it. In any case, the statics explained there is so closely connected with the dynamics set forth in the *Physics* and in the *On the Heavens* that we must attribute the *Mechanical Problems* to some close disciple of the Stagirite. The methods of demonstration followed in this work might have been methods of invention. However, the same cannot be said about the deductions of Archimedes.

On the other hand, an ancient tradition stubbornly maintains that the writings about the lever are to be attributed to Euclid. These writings might not be those which we possess today under the name of the great geometer. Yet, merely denying their existence hardly proves the contrary.

If there were predecessors to Archimedes in Antiquity, he also most certainly had successors. The science of Byzantium and Alexandria followed the different paths outlined by Archimedes. The art of engineering brought to a high degree of perfection by the great Syracusan, inspired the works of Ctesibius, Philo of Byzantium and Hero of Alexandria.

Pappus, on the other hand, turned his attention to the study of centers of gravity, hoping in this fashion to become the equal of the Geometer. The enigmatic Charistion, finally, carried the principles of statics beyond Aristotle and Archimedes with his arguments on the Roman balance.

The Arabs transmitted only a tiny fraction of Hellenic statics to the Western medieval world, but they are far from being the servile and unimaginative commentators they are usually depicted as. Their minds are quite receptive to the remnants of Greek thought which reached them via Byzantine and Islamic science. These remnants are enough to arouse their attention and to stimulate their minds. From the XIIIth century on, and prehaps even earlier, the School of Jordanus opens up avenues for the mechanicians which Antiquity had not known.

The intuitions of Jordanus de Nemore are initially rather vague and uncertain. Serious errors are mixed with great truths. But, little by little, the disciples of the great mathematician clarify the ideas of their master. Errors disappear and truths become more precise and established. Several of the most important laws of statics are finally confirmed with full certainty.

To the School of Jordanus we owe, in particular, a principle which will continue to grow in importance with the further development of statics. This principle bears no analogy to the special postulates on the lever upon which Archimedes had based his deductions, and it has only a slight affinity with the general axiom of Peripatetic dynamics. It asserts that the same motor force can lift different weights to different heights, provided that the heights are inversely proportional to the weights. Thanks to this principle, which Jordanus had applied exclusively to the straight lever, the Precursor of Leonardo da Vinci is able to grasp the law of equilibrium of the bent lever, the notion of moment, as well as the apparent weight of an object placed on an inclined plane.

During the XIVth and XVth centuries, the statics which had originated in the School of Jordanus quietly pursues its own course and no other important stream of ideas adds to its flow. Not until the beginning of the XVIth century does it become a raging torrent, when Leonardo da Vinci brings to it his genius.

Leonardo da Vinci is far from being a seer who all at once discovers previously unsuspected truths. He is in possession of a prodigiously active mind, which is simultaneously daring and cautious. He returns to the laws of mechanics established by his predecessors, discusses them, and analyzes their every aspect. His relentless reflections enable him to refine several ideas already known to the disciples of Jordanus, and to demonstrate their richness and fecundity. An example of this is the notion of the motor force or the notion of moment. By means of a marvelous demonstration he is able to extract from this latter notion the Law of the Composition of Concurrent Forces. Yet, his mind, given to hesitation, alternation and reversal of opinion, does not always hold firmly to the truths which it has once grasped. Leonardo is unable to come to a definitive conclusion on the problem of the inclined plane, which had been quite satisfactorily resolved since the XIIIth century.

The indecision which always plagued Leonardo's soul and which so rarely allowed him to complete a work, kept him from bringing to completion the *Treatise on Weights* which he wanted to write. The fruits of his intellectual labours, however, were not entirely lost to science. Through the oral tradition which had originated during his lifetime, through the dispersion of his manuscripts after his death, his thoughts were scattered to the four winds and many fell on soil propitious to their growth.

Cardan, one of the most universal minds and also one of the most bizarre men produced by the XVIth century, and Tartaglia, a mathematical genius and a shameless plagiarist, both recovered for the statics of the Renaissance several discoveries made by the School of Jordanus. What they recovered was often in the richer and more fertile form that Leonardo da Vinci had given those discoveries.

Through the works of Tartaglia and Cardan a current of Medieval mechanics starts to spread during the XVIth century. At the same time, a counter-current develops and gains strength in the treatises of Guido Ubaldo del Monte and Giovanbattista Benedetti. The works of Pappus and Archimedes had just been unearthed and were being studied and discussed with great passion and skill. They restored in mechanics the desire for impeccable rigor, the hallmark of geometers since Euclid. This enthusiastic but narrow-minded admiration for the monuments of Hellenic science had only contempt for the profound but still flawed discoveries which the XIIIth century Schools had produced. The most profound intuitions of Jordanus and his disciples are misunderstood by the new school, which impoverishes and weakens statics while pretending to purify it. In the same spirit, the exclusive admiration for the works imbued with Hellenic grace scornfully rejects as "gothic" the most marvelous artistic creations of the Middle Ages.

Thus, at the end of the XVIth century, almost nothing remained of what the specifically Western mind had produced in statics. Everything had to be done again. One had to redo the demonstrations of the truths which the medieval scholars had grasped and give them the clearness, precision and rigor of the theories bequeathed to us by the Greeks. This work of restoration, which will last to the middle of the XVIIth century, will be the task of the most brilliant geometers of Flanders, Italy and France. Yet, despite the extraordinary talents of those men, much groping and many false starts were involved before the work came to completion!

A rigorous deduction must assume certain axioms. Where are the postulates to be found to form a solid foundation for statics? Those formulated by Archimedes are extremely restricted. They barely suffice to deal with the equilibrium of the straight lever. It becomes absolutely necessary to resort to new hypotheses. The mechanicians who are going to formulate them will present them as original principles and previously unknown truths. However, once we remove the pretense of originality, in which the vanity of those who had proclaimed them as truth had wrapped them, we are faced in almost every case with very old propositions, kept alive and nurtured by a long tradition which demonstrated their richness. Where a short-sighted and overly rigid historical view thought it could see a Renaissance of the scientific method which had fallen into oblivion since the Greeks, we ourselves see nothing but the natural evolution of medieval mechanics.

Galileo, as legend would have it, the creator of modern dynamics, returns to the already tottering dynamics of Aristotle to find the foundation for his deductions. He postulates the proportionality between the force moving a body and the velocity of that body. He is influenced by the mechanicians of the XIIIth century when he attempts to deduce from this principle the apparent weight of an object placed on an inclined plane. However, he fails to conclude from the works of these same mechanicians that the cardinal notion of all of statics is the notion of motor power, the product of a weight and the distance of its fall. Galileo replaces this notion with that of *momento*, the product of the weight and the velocity of its fall, a notion directly related to the previously rejected dynamics of Aristotle.

When Stevin deals with apparent weight on an inclined plane, he posits the impossibility of perpetual motion. However, Leonardo da Vinci and Cardan had already formulated this principle with remarkable clarity by linking it with the notion of motor power which they, in turn, had taken from the School of Jordanus. However, this notion plays only a secondary role in the work of Stevin. The great geometer from Brugge failed to recognize its extreme importance.

The same notion appears more clearly in the beautiful demonstration which Roberval furnished of the law describing the composition of concurrent forces. This demonstration, which fills so well the major deficiency in the work of Stevin, does not come out of nowhere. In order to deal with the equilibrium of the bent lever, the disciple of Jordanus who was the Precursor of Leonardo da Vinci, had already sketched the model for it.

The wonderfully insightful and systematic genius of Descartes soon grasped unerringly the main idea which must serve as the basis for all of statics. This idea is the same which Jordanus had already called attention to in his theory of the straight lever and is also the same idea used by his disciple in dealing with the bent lever and the inclined plane. It is the notion of motor power. Descartes defines this notion with great precision and shows its superiority to Galileo's use of *momento*. While the concept of *momento* derives from a dynamics henceforth untenable, the notion of motor power allows one to formulate a very clear, very certain axiom upon which all statics can rest. To be accepted, this autonomous principle does not have to wait until the new dynamics has been built upon the ruins of Peripatetic dynamics.

Unfortunately, the obsessive arrogance which so often dominates Descartes causes him to exaggerate the magnitude of the service which he renders to statics, and even to exaggerate to the point of distorting the facts. Descartes, who was even less disposed than Stevin, Galileo or Roberval to do justice to his predecessors, portrays himself as the creator of a doctrine when he is merely its organizer. What we have said here about Cartesian statics, could probably be said for all of Cartesianism. The haughtiness of its author has triumphed, and its triumph is unequalled in the history of the human mind. It has duped the entire world and has portrayed Cartesianism as a strangely spontaneous and unexpected creation. However, the Cartesian system is in most cases nothing else but the clearly formulated conclusion of anonymous efforts pursued throughout centuries. The gracious flight of the butterfly showing off its iridescent wings causes us to forget the slow and laborious creeping of the humble and unobtrusive caterpillar.

The few lines in which Jordanus demonstrated the law of the straight lever contained the seed of an accurate and fertile idea. This idea continued to develop from Jordanus to Descartes until it encompassed all of statics. While the gradual evolution of this truth occurs, science becomes the scene of another equally interesting, but more bizarre phenomenon. An incorrect doctrine is slowly transformed into a very profound and correct principle. It seems as if a mysterious force is watching over the progress of statics and is able to render beneficial both truth and error alike.

Archimedes had used the notion of the center of gravity without defining it. Several other geometers had attempted to make it more precise. However, Albert of Saxony and, after him, the majority of the physicists of the School, took advantage of mechanical imprecision on this point, and attributed to it characteristics which are completely different from those which we ascribe to it today. They considered, for example, that weight was concentrated in each part of a body. The weight of a body appeared to them as the desire of the center of gravity of that body to join the center of the universe. The Copernican revolution which relocated the center of the universe and even went so far with Giordano Bruno as to deny the existence of that center, hardly modified this theory of weight. It considered this quality to be the tendency inherent in the center of gravity of each body to join its analog, the center of gravity of the earth.

One of the claims to glory of Kepler is to have eloquently fought against this hypothesis of an attraction between geometrical points and to have asserted that the attraction of gravity was exerted between the different parts of the earth, taken two by two. However, his less clairvoyant contemporaries did not share this opinion. Benedetti, Guido Ubaldo and Galileo, in particular, postulated a sympathy felt by the center of gravity of each body towards the common center of heavy bodies, while Bernardino Baldi and Villalpand plagiarized the accurate corollaries which Leonardo da Vinci had deduced from this erroneous doctrine.

When this tendency is as completely satisfied as the connections of a system of weight allow it to be; when, in other words, the center of gravity of a system is as close as possible to the center of the earth, nothing induces this system to move any more and it remains in equilibrium. This is the principle of statics as formulated by Cardan, Bernardino Baldi, Mersenne and Galileo and which perhaps they all borrowed from Leonardo da Vinci.

However, this principle is erroneous. In order to render it accurate, one needs only to extend to infinity that center of the earth which Galileo never ceases to invoke in his reasonings, and to consider the verticals as being parallel to each other. This modification seems insignificant. However, it is in truth profound because it transforms a false statement into an accurate and fertile axiom. It is also profound because it presupposes the final abandonment of a very ancient and very authoritative theory of weight.

The confusing and complicated debates provoked in Florence by the research of Beaugrand and Fermat on the variation of weight according to its elevation prepare this reform and Torricelli completes it by giving science a new postulate upon which statics could be founded.

After having traced the continuous and complex development of statics, when the historian looks back to contemplate the total view offered by this science, he can only be astounded when he compares the breadth of the finished theory with the minuteness of the seed which brought it forth. On the one hand, he will be able to decipher several lines of an almost illegible XIIIth century manuscript in Gothic handwriting. Those lines justify very precisely the law of equilibrium of a straight lever. On the other hand, he can leaf through endless treatises of the XIXth century in which the Method of Virtual Displacements helps to formulate the laws of equilibrium for purely mechanical systems, as well as for those where physical changes, chemical reactions, electric or magnetic phenomena can occur. What a world of difference between the simple demonstration of Jordanus and the imposing doctrines of Lagrange, Gibbs, and Helmholtz! And yet, those latter doctrines were contained potentially in that demonstration. History has allowed us to retrace, step by step, the efforts through which they were developed from that tiny seed.

The contrast between the extremely small and simple seed and the strikingly complex, finished theory is analogous to what the naturalist sees when he looks at the development of a plant or an animal of a higher order. However, this marked contrast will not excite his admiration as much as another spectacle far more worthy of his attention and reflection. The process which he is analyzing is the result of an infinite number of different phenomena. A great many cell divisions, budding transformations and reabsorptions are needed to produce the end result. All these phenomena, however numerous, varied and complex they may be, are coordinated with perfect precision. All of them combine very efficiently for the formation of a plant or of an adult animal. And yet, the countless beings which act in these phenomena, the cells which proliferate, the phagocytes which devour the tissues which have become useless, most certainly are unaware of the final goal they are working to reach. They are workers ignorant of the final product, but, who, nevertheless, methodically bring to finality that product. Thus the naturalist cannot help seek outside of and above these individual efforts something which is hard to define, but which already has the final product of the plant or the animal in mind and which during the formation of the organism sees to it that the multitude of unconscious efforts combine to obtain the end product. Like Claude Bernard,[200] the naturalist will accept a "guiding idea" as presiding over the development of every living being.

Anyone who studies the history of science is led to similar reflections. Each proposition in statics was slowly elaborated through a process of research, experimentation, hesitations, discussions, and contradictions. Among all these many efforts, not one was wasted. Each one contributed to the final result. Each one played a greater or lesser role in the formation of the final doctrine. Even error proved fertile. The erroneous and sometimes bizarre ideas of Beaugrand and Fermat forced geometers to sift through the theory of the center of gravity in order to separate the precious truths from the falsehoods which had been intermingled.

Yet, while all these efforts contributed to the advance of a science which we can admire today in its finished form, no single contributor to these efforts even suspected the final magnitude and shape of the edifice he was helping construct. When Jordanus developed the law of equilibrium for a straight lever, he was certainly not aware that he was formulating a principle which could form the basis for all of statics. Neither Bernoulli nor Lagrange had any inkling that their Method of Virtual Displacements would one day be perfectly suited to deal with electric and chemical equilibrium. They could not anticipate Gibbs, even though they were his predecessors. Like skillful masons cutting and cementing stone, they worked on the completion of an edifice without ever having seen the overall design of the architect.

How could all these efforts combine with such precision and bring to completion a plan which was not known to the individual laborer, unless this plan existed previously in the mind of an architect, and if this architect did not have the power to direct and coordinate the labor of all the masons? Even more than the growth of a living being, the evolution of statics is the manifestation of the influence of a guiding idea. Within the complex data of this evolution, we can see the continuous action of a divine wisdom which forsees the ideal form towards which science must tend and we can sense the presence of a

Power which causes the efforts of all thinkers to converge towards this goal. In a word, we recognize here the work of Providence.

*Bordeaux, October 26, 1905.*

# NOTES TO VOLUME II

## A. ON THE AXIOM OF ARISTOTLE

In Chapter I of this work (Vol. I, pp. 11—12) we considered the Principle of Virtual Velocities and the way in which Aristotle applies it in the *Mechanical Problems* to the theory of the lever as a corollary of the following Peripatetic axiom: The same force which moves a given body at a given velocity is also able to move a body K times heavier but with a velocity K times smaller.

We previously gave a formulation of this axiom which we borrowed from the *On the Heavens*. Another now follows taken from the Fifth Chapter of the VIIth Book of the *Physics* where the Stagirite formulates the principles of his dynamics:[1]

If the motor force is $\alpha$, the body moved $\beta$, the distance traversed $\gamma$, and the time needed to traverse this path $\delta$, then the same power called the force $\alpha$, will move in the same time half of $\beta$ over a distance twice $\gamma$; it moves through distance $\gamma$ in half the time $\delta$ and thus the proportion will be maintained.

In an interesting critical analysis which G. Vailati was kind enough to make on Volume I of our work, he used the following terms when discussing this proposition:[2]

It seems to me even less evident that this proposition could have any relation whatsoever with another equally important proposition stated by Aristotle in his *Mechanical Problems*. I am referring to the proposition which attributes the equilibirum of two forces applied to the extremities of a lever to the fact that in any given displacement of this lever, its extremities describe arcs which are in inverse ratio to the forces applied to them.

The only common feature of this proposition and the one stated previously consists in the fact that both assert the existence of an inverse ratio between the two weights (or two forces) and the two velocities. But this common characteristic is of little importance in comparison to the differences between the two propositions. In the first proposition, it is a question of velocities *actually acquired effectively* in the same time intervals by two falling bodies of different weight (today we would say of different mass) under the action of the same force (like two spheres of different weights placed on the same horizontal plane). In the second proposition, on the contrary, one considers the velocities which two heavy bodies *would acquire* or the points of application of two forces which would be in equilibrium with each other in any given mechanism, if this mechanism were to be changed from the configuration in which it is in equilibrium.

Thus it is impossible to consider these two propositions identical without depriving each one of its most significant feature.

Despite this criticism, we persist in our conviction that the Method of Virtual Velocities contained in the *Mechanical Problems* can be derived from the axiom formulated by Aristotle in the VIIth Book of the *Physics* and in the IIIrd Book of the *On the Heavens.*

One can confirm this in the following way: Let us consider a lever with an applied force $\alpha$ and a resistance $\beta$. The resistance is located at a given distance from the point of support and we assume that the force $\alpha$ can move it and make it describe an arc $\gamma$ within a time $\delta$. It will also be able to move the weight $\beta/2$, placed at twice the distance from the point of support, because within the same time $\delta$, it will cause it to describe the arc $2\gamma$. Thus the same force (*ισχυς*) is required to move a given weight placed at a given distance from the point of support as is required to move half of that weight over a distance twice the length. From this, a justification of the theory of the lever as stated in the *Mechanical Problems* can be easily deduced. And it is this very justification which Aristotle seems to invoke when he says in support of his demonstration:[3]

*Ωστ' από τῆς αυτῆζ ισχύος πλέον μεταστήσε τοα τὸ κινοῦν τὸ πλεῖον τοῦ υπομοχλίου απέχον.*

That the Method of Virtual Velocities applied to the lever by the author of the *Mechanical Problems* is a corollary to the laws of dynamics posed by Aristotle in the VIIth Book of his *Physics* is not at all, as Vailati would have it, a view which we dreamed up. Rather, it seems to us that this opinion is universally accepted by tradition.

After commenting on these principles of Peripatetic dynamics, Simplicius adds:[4]

Based on this ratio between the motor force, the moving body, and the distance traversed, Archimedes invented the instrument designed to weigh, called the *charistion.*[5]

It is, indeed, upon the principles of Peripatetic dynamics that Charistion based the theory of the Roman balance. Thâbit ibn Qurra puts this proposition at the beginning of the restored text of Charistion:

If two mobile bodies traverse two unequal distances within the same time, these distances are in the same ratio to each other as the ratio between the motor power (*virtus motus*) of the first body and the motor power of the second body.

The following, adds Thâbit, is an example of this proposition:

Let us consider two moving bodies. The first traverses 30 miles and the second 60 miles in the same interval of time. It is known that the motor power of the moving body which travels 60 miles is double the motor power of the body which travels 30 miles, just as the distance of 60 miles is twice the distance of 30 miles.

This proposition is self-evident. There is no intermediary between it and comprehension.

Immediately after this proposition which he considers to be self-evident, Thâbit establishes the Law of the Lever using the Method of Virtual Velocities in practically the same fashion as the author of the *Mechanical Problems* had done. In order to justify this method, the commentator on Charistion invokes the proposition which he had formulated in the first place:

We stated previously that when two bodies in motion traverse unequal distances in the same time, the motor power of the first is to the motor power of the second as the distance covered by the first is to the distance covered by the second. . . . The motor power of extremity B of the lever is to the motor power of extremity A as the two distances covered by those points in the same time, that is to say as arc BD is to arc AF.

Thus Thâbit justifies the use of the Method of Virtual Velocities in statics with a proposition from dynamics. In Peripatetic terms, this proposition could be stated in the following fashion:

If a given power (ισχύς or δύναμις) moves a given body in a given time a given distance, twice that power is needed to move this same body in the same time twice the given distance.

This axiom from dynamics is not quite identical to the one which we saw formulated in two different ways by the Stagirite. It is not even actually formulated among the rules contained in the Fifth Chapter of the VIIth Book of the *Physics*. However, it is a direct corollary of two of these rules: the one which we previously quoted as well as the following one:[6]

Half of the power will cause half of the moving body to cover the same distance in the same time. Let $\varepsilon$ be half of the power $\alpha$, and $\zeta$ be half of the moving body $\beta$. The power will retain the same ratio to the load ($\beta\alpha\varrho\acute{v}\varsigma$), so that it will cause it to cover the same distance in the same time.

Incidentally, it is not from Aristotle that Thâbit borrowed the axiom upon which he was to found the Method of Virtual Velocities. The source which he used can be found elsewhere, as he himself tells us:

This section, he states, is based upon the book attributed to Euclid.[7] Hoc autem capitulum innixum est super librum qui nominatur *Liber Euclidis.*

With these words the great Arab astronomer is referring to the fragment on specific weight which is entitled *Liber Euclidis de gravi et levi, et de comparatione corporum ad invicem* (Vol. I, pp. 50—56).

This short fragment does, indeed, begin with several definitions and axioms. If we follow the propositions in a chronological order, the fourth, the fifth and the sixth propositions correspond to the postulate affirmed by Thâbit. The following are the three propositions:

— Bodies are equal in power (*virtus*) when they traverse in equal time equal distances within the same medium of air or water.
— Those which traverse the same distance in different time intervals are called bodies different in power (*in fortitudine*).
— And the one with the greatest amount of power will require the least amount of time.

Euclid, or whoever the author of the *Liber de gravi et levi* was, did not merely formulate these postulates, which are the logical equivalents to the principle used by Thâbit. He also deduced from the above-mentioned principle another principle which he states in the following way:

If bodies traverse unequal spaces in an equal time interval, the body which has traversed the greatest space has the greatest power.

Yet, by formulating the proposition in this particular way, the author's goal appears quite different from that pursued by Thâbit. He is not trying to justify a method of statics. He is merely attempting to prove that the power of heavy bodies of the same "kind" is proportional to the volumes of those bodies. With reference to the axioms mentioned at the beginning, it follows that within the same medium of air or water, heavy bodies of the same "kind" (that is to say of the same specific weight) will fall with velocities which are proportional to their volume.

This corollary would be the natural conclusion of what we read in the *Liber de gravi et levi* which the manuscripts attribute to Euclid. However, it is precisely this conclusion which is missing today from the mutilated volume.

This conclusion is one of the fundamental laws of Peripatetic dynamics which Aristotle stated as follows in Book I of *De Caelo*:[8]

The ratio between weights is in inverse proportion to the lengths of time of their fall. If a weight falls from a given height in a given time, twice that weight will fall from the same given height in only half the time needed by the first weight.

Hellenic and Arabic science agree in considering the rules stated in Book VII of the *Physics* as principles that are equally suited to serve as a foundation for dynamics and to justify in statics the Method of Virtual Velocities. (Several modern mechanicians defend the same view.)

Bernardino Baldi, after quoting the passage in which Aristotle formulates the Law of the Lever, adds the following:[9]

This assertion is most surely true and widely acknowledged. Yet, we cannot claim with certainty that this marvelous effect has as its cause the velocity which results from the length of the arm of the lever. What is velocity in an immobile body? The lever and the balance remain immobile when they are in equilibrium and, yet, in that case, a small force is sustaining a large weight.

The answer to this question could be that if a larger velocity is not acting on the larger arm, it exists at least in potentiality. However, I would ask you, of what importance is something which exists only in potentiality in a body in the process of action? A force which provides support can only do so in the process of action.

This criticism directed at the Method of Virtual Velocities closely resembles that formulated by Stevin only several years after Baldi published his exercises and which John of Guevara will attempt to refute:[10] To do this, Guevara resorts to the axiom of Peripatetic dynamics according to which the same body which is successively moved by different forces moves with velocities which are proportional to those forces:

In the case of local motion, he states, velocity always implies or supposes ease; a greater velocity or a greater ease of motion necessarily indicates a greater gravitational force or a greater motor force, as can easily be seen by examining either natural or violent motion. The heavier a body, the faster it descends, if its motion is unimpeded.

Projectiles move faster within a given medium, the greater the impulse given to them by the projecting machine. The greater the motor force of animals, the faster they walk and the faster the motion will be which they are capable of imparting to heavy bodies, assuming the same configuration of the instruments they activate.

It is for this reason that in the question under examination the extremity of the large arm of the lever is moving faster and is endowed with a more powerful gravity *in hoc situ*.[11] This greater velocity also indicates that the extremity is endowed with a greater motor force and that it is capable of supporting a larger weight, even though it does not move.

But there is one mechanician who, very carefully, very explicitly, and under many different conditions justified the Method of Virtual Velocities by using the following axiom from Peripatetic dynamics:

The force which causes a given weight to describe a given trajectory within a given time, is also capable of causing a weight $k$ times larger to describe the same trajectory, but in a time which is also $k$ times longer.

This mechanician is none other than Galileo.

It is indeed by means of the above-mentioned axiom which he emphasizes greatly, that Galileo introduces[12] his notion of *momento*, the cornerstone of the statics which he develops in the *Discorso intorno alle cose che stanno in su l'acqua*, in the treatise *Della Scienza meccanica*, and in the *Discorsi*. This very Aristotelian notion corresponds in many cases with precisely what the Stagirite had called (*ισχύς* or *δύναμις*). When Galileo first defines it, he is careful to cite the *Mechanical Problems*.[13] Thus it is clear that in the eyes of the great geometer of Pisa, the statics delineated in the *Mechanical Problems* is closely related to the dynamics formulated in Book VII of the *Physics*.

All of Galileo's contemporaries share this view. When Mersenne defends the Method of Virtual Velocities against the attacks of Descartes, he invokes the following axiom:[14]

If a force can lift a weight to a given height in a given time, a force which is double the first will lift the same weight twice as high in the same time.

That is the very axiom which Descartes is calling into question[15] when he argues for the Principle of Virtual Work. The Peripatetic Jesuits such as Father Honoré Fabri[16] make of the statics of Galileo a corollary of Aristotelian dynamics. In a word, the majority of the XVIIth century mechanicians follow in the footsteps of Simplicius and Thâbit ibn Qurra by acknowledging the accuracy of the following proposition: the

Method of Virtual Velocities, as it is stated in the fourth problem of the *Mechanical Problems* with reference to the lever, owes its strength to the rules of dynamics set down in the Fifth Chapter of Book VII of the *Physics.*

We think we are justified in qualifying this view. By asserting that the velocity at which a weight moves is proportional to the force which moves it, those rules are certainly in accordance with the theory of the lever which Thâbit ibn Qurra set forth when he restored the writings of Charistion. However, the theory of the balance and the lever set forth in the *Mechanical Problems* seems to us to be far too complex to be explained in its entirety by these principles. Several assertions which appear rather obscure to us moderns become clear when we approach them with a more precise knowledge of Peripatetic dynamics.

No other passage is better suited to reveal to us the true principles of this dynamics and to demonstrate to what degree these principles differ from our science of motion than the chapter in Book IV of the *Physics* in which Aristotle attempts to prove the impossibility of a vacuum.

We are accustomed to distinguish in each moving body two elements: the force which moves and the mass which is moved. Nothing similar can be found in Peripatetic physics. No notion used in that physics bears any analogy to our modern notion of mass. Every body moved is necessarily submitted to two forces, an impressed force and a resisting force. Without the force, the body will not move, without the resistance, its motion would be instantaneous. The velocity at which the body moves depends simultaneously upon the magnitude of the force and the magnitude of the resistance.

For example, in the most simple natural motions, the force is represented by heaviness or lightness; the resistance comes from the medium in which the motion is produced.[17]

> We have seen that the velocity at which the same weight or body moves can increase due to two causes; it can increase due to a change in the medium in which the motion takes place — this medium being either water, air or earth. It can also increase — everything else being equal — by a change in the moving body such as an increase in weight or lightness.

The velocity of the moving body must vary in the same ratio as the force and in inverse ratio to the resistance. What laws are being followed here? According to a very perspicacious insight of G. Milhaud,[18] Aristotle, being a mediocre mathematician, could never conceive of any other form of a function than proportionality. Thus he will assume, without explicitly stating it, that the velocity of a moving body is proportional to the force and in inverse proportion to the resistance. Such a law is inadmissible, since the velocity is nullified when the force is equal to the resistance. This insight does not escape the attention of the *Calculatores*[19] of the XIVth and XVth centuries and provokes quite a debate among them. However, it does not seem to have occurred to Aristotle.

The Stagirite goes even further. Without balking, he assumes that the resistance of a medium is proportional to its density, so that the velocity of descent of a falling body within a given medium is inversely proportional to the latter's density.

Let us assume the body $\alpha$ is moving through medium $\beta$ for a time $\gamma$; later, it is moving through a more permeable medium $\delta$ for a time $\varepsilon$. The distance traversed is assumed to be the same for both media $\beta$ and $\delta$. These displacements take place according to the ratio between the resisting media. If, for example, medium $\beta$ is water and medium $\delta$ is air, the more permeable and more incorporeal the air, the faster the motion of $\alpha$ through the medium $\delta$. The different ratio between water and air will also be the ratio between the two velocities. Thus, if air is one-half as dense as water, the moving body will take twice as long to cover the same distance through medium $\beta$ as through medium $\delta$, and time $\gamma$ will be twice time $\varepsilon$. The moving body will always move faster, the more uncorporeal, the less resistant and the easier it is to permeate the medium which it is traversing.[20]

The dynamics described in this passage which is at odds with modern ideas, must be taken into account by anyone wishing to explain the quite obscure arguments contained in the second of the *Mechanical Problems*.

We have already given[21] a succinct analysis of these arguments. We saw how Aristotle makes a kinematic analysis of circular motion and comes to the following conclusion: When a point traverses the lower half of a circle in a vertical plane, it is simultaneously carried downwards in a natural motion as well as towards the center of the circle, a motion which goes against its nature.

It is clear from the terms used by the author of the *Mechanical Problems* that for him the two components of the velocity correspond to the two forces which are proportional to them: the moving point "is being held back by force" (*κρατεῖται*) from the center.

Moreover, these two forces play the same role that force and resistance played in the fall of a body within a given medium. The force which corresponds by nature to the motion plays the role previously assigned to the body during the fall, while the violence exerted by the center is comparable to the resistance of the medium.

For the same value of the first force, the velocity of the moving body will be all the smaller, the larger the resisting action.

If of two moving bodies moved by the same force, one experiences a greater resistance while the other experiences a smaller one, it is accurate to say that the one restrained more moves more slowly than the one restrained less.[22]

When the moving body descends from a given height along a circle, it partakes of a motion which goes all the more against nature, the smaller the circle.[23] In these two circles, the ratio between the natural motion and the motion contrary to nature is not the same. Because of this, under the action of the same force, the body which is furthest away from the center, will move more rapidly. This is obvious from what we have said.[24]

We are convinced that the analysis which we have just finished reproduces the essence of the ideas of the author of the *Mechanical Problems*. It shows us how he reached the following proposition: It takes a smaller force to move a body at a given velocity when the motion takes place along a large circle rather than a smaller one. In the treatise of Charistion this proposition is deduced very simply from the rules formulated in Book VII of the *Physics* and in the treatise *De gravi et levi*, attributed to Euclid. This simple form of the Method of Virtual Velocities is suggested in the fourth problem of the *Mechanical Problems*, which deals with winches and capstans. However, it is not explicitly formulated in any of the *Problems*. The author of the *Mechanical Problems* viewed statics in a more complicated fashion. It is certain that he considered statics as derived from Peripatetic dynamics.

## B. ON CHARISTION AND ON THE ΠΕΡΙ ΖΥΓΩΝ OF ARCHIMEDES*

After expounding[25] our reasons for considering the *Liber Charastonis, editus a Tebit filio Corae* to be the work of a geometer by the name of Charistion, we undertook to find further evidence of this geometer in other works of Hellenic science.

* The Greek reads, *On Balances*.

In particular, we thought that Charistion might be identical with Heriston, the son of Ptolemy, to whom the latter had dedicated the *Liber diversarum rerum*. However, Eneström called to our attention[26] that the above-mentioned work was probably apocryphal and that the person to whom it is dedicated, called Heriston in the edition of the *Liber diversarum rerum* published in Venice in 1509, is called Ariston in other manuscripts. Eneström further remarked that this Ariston was the name of an otherwise unknown person to whom Philo of Byzantium addressed all of his writings and that the author of the apocryphal work had made this individual into a son of Ptolemy, not knowing that Ptolemy lived and wrote much later than Philo.

We are very grateful that Carra de Vaux, who published the *Livre des appareils pneumatiques et des machines hydrauliques* of Philo of Byzantium based on an Arabic version, wrote us on the aforementioned subject. The following is a passage from his letter:

> Allow me to call your attention to a small detail. The Arabic text of the *Pneumatiques* of Philo of Byzantium also contains the variant Mariston for the name Ariston. The letter M is interesting because it could very well be a spelling mistake for H or less probably for K:
>
> Arabic
> mâ, hâ, kâ
>
> The confusion of M, H, and K is common in Arabic writings. It would explain why the three names Mariston, Heriston and Karistion appear here together.

The observation by Carra de Vaux leads to a new hypothesis according to which Charistion, the author of the *Book on the Balance* as restored by Thâbit ibn Qurra, would be the contemporary and friend of Philo of Byzantium, to whom the latter dedicated all of his works. The name Ariston, like the name Karaston, would be an Arabic corruption of the Greek name *Χαριστίων*.

Moreover, this mistake has had very diverse effects. In the Arabic manuscripts of the works of Philo, one finds[27] the forms "Mouristos", and "Ristoun." In the Latin manuscripts one can read:[28] "Marzotom" or "mi Argutom."

It is generally believed that Philo of Byzantium lived during the second century B.C. Thus we would be forced to predate the life of Charistion as well as the composition of his work on the balance to that earlier epoch.

An earlier date for the work by Charistion would explain why it was attributed to Archimedes. We have already given the following quote from Simplicius:[39]

By using the proportionality between the motor force, the moving body and the distance traversed, Archimedes had invented an instrument for weighing which is called the *charistion.*

This quote can be compared to a passage from Pappus[30]

Archimedes, in his book, *On Balances,* as well as Philo and Hero in their *Mechanics,* respectively have shown that the smaller circles were less efficacious than the larger circles when both were produced by a rotation around the same center.

These passages from Simplicius and Pappus make claims which are hardly acceptable. First of all, contrary to the claim of Simplicius, Archimedes does not appear to have invented the Roman balance, which is discussed earlier in the XXIst problem of the *Mechanical Problems* of Aristotle. However, it could be argued that the great Syracusan had written a book *On Balances* meant to furnish the theory of that instrument. Yet, it would be quite improbable to think that he found in the Method of Virtual Velocities the principle of that theory, since he based his investigation entitled *On the Equilibrium of Planes* upon entirely different hypotheses and since the theorems thus arrived at could have easily furnished him with the laws of the Roman balance.

While the statements made by Simplicius and Pappus appear to be untenable when applied to Archimedes, they are, on the contrary, quite compatible with the writings of Charistion. And, immediately, the following supposition comes to mind: The treatise *Πεϱι Ζυγων,* which was read at Alexandria and Athens at the time of Pappus (IVth century A.D.) and Simplicius (VIth century A.D.) and which was attributed to Archimedes, might very well be the book *On the Balance* written by Charistion.

This hypothesis seems quite plausible. After all, Archimedes, whose renown was such that he had become a veritable legend, was credited with a great number of works which he had never written, such as, for example, the treatise *On Waterclocks* which was dedicated to Ariston, but was written without a doubt by Philo of Byzantium.[31]

Furthermore, other remarks concerning the *Πεϱι Ζυγων* of Archimedes appear to apply, in reality, to the lost treatise of the great Syracusan and to describe this work in terms far removed from those used by Pappus and Simplicius.

These valuable remarks can be found in the treatise on *Mechanics* written by Hero of Alexandria.[32] By using the notion of the moment of a weight with respect to the point of suspension, Hero of Alexandria formulates the condition of equilibrium for a nonrectilinear balance.[33] To this, he adds the following:

This is what Archimedes demonstrates in his book *On Balances*.

This passage is followed by the solution to another problem with the help of the same concept of moment. It concerns the equilibrium of two weights which are suspended from two points on a circumference of a wheel which rotates about its center. This problem could have been taken from the same work *On Balances*.

In another passage,[34] Hero deals with the equilibrium of a winch. At the outset, we find the following words which emphasize the importance of the problem posed:

As far as the cause is concerned which makes each of these instruments (the five simple machines) able to move large weights by a small force, we shall presently discuss it.

After having solved this fundamental problem, he adds:

We shall now apply to the five machines the demonstration which we have just made with the example of the circle. After this analysis, their operation will be clear. The Ancients always preceded it by a lemma.

Hero demonstrates this essential lemma quite simply by comparing the winch and a balance with a horizontal beam with the two arms unequal in length. He adds:

Archimedes has already furnished this proposition in his book *On the Equilibrium of Weights*.

Of the two quotes from Archimedes which we have just stated, the first most assuredly alludes to a work which is lost today. There is nothing to prevent us from supposing that this work, called *On Balances* by Hero, might have been entitled in Greek *Περι Ζυγων*.

Contrary to the opinion stated by Carra de Vaux,[35] we do not think that the second quote refers to the same work. Indeed, this quote merely discusses the rule according to which two weights suspended from a beam of a horizontal balance are in equilibrium when they are inversely proportional to the arms of the balance. This same proposition had been demonstrated by Archimedes in his well-known treatise Επιπέδον ισορροπικῶν ἢ κέντρα βαρῶν επιπέδων[36] This must be the work which Hero calls the book *On the Equilibrium of Weights*, a title which appears to be related to the word ισορροπικῶν.[37]

A further quote also clearly alludes to the same work by Archimedes. It reads as follows:[38]

When a heavy body is in equilibrium with another heavy body and when both are suspended from two points on a line which is divided in two and which is supported at the point of division, this line is parallel to the horizon if the ratio between the magnitudes of the weights is equal to the inverse ratio of the distances between their points of suspension and the point of division of the line. The weights suspended in this manner are in equilibrium without any inclination of the beam, as Archimedes demonstrated in his books *On the Equilibrium of Figures where Balances are Used.*

The proposition stated here by Hero is the theorem of mechanics which supports the entire theory set forth in the *On the Equilibrium of Planes*. The first part of the title, *On the Equilibrium of Figures*, could be considered a faithful translation of the Greek title. However, the Arabic title has as its second part, *Where Balances are Used.* If we recall that in another passage the title, *On Balances*, is probably applied on Περι Ζυγων, one wonders if Hero did not combine in a single reference the books *On the Equilibrium of Planes* and *On Balances.*

According to Hero himself, the quote which we just discussed is preceded and followed by other borrowings from Archimedes. Since these borrowings do not come from the *On the Equilibrium of Planes* they must come from the Περι Ζυγων.

Let us stop a moment and comment upon them, because they are certainly worth our consideration.

They deal with the center of gravity which is sometimes referred to by Hero as such, but more often as the "center of inclination" or even as the "point of suspension." Here is the definition (p. 73):

The point of suspension is an arbitrary point on a body or a plane figure such that when the body is suspended from this point, its parts are in equilibrium, that is to say, the body neither rotates nor inclines.

Hero adds the following to this definition:

According to Archimedes, heavy bodies can remain in equilibrium without inclining about a line or about a point. This is applicable to a line when the body resting on two points of this line does not incline in either direction. In that case, the plane perpendicular to the horizon drawn through this line remains perpendicular and does not incline about the line no matter where one puts it. . . . As far as the equilibrium about a point is concerned, it occurs when the body is suspended from that point — no matter what the motion of the point — and when its parts counterbalance each other.

A few pages further on (p. 75) Hero demonstrates two theorems without mentioning if his arguments stem from Archimedes. He formulates them in the following way: If a heavy body is successively suspended by different ropes, all of these ropes, if prolonged, will meet at the center of gravity of the body. If one observes successively the equilibrium of a body about different axes and if one notes each time the vertical plane passes through this axis, all of the planes will pass through the center of gravity of this body.

The various passages which we have just mentioned are preceded by the following (p. 73):

This question has been formulated by Archimedes with sufficient proofs. On this subject it should be said that Poseidonios, a Stoic philosopher,[39] furnished a mechanical definition of the center of gravity. According to him, the center of gravity or the center of inclination is a point such that when a body is suspended from this point, it is divided into two equal parts. Because of this, Archimedes and the mechanicians who followed him refined this definition and differentiated between the point of suspension and the center of inclination.

After reading these rather disjointed statements, we are led to make the following conclusions: The individual identified by the name of Poseidonios provided a mechanical definition of the center of gravity: A point such that a body suspended by this point remains in equilibrium. Archimedes gave two propositions: the first concerning the equilibrium of a body suspended by an axis, and the second concerning the equilibrium of a body suspended from a point other than the center of gravity. These two propositions present ways to determine the positon of the center of gravity. These propositions were established in the treatise *Περὶ Ζυγων*.

The evidence furnished by Hero would hardly be of much interest, were it not for a particular circumstance which corroborates what he says. Archimedes tells us himself that he had submitted the center of gravity to similar considerations. In his treatise *On the Quadrature of the Parabola*, he states the following:[40]

Every suspended body — no matter what its point of suspension — assumes an equilibrium state when the point of suspension and the center of gravity are on the same vertical line. This has been demonstrated.

Here we find the distinction made by Hero of Alexandria between the point of suspension and the center of inclination.

Now that we possess some precise information about the *Περὶ Ζυγων* of Archimedes, we can return to our study of the *Mathematical Collections* of Pappus and make the following assertion: the treatise *Περὶ Ζυγων* must have been already lost at the time Pappus was writing and he seems to have never seen it.

In Book VIII, Pappus returns to a more precise analysis of the considerations of the center of gravity which Hero borrowed from Poseidonios and from Archimedes. Pappus adds[41] that anyone wishing to study the elements of the centro-baric doctrine will find them in the book *On Bodies which are in Equilibrium* of Archimedes as well as in the *Mechanics* of Hero. Instead of quoting the treatise *On the Equilibrium of Planes*, from which he certainly did not borrow the above-mentioned statement, why would Pappus not quote *Περὶ Ζυγων*, the original source of this formulation if he had read it originally in the work of Archimedes, not merely in the summary written by Hero?

It is true that Pappus quotes *Περὶ Ζυγων* in a passage which we mentioned above. However, a reading of that passage only confirms our conclusion. Pappus states the following in that passage:

Archimedes, in his book *Περὶ Ζυγων*, as well as Philo and Hero in their *Mechanics*, respectively have shown that the smaller circles were less efficacious than the larger ones when they are produced by a rotation around the same center.

To which passage in Hero does this sentence refer? It must be the theory of the winch which the mechanician of Alexandria considers as the cornerstone of the theory of simple machines. Pappus tells us that Philo of Byzantium had already formulated a similar theory. This agrees very closely with what Hero tells us. Indeed, the latter tells us that "the Ancients always put this lemma" at the beginning of their theory on simple machines. He adds (pp. 111—112):

The winch is nothing but two concentric circles, one small — the circle of the shaft — and the other large — the circle of the drum. It is correct to suspend the weight from the axle and than apply the impressed force to the drum because in this fashion a small force prevails over a large weight. Those who have preceded us have already stated this. We have merely repeated it so that our book will be complete and well-organized.

These references to the Ancients and to those who preceded Hero of Alexandria conform well with Philo of Byzantium and his School.

In this passage to which the quote by Pappus conforms so well, Hero of Alexandria names Archimedes. Yet, as we have seen, he attributes to Archimedes neither the theory of the winch nor the remarks about the relative efficacy of unequal circles, but merely the law of the equilibrium of the lever. The work which he quotes is not the *Περὶ Ζυγων* but the book *On the Equilibrium of Weights*, that is to say, the treatise Ἐπιπέδων *ἰσορροπικῶν*, which in another passage Pappus calls *On Bodies which are in Equilibrium*. It would be strange that in this passage Hero did not quote from the book *On Balances*, Περὶ *Ζυγων*, which he knows and which he quotes twice on other occasions and from which he borrowed the fundamental properties of the center of gravity, if the mechanical properties of the two concentric circles were stated in that book.

Thus it appears that a theory of the winch was not included in the Περὶ *Ζυγων* of Archimedes. However, Pappus, who does not quote the above work when he is formulating his own theory, quotes from it in connection with a problem which, in all likelihood, is not contained in the work. Should we not conclude from the above that Pappus did not know that work of the Syracusan — except perhaps by hearsay — because the latter was no longer read in Alexandria during the time of Pappus?

It appears that during Pappus' time in Alexandria several books of Archimedes were only known by name, a fact which Thurot has recognized:[42]

Pappus quotes[43] the Περὶ *ὀχουμένων*[44] of Archimedes among the books on applied mechanics along with the *Pneumatics* of Hero. Obviously, all he knew of the book was its title.

If the Περὶ *Ζυγων* was already unknown in Alexandria during Pappus' lifetime, it stands to reason that it was also unknown in Athens during the time of Simplicius. Since, on the other hand, the work of Archimedes was mentioned in later works, such as the *Mechanics* of Hero and the *Collections* of Pappus, it was only natural that some looked for it among treatises which had some resemblance to it either in their title or in their content and that it was taken for a book on the balance written by a forgotten ancient author. It is in this way that Simplicius could well have mistaken the book of Charistion for the Περὶ Ζυγων of Archimedes.

Let us return to Charistion. We stated earlier[45] that the copyists of the *Liber Charastonis* had generally considered Charasto to be a proper name — the name of the author of the treatise. The following testifies remarkably well to this. The Biblioteca Ambrosiana at Milan has in its possession a manuscript (Ms. T. 100. Parte superiore) in which the treatise edited by Thâbit ibn Qurra bears the title.[46] *Liber Carastonis super Euclidem de ponderibus in mensuris.*[47] We know that in his preamble, Thâbit actually refers to the book which he intends to restore as a work containing deductions which are based upon the book on weights attributed to Euclid.

Besides the *Liber de statera*, what other work do we know which is attributed to Charistion? A manuscript in the Bibliothèque Nationale[48] contains within the *Liber Carastonis* a treatise *De figura sectoris*,[49] which it attributes to Thâbit. In a work kept at the Public Library of the University of Basel (Ms. F. II. 33), this treatise bears[50] the following title: *Liber Castoris de figura sectoris, seu Thebitus.* The word Castoris might well be the misspelling of the word Carastonis by a copyist. It is, therefore, possible that the treatise *De figura sectoris* is a work of the Greek geometer Charistion and that Thâbit ibn Qurra was only its editor.

We might also add that in certain manuscripts, this treatise *De figura sectoris* is attributed to Campanus.[51]

## C. ON THE *DE ARCHITECTURA* OF VITRUVIUS

Very rarely did the Ancients quote the *Mechanical Problems* of Aristotle. Diogenes Laertius is perhaps the only one to attribute to the Stagirite a work on mechanics. It is for this reason that the authenticity of that work has often been called into question, especially after Cardan rejected it in his *De proportionibus.*

However, this rarely quoted work has had considerable influence upon the development of mechanics, perhaps an even greater influence than the works of Archimedes. Indeed, the *Mechanical Problems* served as a model for various collections which considered from various aspects some of the problems treated by Aristotle. Many times

Aristotle's problems were discussed in connection with other similar problems.

In later notes,[52] we shall have the opportunity of calling attention to two of these collections. For the time being, we would like to mention briefly the least known and least interesting of these collections, that of Vitruvius.

In Book Ten of his *De Architectura*,[53] Vitruvius devotes one chapter[54] to propounding the principles of statics which explain the capabilities of machines. The content of that chapter is borrowed entirely from the *Mechanical Problems*. It is possible that the manner in which Vitruvius summarizes these problems was a result of the influence of Philo of Byzantium and his School. Above all, it bears the mark of the Roman mind which was so unable to retain from the Greek works which it analyzes their original philosophical depth and logical rigor.

In the peculiarities of circular motion Aristotle had searched for the reasons for the effects of different mechanisms. Hero tells us that the Ancients always prefaced their theory of simple machines with reflections on the relative efficacy of two concentric and unequal circles. Thus it should not surprise us to see Vitruvius give the following title to his chapter on statics: *On the force which the straight line and circular curve possess in machines designed to carry loads*. It should also not surprise us to read the following introduction:[55]

> I have said in brief what I consider necessary for understanding machines designed to pull and in which two motions or powers must be considered which are different from one another, but which work together as principles of two actions. One of these powers is the force of the straight line called *eutheia* by the Greeks and the other one is the force of the circular curve called the *cyclotes* by the Greeks. However, the truth is that the straight line cannot exist without the circular curve nor the circular curve without the straight line when loads are being lifted by winching machines.

To support these remarks, Vitruvius gives an example based upon the pulley. As Perrault points out, this example is confusing and unclear. Any interpretation of this obscurity is bound to be false. This obscure passage might perhaps be read as a garbling of the remarks on the windlass, which according to Hero, the Ancients always prefaced to their theory of simple machines.

We cannot see that Vitruvius tries to prove *more geometrico*[56] the efficacy which he attributes to circular motion. He does not attempt at all to apply to this motion the principles of Peripatetic dynamics, as did, on the one hand, the author of the *Mechanical Problems* and on the other hand, Charistion. Furthermore, he does not try to deduce these principles from the law of the lever taken as a principle. He is content to make simple allusions on the subject of different instruments, as can be gathered from the *Mechanical Problems.*

For example Vitruvius states the following about the lever:[57]

. . . the reason for this is that the part of the pincers from the fulcrum on, upon which it clamps the load which it is lifting, is the smallest. The largest part — which goes from the fulcrum to the other extremity, if one chooses to stretch it over that length — will give the following result by virtue of circular motion: squeezing with one hand, one can develop with this hand a force equal to the weight of a very heavy load.

After briefly discussing several problems of statics, all borrowed from the *Mechanical Problems*, Vitruvius adds the following:[58]

These examples show that it is because of the distance from the fulcrum and because of circular motion that all bodies are moved.

Such vague theoretical remarks satisfied the utilitarian Latin mind of Vitruvius.

## D. ON THE *MECHANICS* OF HERO OF ALEXANDRIA

Today, it is generally agreed that Hero of Alexandria lived long after Christ and that he was a contemporary of Ptolemy. Thus we are justified in having this section follow the one dealing with Vitruvius.

A great many writings of Hero have come down to us more or less intact in the original Greek and have been known for a long time. An exception is the important work entitled either *The Elevator* or the *Mechanics.*

For a very long time this work was only known through the numerous references made to it by Pappus in Book VIII of his *Collections* and through an excerpt of it which a copyist had added to the work of Pappus.[59]

The Greek text of *The Elevator* seems to be definitively lost. However, the renowned Qusta ibn Luqa furnished us with an Arabic version which was brought back from the Orient during the XVIIth century by a scholar named Golius, who bequeathed it to the Library at Leyden where it has remained ever since.

It is this Arabic version of Qusta ibn Luqa which Baron Carra de Vaux published after having translated it into French and which he commented upon in a remarkable introduction.[60]

Hero of Alexandria was familiar with the works of Archimedes. The great Syracusan is cited nine times in his treatise. The same holds true for the disputed name of Poseidonios, the only other person cited. Hero not only knows the *On the Equilibrium of Weights* of Archimedes, which we have, but also the book, *Περὶ Ζυγων*, which is lost today. His work is actually the only treatise which furnishes us with reliable information about this latter book. We examined this book earlier.[61]

The influence of Archimedes is not the only one evident in Hero. In two different passages,[62] he talks about the "Ancients" and about "those who preceded him." We have quoted these passages elsewhere.[63] By comparing them with a quote from Pappus, we came to consider them as allusions to Philo of Byzantium and his School.

If there is, however, one influence which deeply marks the treatise of Hero, it is surely that of the author of the *Mechanical Problems*. Carra de Vaux correctly sees this influence in the following remark:[64]

> In matters of natural philosophy Aristotle was the teacher of the author of the *Mechanics*. Hero was ungrateful in so far as he did not make reference to him. Nonetheless, the influence of Peripatetic thought upon the *Mechanics* is obvious. Like Aristotle, Hero is concerned with searching for the causes, the *why* of mechanical phenomena as well as with reducing these phenomena to simple principles. The chapters which he devotes to this end are among the most elegant and best organized in his book and bestow upon the entire work a mark of grandeur rendering it worthy to be ranked above the majority of mechanical treatises left by Antiquity and by Hero himself.
>
> . . . Besides the influence of Aristotelian thought, one also finds in the *Mechanics* an entire chapter[65] which seems to be an actual excerpt from the *Mechanical Problems* of Aristotle, although in an abridged form and with some significant variations. This chapter comprises seventeen problems in the form of questions and answers, the form Aristotle used in the *Mechanical Problems*. Furthermore, the chapter is preceded by an introduction which faintly resembles the introduction to the *Physics*.

Let us attempt to outline the major ideas of Hero on the principles of statics. It seems quite appropriate to take as the basis for the deductions of the mechanician from Alexandria the law of the lever taken upon the authority of Archimedes.[66]

Archimedes has already stated this proposition in his book *On the Equilibrium of Weights.*

This law is used, as we came to see in Note B, to establish the condition of equilibrium for the winch. This condition of equilibrium, in turn, is used to establish the following truth which is utilized to explain the lever: A larger weight which is moving on a small circle will be kept in equilibrium by a small weight moving on a large circle concentric to the first, if these weights are in inverse ratio to the arcs of the circles which they describe in the same time.

It is this deduction, which Hero says he borrowed "from the Ancients" and "from those who preceded him," which leads to the principle of statics first used by Aristotle and later by Charistion. However, this deduction does not lead to that principle by the means used by the above-mentioned mechanicians. They tied the principle of statics to the fundamental laws of Peripatetic dynamics. Hero, on the contrary, who was undoubtedly following the example of Philo of Byzantium, founded the principle upon the law of the equilibrium of the lever, which he presumes as having been directly established. It is interesting to note that such an approach is precisely the one which we encountered in one of the four propositions on the lever which the manuscripts attribute to Euclid.[67]

What had been the central idea of the entire statics of the *Mechanical Problems* of Aristotle and of the book *On the Causes* of Charistion was relegated in the line of reasoning adopted by Hero to a mere intermediate step, trifling at best. Instead of reducing all simple machines to the motion of two weights on two concentric circles, it was just as easy and even more natural to reduce them directly to the lever.

Hero understood this perfectly well:

> The five simple machines which move weight, he says,[68] can be reduced to circles with one common center; this is what we demonstrated with the different figures previously described. Let me remark, however, that the machines can be reduced even more directly to the balance than to circles. Indeed, we have seen that the principles for the demonstration of the circles derive from the balance. It can be demonstrated that the ratio between the weight suspended from the small arm of the balance and the weight suspended from the large arm is equal to the ratio between the length of the longer and the shorter arm.

The intermediate step, challenged by Hero as to its usefulness, has the advantage nevertheless of demonstrating the following proposition:

The force and the resistance are inversely proportional to the velocities at which their points of application move simultaneously. We know what role this proposition played in the development of statics.

Hero of Alexandria is familiar with this proposition. How did he come to know it? Was it by reducing all simple machines to two concentric circles? Was it simply by reading "those who preceded him?" He remains silent on that question. He does not even furnish any *a priori* justification of this law. He merely states that it is verified in the different machines for which he formulated a theory.

Thus, after having stated the theory of the windlass, he says:[69]

This instrument as well as all other similar machines are slow because the smaller the force is in relation to the heavy weight which it is moving, the greater the time needed to accomplish the work. The same ratio exists between the forces and the times.

He then goes on to verify the accuracy of this statement.

He proceeds in the same manner for the block and tackle by stating the following truth:[70]

The reduction in velocity also occurs in this machine . . . The ratio between the times is equal to the ratio between the motor forces.

A little further on, Hero writes the following about the lever:[71]

There is a reduction in velocity in this case too and according to the same ratio. There is, indeed, no difference between levers and windlasses. . . . As we already demonstrate in the case of the windlass, the ratio between the forces is equal to the ratio between the times, and the same demonstration can be applied in the present case.

When Hero formulated the theory of the windlass,[72] he actually did not say one word about the ratio between the motor force and the time. However, this ratio can be deduced easily from the theory which he borrowed from the "Ancients" and those "Ancients" undoubtedly had been careful not to neglect this corollary. However, Hero failed to restate this corollary although he uses it as if he had developed it.

Hero[73] also makes the same claims with regard to the wedge and the screw:

There is also a reduction in velocity in these two instruments. The ratio between the times is like the ratio between the forces.

The mechanician from Alexandria merely makes this assertion, without giving any proof. He is unable to give such a proof because he does not possess a complete theory of the wedge and the screw of which he has only vague notions. If he had a correct theory of these instruments, he would notice that the law which he is stating cannot be applied to the wedge and the screw without modification. He would see that one cannot use the velocities at which the points of application of the force and the resistance move, but only the vertical component of these two velocities.

Such a modification would probably not have overly surprised Hero of Alexandria. In one passage in his work, he seems to have felt the need for such a refinement.

Among the problems which are for the most part borrowed from the *Mechanical Problems* of Aristotle, we find however the following which is not in that work.[74] Let a weight be suspended from a support by means of a rope which hangs vertically. Let someone then grasp the rope anywhere between the support and the weight, and then pull[75] it until the segment which continues to hang down comes to be superimposed upon a given vertical line. Why is the effort required to pull the rope all the greater, the closer to the point of support one has grasped the rope? Hero proves that this is so because one imposes on the weight an ascent which is all the greater the closer to the point of support one has grasped the rope:

> And in order to lift the weight higher, one needs a greater force than to lift it less high, because in order to lift it higher, one needs more time.

One could see in this passage a hint of the change which Galileo will apply to the Principle of Virtual Velocities as formulated by Aristotle and Charistion. However, the presentation given here by Hero is extremely vague and imprecise.

In order to finish this account of the views held by Hero on the principles of statics, we remind the reader that the mechanician from Alexandria understood well the notion of the moment of a weight with respect to a point. As we saw in Note B, he applied that knowledge when he stated the conditions of equilibrium for a balance with a curved beam as well as for a wheel which can revolve around its center and which carries two weights suspended from two points on its circumference.

This short summary is enough to demonstrate the richness of the insights into the principles of mechanics contained in *The Elevator*. However, we should not give too much credit for these views to the mechanician from Alexandria. His own original contribution is small if not null.[76] Whether he reveals to us the sources of his theories or hides them from us, we can easily see that he has borrowed almost everything from those who preceded him, from Aristotle to Archimedes and perhaps even from the mechanicians of the School of Philo of Byzantium and as well as from Charistion.

## E. JORDANUS DE NEMORE

We noted earlier (Vol. I, p. 80) that the mathematician who is usually known by the name of Jordanus Nemorarius should actually be called Jordanus de Nemore, according to the unanimous testimony of the manuscripts. We added that in our opinion the surname "de Nemore" should be considered as designating the birthplace of Jordanus. This same opinion happens to have been stated as early as the end of the XVIth century by Bernardino Baldi. Indeed, he states it in the following way:[77] "Giordano, d'un luogo detto Hemore, si chiamò Hemorario."[78] It is scarcely necessary to note that the spelling Hemore or Hemorario for Nemore or Nemorario resulted from an error by a scribe or printer.

## F. ON THE PRECURSOR OF LEONARDO DA VINCI

The Precursor of Leonardo da Vinci[79] is the name we have given to the unknown author of a treatise on mechanics which was widely known in the XIIIth century and which we have already analysed at great length.[80]

However, we were unable to pursue this analysis as far as we would have liked. The edition of this text, published in 1565 by Curtius Trojanus contains so many errors that it is nearly incomprehensible. Besides this one printed text, we had access for quite some time to a unique manuscript contained in MS 7378A (Latin Collection) of the Bibliothèque Nationale. This latter text, quite difficult to read, also contains many errors. It was not until we began the revision of our present work that we learned of a manuscript from the XIIIth century which is both clear and accurate. It can be found in MS 8680A (Latin Collection) of the Bibliothèque Nationale.

With the help of this text, we were able to undertake a detailed study of the treatise by the Precursor of Leonardo da Vinci. This effort led us to new conclusions which we have developed elsewhere[81] and which we shall present only summarily in this note.

The treatise *De ponderibus*[82] which we attribute to the Precursor of Leonardo is divided into four books in these manuscripts. The first book deals with the propositions which Jordanus de Nemore had formulated earlier. It restates them or corrects them. Furthermore, it contains two very important additions: the condition of equilibrium for the bent lever and the apparent weight of a body on an inclined plane. These additions are arrived at by means of the very same approach which led Jordanus de Nemore to state the law of equilibrium for the straight beam.

The second book deals with problems which are very similar to those found in the *De canonio*. The third book concerns itself with the concept of moment and with the conclusions which can be drawn from it with respect to the stability of the balance. Finally, the fourth book treats of diverse problems in dynamics.

The conclusion to which our detailed study has led us can be stated in the following manner: While the first book was written during the Middle Ages by a disciple[83] of Jordanus de Nemore, the last three books are relics of Greek science which undoubtedly reached the West through the Arabs.

The letters on the figures and which are used in the demonstrations of Books II and III occur almost invariably in the following order:

A, B, C, D, E, Z, H. T.

This brings to mind the sequence in the Greek alphabet:

$$\alpha, \beta, \gamma, \delta, \varepsilon, \zeta, \eta, \theta.$$

Hultsch has already observed that this is a characteristic which allows us to recognize with confidence mathematical treatises of Greek origin.

In Book III, the concept of moment is stated in a form which closely resembles that contained in *The Elevator* of Hero of Alexandria. Finally, a great many of the problems in Books III and IV are borrowed from the *Mechanical Problems* of Aristotle, although the author has largely modified and often greatly refined the solutions of the Stagirite.

More than one mechanician from Antiquity borrowed heavily from the *Mechanical Problems* of Aristotle. In his *De Architectura*, Vitruvius gives the following title to the eighth chapter of Book X: On the force which straight and circular lines have in machines designed to carry weights. The content of this chapter is borrowed entirely from the *Mechanical Problems*. In his poorly organized work on *Mechanics*, Hero of Alexandria likewise restates[84] several problems from the *Mechanical Problems* of the Stagirite, although he does add important variations.

The last three books of the treatise which concerns us here form a similar collection. They represent for us a precious document of Greek science. It is through them that a great number of ideas expressed by Aristotle in his *Mechanical Problems* reached the West during the Middle Ages.

It is likely that these ideas arrived through the mediation of Arabic versions of those Greek texts. This transmission through the Arabic is the only explanation for the total absence of any latinized Greek word in the treatise which concerns us here. On the contrary, such latinizations can be found in abundance in works which have been directly translated from Greek into Latin, such as is the case for the *De canonio*.

Therefore, Books II, III, and IV represent important relics of Greek science. However, Book I is of a completely different character. There is no trace of Hellenic science to be found in it. The letters which designate the different points on the figures occur in the sequence of the Latin alphabet. The only influence which can be clearly recognized is that of the School of Jordanus. This book is thus obviously a product of the Occidental Middle Ages.

Between the first book of the treatise *De ponderibus*, which now concerns us, and the last three books, there exists only a very loose connection and it is quite easy to disregard that connection. No demonstration in the last three books explicitly refers to a proposition of Book I. Moreover, the two concepts — the concept of *gravitas secundum situm* and the concept of the work of the weight — which play such an essential role in Book I, do not even appear in the last three books. It is clear that the first book, on the one hand, and the last three books, on the other, are two distinct works which were artificially joined together.

These manuscripts, by the way, are not always joined together. A. A. Bjornbö[85] has drawn attention to a manuscript in the Vatican Library, MS Number 3102, in which one finds first of all the nine propositions of the *Elementa Jordani* and secondly, the four theorems of the *De canonio*. These thirteen propositions are then followed by the last three books of the treatise *De ponderibus*, which concerns us here.

One further remark should be made. It seems that Leonardo da Vinci had in his possession a manuscript which was assembled in the above described fashion. Indeed, of the propositions demonstrated in the last three books of the treatise *De ponderibus* there is hardly a single one which has not left a recognizable trace in the notes. It appears, on the contrary, that the demonstrations of Book I were completely unknown to him. For example, his vacillations and gropings, so evident in his research on the inclined plane, could have been avoided if he could have read the elegant solution to this problem discovered by the XIIIth century mechanician.

Thus it appears certain that the treatise *De ponderibus*, which was first believed to be the work of one author, is, in reality, the juxtaposition of two heterogeneous treatises, of which one is a legacy of Greek science while the second has its origin in the Middle Ages.

One must ask, however, if it is due to pure chance that these two works which are so different in origin and character appear together so often.

The works used by the School formed around *De ponderibus* include another example of a juxtaposition of a treatise of Greek origin with a treatise written by a medieval geometer. Beginning in the XIIIth century, one finds the *Elementa Jordani super demonstrationem ponderis* almost always joined with the *De canonio*. The reason for this is obvious. The *De canonio* is incomplete by itself since it refers to propositions which have been demonstrated "by Euclid, Archimedes and others." The demonstration of these propositions forms one of the main themes of the work by Jordanus. Thus Jordanus' work serves as a very natural introduction to the *De canonio*, something which Jordanus himself may have intended.

Could the same reasoning be applied to the juxtaposition of the first book of *De ponderibus* and the last three books?

Like the *De canonio*, the second book of this treatise presupposes the law of the lever and its extension to the case in which the weight of the arms of the lever are considered. The author of the first book demonstrates these propositions exactly as Jordanus had done before him. Thus his first book, just as well as the *Elementa Jordani*, might serve as an introduction to the second book.

This first book, moreover, contributes to the third book in a way in which the *Elementa Jordani* cannot do.

Indeed, if one follows carefully the reasonings by which the first proposition of this third book is demonstrated, one easily recognizes that this line of argumentation presupposes a lemma. This lemma can be formulated in the following way:

If equal weights are suspended from unequal arms of a bent lever, these weights must be at equal distances from the vertical through the point of support, should one wish to have equilibrium.

This proposition must have been known by the geometers of the School of Alexandria. It was quoted by Hero of Alexandria[86] who rightly considered it to be implied in the theorems of Archimedes. But, far from being firmly established in the *Elementa Jordani*, it is actually refuted there. On the contrary, the author of the *De ponderibus* states it precisely in Theorem VIII and establishes it by means of a most elegant demonstration.

Just as Jordanus de Nemore seems to have written the *Elementa* as an introduction to the *De canonio*, his disciple seems to have written the first book of the *De ponderibus* to furnish the last three books with the lemma they needed.

## G. ON A PASSAGE IN THE *TRACTATUS DE CONTINUO** OF THOMAS BRADWARDINE

The free surface of a liquid in equilibrium is a sphere which is concentric to the earth. From this fact the following corollary is derived: A cup filled to the rim contains more liquid when it is closer to the center of the earth than when it is far from it.

This corollary is one of those propositions containing an obvious paradox which the Masters of the School delighted in using to fire the imagination of their students. We first came across it[87] in the *Opus majus* of Roger Bacon. We found it again[88] in one of the XIV Questions of Pierre d'Ailly. Thomas Bradwardine, who wrote after Bacon and before d'Ailly, also makes mention of it.

The Englishman Thomas Bradwardine was born towards the end of

* T.N.: The Latin title reads, *Treatise on the Continuum.*

the XIIIth century in Hartfield near Chichester. In 1325 he was Proctor of the University of Oxford where he taught theology, philosophy, and mathematics. His teachings earned him the surname of "Doctor profundus." He died on August 26, 1349, several days after being nominated to the Archepiscopal Chair at Canterbury.

Several of the mathematical works of Bradwardine had a great influence upon medieval science and were printed at the end of the XVth and the beginning of the XVIth century. Other works remained in manuscript form, such as the *Tractatus de continuo.* We are indebted to Maximilian Curtze for providing us with a description and an analysis of it.[89]

After having shown that the same chord subtends unequal arcs in unequal circumferences and that the smallest arc corresponds to the largest circumference, Bradwardine adds the following:

> If a container is completely filled with a homogeneous liquid, the liquid leaves the extreme rim of the container dry; in a half-filled container, the liquid forms an intumescence above the diameter of the container. If one raises this half-filled container, it becomes gradually fuller with a convex surface extending upward; . . . if lowered, on the contrary, it becomes emptier.

Maximilian Curtze believes that this passage alludes to the properties of capillarity. We do not share this opinion because we believe that Bradwardine is discussing a corollary given earlier by Roger Bacon.

## H. *ON THE PROGRESSION OF THE ELEMENTS* ACCORDING TO THOMAS BRADWARDINE

We have shown[90] how certain commentators on Aristotle supported the following view: Each of the four elements, fire, water, air and earth has a volume which is precisely tenfold the volume of the next element. In the last chapter of his *Tractatus de proportionibus,*[91] Thomas Bradwardine proposes another analogous law which enjoyed great popularity in the Schools.

At the outset, Bradwardine asserts that the four elements completely fill the spherical volume defined by the lunar globe. He further asserts, in agreement with Alfraganus and Thâbit ibn Qurra, that the ratio between the diameter of the earth and the diameter of the moon equals 32.30.[92] Finally, he asserts that the volumes of the four spheres which mark the bounds of earth, water, air and fire form a geometrical progression. From a rather simple calculation as we know it today, Bradwardine, with obvious pride, arrives at the ratios of the volumes of these four spheres as well as the ratios between their radii.

This theory of Bradwardine was obviously well received in the Schools of the Middle Ages. It was discussed with great care and subsequently rejected by Themon Judaeus.[93] It was also discussed[94] by the author of the *Meteorologicorum libri quatuor*, falsely attributed to John Duns Scotus. Around the middle of the XVIth century, it still had its supporters. In 1552, Nonio Marcello Saia refers to it as a doctrine comprehensible to mathematicians only.[95]

## I. ON THE *TREATISE ON METEORS* FALSELY ATTRIBUTED TO JOHN DUNS SCOTUS

We recounted previously[96] how in 1617 Franciscus de Pitigianis had had the *Questions on the Physics of Aristotle* printed, attributing them to John Duns Scotus. Those same *Questions* had already been published for a century under the name of Johann Marsilius of Inghen, who was widely quoted by Scholastic philosophy as the author of the *Questions*.

The Franciscans who published the complete works of Duns Scotus[97] did not commit the error of Franciscus de Pitigianis. It is true that they published[98] the *Quaestiones in libros Physicorum* which de Pitigianis had published, but they added an introduction[99] in which the Reverend Father Wadding showed how untenable it was to attribute these *Questions* to Duns Scotus. Father Wadding clearly saw in them the influence of the Nominalist School of Paris and he pointed to Marsilius of Inghen as a probable author.

Furthermore, Duns Scotus was not only given credit for the *Questions on the Physics*, actually written by Marsilius of Inghen. When the complete edition was published, the apocryphal *Questions on the Four Books of the Meteorology* of Aristotle[100] were further added as being part of that work.

These *Questions on the Four Books of the Meteorology* are also preceded by an introduction by the Reverend Father Wadding.[101]

The erudite Irish Franciscan does not absolutely deny their authenticity. Yet, the arguments which he proposes against their authenticity are convincing and he certainly does little to discredit them.

In the first place, he remarks that the author who quotes[102] Thomas Aquinas calls him *Beatus Thomas*. Now Thomas Aquinas was canonized by John XXII, who became Pope in 1316. John Duns Scotus died in 1308.

In the second place, the work on the *Meteorology* quotes[103] the *Tractatus de proportionibus* of Thomas Bradwardine. However, it seems unlikely that Thomas Bradwardine, who was Proctor at Oxford University in 1325 and who died in 1349, forty-one years after Duns Scotus, could have written the *Tractatus de proportionibus* during the lifetime of the Subtle Doctor.

Father Wadding proposes the hypothesis that the *Questions on the Four Books on Meteorology* could have been written by an English Franciscan, by the name of Simon Tunsted, who died in 1369. Indeed, Pitseus and other Franciscan authors claim that this friar wrote a treatise on this topic which brought him considerable reputation.

A close look at these *Four Books on Meteorology* does not reveal anything which contradicts the hypothesis of Wadding. A reference to England can be found in the Fifth Question of Book I and seems to indicate that the author was English or that he lived in England.

On the other hand, the influence of the *Questions on the Books of the Meteorology*, written by Themon, is evident everywhere in the work falsely attributed to Duns Scotus. The titles are often identical in both works. The reasons given to solve them are frequently the same. They follow in the same order and resemble each other almost word for word. It is clear that the author of the treatise attributed to Duns Scotus had for the most part merely abridged the work of Themon.

Furthermore, it appears that yet another abridgement was written, based on the first abridged version. We know the author of the second abridgement, who is Nicolas Oresme.

Indeed, Heinrich Suter called attention to a manuscript[104] kept in the Library of the Chapter of St. Gallen under Number 839. At the end of the manuscript one can read the following remark:[105]

*Rescriptae sunt hae quaestiones venerabiles Magistri Orem super libros Metheororum Aristotelis Peripotetici* (sic). *Anno Domini 1459 pridie idus mensis Septembris indictione.*

Suter pointed out the obvious similarity between the treatise of

Nicolas Oresme and the one he attributes to John Duns Scotus. The unfortunately limited excerpts which Suter published from the *Questions* of Nicolas Oresme lead us to believe that Oresme summarized the commentary which was to be attributed later to Duns Scotus. We believe that this commentary served as an intermediate work between the *Questions* of Themon and the *Questions* of Nicolas Oresme.

If one considers that Themon the Jew (or more precisely, Themon, the Son of the Jew) wrote his *Questions on the Meteorology* at the University of Paris where he was from 1349 to 1361, and furthermore, if one considers that Nicolas Oresme, born in 1320, became Grand Master of the College of Navarre in 1355, before becoming in 1377 the Bishop in Lisieux where he died in 1385, it seems quite natural to attribute the *Questions on the Meteorology*, which were added to the works of Duns Scotus, to an English author who must have written them around 1360. Simon Tunsted seems to fit this conjecture.

The author turned his attention to the shape of the earth and oceans on two different occasions. It is the Thirteenth Question of Book I which deals with that topic first. The question is devoted to an examination of the view that the volumes of the four elements form a geometrical progression.

The author, who finds much inspiration in the opinions of Themon on this topic, proves that the volume of the ocean is smaller than the volume of the earth. "Otherwise," he states,[106] "the entire earth would be submerged, which is contrary to experience. This can be proved in the following way:"

Let us imagine the earth being outside of its natural locus and water occupying the center of the Universe. Since earth descends, before its center could reach the center of the Universe, it would be completely submerged, assuming that earth is less voluminous than water.

One could assume the earth was situated on one side of the center of the Universe and water was its counterweight on the other side.

However, if this were so, the ocean would get deeper, the further one left its coasts: that is contrary to experience.

Secondly, earth tends naturally to locate below water. Thus water situated on the other side of the center of the Universe could not become a counterweight.

Finally, the aggregate formed by earth and water would not be spherical. This conclusion is incorrect, because we can see during eclipses that the shadow of this aggregate has the shape of a circle. But, on the other hand, the conclusion would result from the premises if water had a larger volume than earth and the latter were partly emerging.

Simon Tunsted combats here the view of Nicolas of Lyre (vide supra, p. 292). Among the arguments to which he objects is one which we have already encountered (vide supra, p. 269) in the commentaries on the *De Caelo* written by John of Jandum.

It is in the first question of his second book that the author returns to this discussion in order to study it more thoroughly. He formulates the question in the following fashion:[107]

Utrum mare semper fluat a septentrione ad austrum.

In the second article of this question he wonders:[108]

Whether the ocean is the natural locus of water.

This prompts him to examine the natural loci for earth and water. Among the problems which he examines, the fourth reads in the following way:

Either water tends towards the same natural locus as earth does or it does not. From the first supposition, it would follow that the center of the earth is the natural locus for both water and earth. From the second supposition it would follow that gravity, throughout the Universe, does not tend towards the same center.

The answer by which Simon Tunsted intends to resolve this difficulty is worth quoting in its entirety because it gives rise to many interesting observations. Here is the quote:

The fourth argument presents us with a great difficulty. In chapter five of his treatise *On the Sphere*, Campanus images that the earth, on our side, is situated above the center of the Universe and that water, on the opposite side, counterbalances the earth.

The gravity of the earth and the gravity of water, therefore, have two distinct centers. He supposed that the earth was initially covered by water. Then, by Divine Command, water united in one place and the firm land masses appeared in order to give man and the other animals a habitable space. The concentration of water in one place is only possible if the earth remains at its center because water would otherwise attempt to cover the firm land. Thus land must have moved from its natural locus. The following are the very words of Campanus. After he has enumerated the position and the orders of the celestial spheres, of fire and earth, he states:

The second sphere is the sphere of water: By Divine Command its surface is interrupted by firm land. The latter emerges by piercing the water's surface according to Divine Command: *Ut congregarentur aquae* . . .[109] Against this supposition, we propound the following basic argument: If it were so, there would exist places on the earth where a given mass of earth and a given mass of water would not fall in the same direction. Such a result is contrary to experience. No matter where a given mass of water is lifted above the ground, it always falls in the same direction as a given land mass placed at the very same point. On the other hand, this result does derive from the above supposition because water would move towards the center of water and earth towards the center of the earth and these two centers would be distinct if the spheres of water and of earth were eccentric.

In the second place, the contour of habitable land would be a circular shape. This conclusion is inadmissible because according to the second book on *Meteorology* of Aristotle, as well as according to several other authors, habitable space distends more from East to West than from North to South. Furthermore, it can be proved that the conclusion derives from the established hypothesis, because the part of the sphere which protrudes from a spherical surface which is eccentric to it, has a circular contour.

Thus, if we abandon this supposition, we must accept that both earth and water are concentric to the Universe, as far as gravity is concerned, that is to say, that the earth and water possess the same center of gravity, without, however, having the same center of magnitude. In order to comprehend this concept, we need to remark at the outset that earth, in its entirety, is not just a simple element. The space which we inhabit is a composite one and thus lighter than pure earth which is situated on the opposite side. This is a fact because digging into the earth has always revealed matter of different nature, such as sand, stones and other composite materials.

Secondly, it needs to be said that if an object with a non-uniform gravity were to fall to the center of the Universe, it would be its center of gravity and not its center of magnitude that would come to rest at the center of the Universe. So much is clear. But let us suppose that at the center of the Universe there is neither earth nor water but only air. If one emptied a glass of water, it would fall to the center where it would gather around that center in the shape of a small sphere of water. If one were to take a long iron nail with a very large head, one would see the following: at the end opposite the head the nail would emerge from the water gathered about the center, but its head would not emerge for reasons which are easily understood.

It follows that the center of gravity of the earth is distinct from its center of magnitude since according to the first supposition, the earth does not possess uniform gravity and the composite part which is situated on our side is the lightest. Thus the part of the earth which is inside of its center of magnitude is not as heavy as the part outside it. Since its center of gravity coincides with the center of the Universe, its center of magnitude must be inside the center of the Universe. Let us pause for a moment and examine this text more closely. Its author expounds here the theory according to which earth and water form two spheres which are eccentric to each other. We followed Giuntini in attributing this theory to Nicolas of Lyre. However, in the text which concerns us here, the theory is attributed quite explicitly to Campanus of Novara and is supported by a quote from Campanus.

The author of the *Meteorologicorum libri quatuor* must surely have had a different version of the *Tractatus de Sphaera* of Campanus from those whch were finally published, because in the published versions,[110] there is no mention of the phrase which Simon Tunsted quotes and the ideas of Campanus appear quite different from those attributed to him by Tunsted. Let us further quote from Chapters IV and V from the *Tractatus de Sphaera*:

*Chapter IV.* On the natural form, locus and order of the elements. The natural locus of the elements as well as the form and order in which they occur are as follows: Imagine that the earth is perfectly spheroidal and that the entire water mass covers it with a spheroidal layer. Imagine further that the spherical layer is enveloped by a spherical layer of the entire air mass and that around this layer there is another spherical layer formed by the entire fire mass. The four elements would thus be completely spherical and concentric. They would have a common center which would be the center of the earth. Such is the description of the natural locus, form and order of the elements.
*Chapter V.* The reason why the sphere of water is not complete. In reality, water does not completely cover the earth with a spherical layer. Creation has mandated that it would not be so in view of the necessity of land which many created beings need to live. Therefore, He who created all things, after having contemplated the natural order which we described above, wished to preordain the elements to his own end and spoke in the following way: Let the waters which are under the heavens be united in one and the same place and let there be firm land. This does not mean that the waters swelled up and assumed the shape of a sphere elevated towards the heavens, but rather that earth, in the place which now appears as protruding from the waters, rose like an island by abandoning its perfect sphericity and by creating a protrusion within the spheroidal surface of water. Indeed, water because of its fluidity, can only be contained by a retaining wall other than itself. On the contrary, land is solid and coherent and can be its own retaining wall. Thus the inequality of which we spoke and which was impossible for water was not so for land. Each heavy body moves towards its center along a path which takes it closest to the center. Consequently, let us suppose that the water rose in the manner indicated above and that it rose above the spheroidal surface proper to it. Nothing could prevent the water which has risen from returning to the spheroidal surface, because when the waters are contained within that surface they are closer to the center than when they have risen above that same surface. The part of land which is visible must have emerged from the water masses, such as islands emerge at different places out of the ocean. Just as these islands are further removed from the center than the surface of the ocean, so are the different parts of firm land further removed from the same center than the different parts of the surface of the waters. Firm land is similar to a large island rising into the air above the surface of the water.

Let us summarize what we have said so far. The surface of the sum total of the water is completely spherical. Its center is the center of the sphere which would be natural to the earth. It is also the center of the other two elementary spheres, the sphere of air and the sphere of fire.

Any comment would only distract from the clarity and precision of these two chapters by Campanus. There is no trace of any influence of the doctrine which Tunsted attributed to this geometer. The text which he used to justify this connection must have been an altered text.

The error of the author of the *Meteorologicorum libri quatuor* spread and as we have indicated,[111] can be found in the *Comments on the Physics* of Gaëtan of Tiène.

Others, such as Johann Marsilius of Inghen[112] and Giovanni Battista Capuano of Manfredonia,[113] attributed to Campanus the doctrine defended by Albert of Saxony. This latter attribution is, however, no more justified than the former. Campanus never made a distinction between the center of magnitude and the center of gravity of the earth.

Although the author of the *Meteorologicorum libri quatuor* quotes the theory of Albert of Saxony without referring to him by name, he does at least pay homage to a physicist who otherwise is completely unknown.

The explanation which he furnished of this theory greatly influenced the teachings in the Schools. In order to explain how the center of gravity of the earth is situated at the center of the Universe while the center of magnitude is situated elsewhere, he proceeds to imagine a long nail with a large head placed at this center. This striking example must have pleased the Scholastic Masters because Johann Marsilius of Inghen uses it when he wishes to explain[114] the doctrine of Albert of Saxony which he nonetheless ends up rejecting.

We have seen[115] that Leonardo, trying to obtain a conception of the doctrine of Albertutius, had imagined that the earth would assume the shape of a tetrahedron. It seems he was the first to have calculated the center of gravity of such a body.

The tetrahedron could have been suggested to Leonardo by the nail analogy used by Simon Tunsted and by Marsilius of Inghen. Although we have already commented upon this, we shall reiterate our observations in view of new light shed upon this matter. In the same notebook in which Leonardo replaced the earth with a tetrahedron and in which he calculated the center of gravity of this figure, there are two passages which remind us immediately of the explanation by Simon Tunsted. These two passages follow:

Water, air and earth.[116] However the earth might move, the surface of water will never move outside of its own sphere, but will always remain equidistant to the center of the Universe. If earth were to move away from the center of the Universe, what would happen to water? Water would remain around this center with equal depth, but with a smaller diameter than when it had the earth within it.

Lead and dewdrops.[117] A well-rounded dewdrop can help illustrate many different aspects of the role played by the sphere of water, such as, how it contains the land mass within itself without destroying the sphericity of its surface. At the outset, let us take a cube of lead which is the size of a millet grain. After we attach a very fine thread to it and submerge it in this drop, we shall see that this drop does not lose its original sphericity, even though it has increased in size by the volume taken up by the cube of lead which is now inside the dewdrop.

At the beginning of Notebook F, Leonardo tells us that he had at his disposal a treatise on *Meteors*. We have attempted[118] to prove that this treatise was none other than the collection of *Questions* composed by Themon Judaeus. Our previous remarks on Tunsted could put into doubt the accuracy of that attempt. It could lead the reader to believe that Leonardo had not read the *Quaestiones in libros meteororum* of Themon but the *Quatuor libri meteorologicorum* of Simon Tunsted.

The similarity between these two treatises is often very striking. Thus at first it seems to be difficult to decide which treatise Leonardo had used. However, we believe that a closer examination can resolve this question.

Perhaps Leonardo had in his possession the treatise on meteors which was later attributed to Duns Scotus. However, many ideas expressed in the notebooks of Leonardo could not have come from that work. They could have only been suggested to him by a reading of the *Questions* of Themon.

For example, the first book of the treatise attributed to Simon Tunsted ends with two questions (the XXVth and the XXVIth) which read as follows:[119]

Utrum fontes et fluvii generentur ex aqua pluviali, congregata in visceribus terrae? — Utrum in concavitatibus terrae generetur aqua fontium ex aëre evaporato?

Likewise, the first book of the work of Themon ends with the following question (the XXth):[120]

Utrum aquae fontium et aquae fluviales generentur in concavitatibus terrae?

As far as the origin of springs is concerned, both authors defend the same doctrine, which happens to derive from Albertus Magnus. Yet, in explaining this theory, Themon resorts to the example of distillation which occurs in a still, while neither Albertus Magnus nor Simon Tunsted make use of this comparison. Leonardo, however, insisted on this analogy.

Likewise, Leonardo argued fervently in favor of the theory of the solar tide, formulated by Themon in the first two questions of his second book. However, in his discussion on the ebb and flow of the tide in the second question of his second book, Simon Tunsted makes no mention of this theory. He merely borrows from Robert Grosseteste the explanation of tides by the influence of the moon.

Thus it appears possible that Leonardo had read the *Meteorologicorum libri quatuor* which the XVIIth century was to attribute to John Duns Scotus despite all evidence to the contrary. What is certain is that Leonardo had read and meditated upon a work which influenced John Duns Scotus, the *Quaestiones in libros meteororum* of Themon Judaeus.

## J. THE INFLUENCE OF ALBERT OF SAXONY AND NICOLAS ORESME

The theory of gravity formulated by Albert of Saxony and the conclusions which he deduced from its as far as the relative configuration of land and water is concerned, continued to exert a great influence on the opinions of philosophers and physicists up to the end of the XVIth century. This same influence was already apparent among the Masters at the University of Paris who were contemporaries of Albert of Saxony. Nicolas Oresme is our example.

Nicolas Oresme was born in Normandy around 1320 and died in 1382. In 1355, at the time when the teachings of Albert of Saxony exerted their greatest influence, Nicolas Oresme became Grand Master of the College of Navarre. As we know, he was also the tutor of the Dauphin, the future Charles V, before he became Bishop of Lisieux in 1377.

This universal mind, this great mathematician to whom we owe our first notions on coordinates, has also left us a short treatise on astronomy,[121] written in French and designed to teach the principles of this science to those whom the XVIIth century was to call "les honnêtes gens" and which our century would call "les gens du monde."

In a *Prologue to the Reader*, Oresme emphasized the purpose of his book:

The shape and the configuration of the world, he states, the number and the order of the elements, as well as the motion of the celestial bodies, are the concern of every man of free condition and of noble station. And it is a beautiful, delightful, profitable, and honest thing . . . what I wish to say in plain French should be known by every man, without delving overly into demonstrations and subtleties which are the concern of astronomers.

In Chapter I of the *Treatise on the Sphere* which bears as its title: "On the shape of the Universe and of its main parts," we read the following:

After earth comes water or the ocean, but water does not cover all of the earth, because none of the parts of the earth is equal in weight. For example, we can see that tin weighs less than lead. Therefore, the lighter part is higher and further away from the center and not covered by water so that the animals are able to live on it. This is, as it were, the face of the earth, entirely uncovered, except that here and there one finds several small seas, arms of the sea and rivers. The entire remainder thus resembles a head wearing a band which is the great ocean.

Thus, thanks to Nicolas Oresme, before the end of the XIVth century, several corollaries of the doctrines of Albert of Saxony had ceased to belong solely to the "astronomers" and had entered the domain of all men "of free condition and of noble station."

## K. ON SEVERAL PASSAGES FROM THE *XIV QUESTIONS* OF PIERRE D'AILLY

In Chapter XV (Vol II, p. 297), we mentioned the important work of Pierre d'Ailly which is entitled: *In sphaeram Johannis de Sacro Bosco subtilissimae XIV quaestiones.*[122] Our attention had focused, in particular, on the fifth of these fourteen questions, which reads as follows: *Quaeritur utrum caelum et quatuor elementa sint sphaerica?*[123] There we saw the celebrated Cardinal restate almost verbatim entire passages from Albert of Saxony. In particular, he borrowed the following crucial remark from the Master:

One can test the sphericity of the earth in the following way: Let a man travelling upon the surface of the earth, starting at a given point and moving southwards, measure by how much the height of the pole has changed and measure the distance which he has covered. Then let the man continue his trip until the height of the pole has undergone a second variation equal to the first one. If the second distance covered is equal to the first, the earth must necessarily be spheroidal.

This remark, which contains the seed of an entire theory of geodesy, seems to have its origin in Albert of Saxony (Vide supra, Vol. II, p. 287). The *XIV Questions* most certainly contributed at least as much if not more than the *Questions on the De Caelo* of the same author, to the popularity of that theory among astronomers.

On one essential point, however, Pierre d'Ailly diverges from the teachings of Albertutius or at least his essential doctrine. In Question V, Pierre d'Ailly asks if the earth is in the middle of the firmament. Concerning this question he states that: "these words can be understood in four different ways. They can mean:

(1) That the center of the firmament coincides with the center of magnitude of the earth.
(2) That it coincides with the center of gravity of the earth.
(3) That it coincides with the center of gravity of a given aggregate to which the earth belongs.
(4) That the earth is completely surrounded by the firmament.

"These remarks," the famous Bishop of Cambrai adds, "call for conclusions."

*First conclusion.* The center of gravity of the earth does not coincide with its center of magnitude because the earth does not have a uniform gravity. Indeed, the part not covered by water and upon which the sun shines becomes lighter due to solar heat. On the contrary, the part covered by water becomes heavier due to the frigidity of water.
*Second conclusion.* The center of gravity of the earth is not in the middle of the firmament. This is obvious. If one were to divide the earth into two parts with equal gravity, the half which is partially covered by water and partially surrounded by water would repel the other half until the center of gravity of the entire aggregate were at the center of the universe.
*Third conclusion.* The Earth does not have a center of magnitude situated at the center of the firmament, because in that case it would be entirely covered by water . . . Thus we have to imagine three distinct centers within the earth: The first one is the center of magnitude, the second the center of gravity and the third the center of the firmament. From this we conclude that the earth cannot be said to occupy the center of the firmament, either in the first or the second sense. It does not occupy this center either with its center of magnitude or with its center of gravity.
*Fourth conclusion.* The center of gravity of the aggregate formed by land and by water occupies the center of the firmament. This is obvious, because the aggregate forms a

heavy body unimpeded in its motion. It moves until its center of gravity occupies the center of the universe, according to the nature of a heavy body. Consequently, since the center of gravity of the aggregate formed by land and water is situated at the center of the universe, it follows from our preliminary remarks that this aggregate can be said to occupy the center of the universe. Secondly, according to the third sense of these words, the earth can be said to occupy the center of the firmament since it is part of an aggregate which is situated at the center of the universe. The same is true for water.
*Last conclusion.* Land and water can be said to occupy the center of the universe, according to the fourth interpretation of these words.

In these passages, Pierre d'Ailly clearly favors the doctrine which Albert had stated in his *Questions on Physics* (vide supra, Vol. II, p. 275), but had then rejected in his *Questions on the Heavens* (vide supra, Vol. II, p. 276). Themon was tempted to subscribe to this same opinion (vide supra, Vol. II, p. 291). We have seen (Chapter XV, Section 7) what role this doctrine, taken up again by Mauro of Florence, played during the scientific discussions of the XVIth century. Pierre d'Ailly and the reputation of his *XIV Questions* probably contributed greatly to the rage which the doctrine, strangely enough, continued to enjoy for such a long period.

There is yet another point on which the doctrine of Pierre d'Ailly diverges from the teachings of Albert of Saxony. In the same fifth question, we read the following:

One can express a doubt: Does this aggregate of land and water which naturally rests at the center of the universe, have any actual gravity? This doubt can be answered in the affirmative, at least with high probability. One can convince oneself of this with the following argument: outside of its natural locus, this aggregate would actively be heavy. However, it does not lose this quality by returning to its natural locus. It continues to possess actual gravity within this locus. Nothing would be gained by objecting that this gravity pulls neither upwards nor downwards. Yet, nothing is more certain than that gravity remains and that it actively exerts its function of gravity. The following argument is proof: If the aggregate formed by land and water were not actively heavy, a small fly would be capable of moving it. Such a conclusion is unacceptable and yet, it can logically be deduced from the premises. The fly does, indeed, have a given power for pushing or pulling. The aggregate, on the contrary, would offer no resistance to this impetus if gravity were not active. ... We need to realize, therefore, that gravity and lightness have two functions. The first of these two functions consists in moving the body from its location when this body is situated outside of its natural locus. The second function is designed to keep and maintain this body in its place once it has reached it. No matter which of these two functions is being exerted, gravity or lightness must be said to be active. Our aggregate of land and water is, therefore, actively heavy.

The difference in opinion between Pierre d'Ailly and Albert of Saxony is essentially a question of semantics. What the latter calls *habitual* or *potential* gravity, the former considers, illogically enough, to be an active gravity. However, this has no bearing as far as the essential ideas of both authors are concerned.

The agreement between them is complete concerning the continual motion which gravity must impart to earth. The Cardinal, Pierre d'Ailly, expresses this in the following way in Question Three: *Quaeritur utrum motus primi mobilis ab oriente in occidentem circa terram sit uniformis?*[124]

At the outset, we must assume that the center of gravity of the earth is permanently situated at the center of the universe. This is true. Indeed, every heavy body tends towards the center of the universe. Therefore, the heaviest of all bodies must have its center situated at the center of the universe. Secondly, if we imagine the earth to be divided into two halves of equal weight by a plane which contained the center of the universe, these two halves would interact like two weights held in equilibrium. If one were to add a small weight to one of the halves, it would move downwards by pushing the other half upwards. Thirdly, if the earth were divided into two halves of equal volume, these two halves would not weigh the same. Indeed, the part of the earth which is continually exposed to the sun would warm up and become lighter through solar heat. The other half, which is continually submerged, becomes heavier due to the cold water. Thus the part of the earth which is not submerged is lighter than the other half. Finally, one recognizes that the parts which are detached from the land are continually being carried to the ocean by water. One further recognizes that certain parts of the land, transformed into dust by dry air, are transported by winds and end up falling into the ocean.

Once these suppositions are made, one can state this initial conclusion: Each part of the earth is continually moving in a straight line. Indeed, one half of the earth continues to become heavier than the other. Thus, according to our first two suppositions, one half pushes the other half, with the result that the part of the earth which is at the center at a given time, will be at the surface at another time.

Everything quoted thus far has been borrowed from Albert of Saxony, with the exception of the remark that erosion causes the center of gravity of the earth to shift continually.

Quotes from Marsilius of Inghen (vide supra, Vol. II, p. 295) showed us that the treatises on statics produced by the School of Jordanus were not unknown in the universities during the second half of the XIVth century. Thanks to the evidence from Pierre d'Ailly, we can see that they were also known at the turn of the XIVth century.

In the first of his *Fourteen Questions*, the Bishop of Cambrai is led to divide mathematical sciences into five main divisions: geometry, arithmetic, music, perspective and astrology. In oppostion to this division, criticisms are made and he in turn examines these. Specifically, one can ask:

> To which sciences do certain short treatises belong, such as the treatise *De ponderibus* or the treatise *De speculis*?
>
> The answer is that the treatise *De ponderibus* belongs to astrology and the treatise *De speculis* belongs to perspective.

Thus, through the statements given by Roger Bacon, Albert of Saxony, Marsilius of Inghen, Pierre d'Ailly and even by the treatise of Blasius of Parma, we come to realize that statics occupied a special place within Medieval science for several centuries. This *Scientia de ponderibus* remained somewhat outside the main current of physical science. The *Tractatus de ponderibus* was not among the treatises taught in the universities and its proponents were not to be found among the Masters of the Faculty of Arts. They were referred to collectively as the "Auctores de ponderibus"[125] and their works were attributed to Euclid or to Jordanus by the scribes. However, the ideas contained in these works were neither ignored nor denigrated by the Scholastic doctors.

## L. ON THE *TRACTATUS DE PONDERIBUS* OF BLASIUS OF PARMA

We stated earlier (Vol. I, p. 108) that in his *Tractatus de ponderibus*, Blasius of Parma had formulated the following proposition:

> If a balance with two equal arms, from which are suspended two equal weights, is removed from the center of the universe, these weights seem all the heavier the further the balance is removed from the center of the universe.

Pelacani proves this proposition by stating that the direction along which each of the weights tends to fall forms with the vertical line passing through the point of support on the beam an angle which is all the more acute the further removed the balance is from the center of the universe.

The history of this proposition as it pertains to the history of statics is worth mentioning. In their discussion with Fermat (Vol. II, p. 370) Roberval and Étienne Pascal used very similar reasoning. Descartes attempted to demonstrate in his own way (Vol. II, p. 376) the very same proposition of Blasius of Parma and Mersenne restated (Vol. II, p. 383) the reasoning used by the great philosopher.

We quoted (Vol. II, p. 297) a passage by Albert of Saxony in which, in an attack on a view of Roger Bacon, the Master-of-Arts at the University of Paris, seems to lay the groundwork for the proposition which Blasius of Parma was to formulate some time later.

We can go even further by stating that Albert of Saxony not only anticipated the theorem which we are discussing here, but that the actually knew it and had gotten it from someone else. He himself attributes it to those "Auctores de ponderibus" whose names remain unknown and whose works are collectively attributed to Jordanus. Indeed, in one of the *Questions*[126] of Albert of Saxony on the *De Caelo et Mundo* we read the following:

The "Auctores de ponderibus" say that the further a body is from the center, the heavier it is "secundum situm."

This passage is a part of a question which plays a capital role in the history of dynamics. In this question Albertutius analyzes the reason why natural motion is an accelerated motion. After stating and discussing the different answers proposed to that question, he accepts the view which attributes this acceleration to an increase in *impetus*. He is then led to consider the law of inertia and its application to the conservation of celestial motions. On the other hand, he attempts to discover the law which governs the increase in velocity of a falling body. He asks if it is proportional to the time needed or to the distance traversed. However, he rejects these two laws which would cause the velocity to increase without limit and at the same time increase the length of the fall. He decides upon a third law according to which the velocity tends towards a finite limit. He seems to imply that the resistance offered by the medium is the reason for which he chose the latter solution over the two former ones.

By a strange coincidence, George Lokert left out this all important question in the two editions of the *Quaestiones in libros de Caelo et Mundo*, published in Paris in 1516 and in 1518.

Nonetheless, the passage above plays an important role in the history of statics. It shows that the proposition of Blasius of Parma was already contained in the treatise *De ponderibus*, written before 1350. However, it is nowhere to be found in any of the works of the School of Jordanus we were able to consult or in any description of them. These works thus do not comprise the entire collection of works attributed to the "Auctores de ponderibus."

## M. *ON THE SHAPE OF THE EARTH AND THE OCEANS* ACCORDING TO GIOVANNI BATTISTA CAPUANO OF MANFREDONIA

We have already quoted from (Vol. II, p. 298) the commentary on the treatise *On the sphere* of John of Sacrobosco, written by Giovanni Battista Capuano of Manfredonia. We commented on the undeniable influence of this author on the doctrines of Albert of Saxony concerning the center of gravity. In the present note, we would again like to dwell upon several passages in this commentary.

At the outset, we need to assign a more recent date to the composition of this work than we had previously assumed. When Giovanni Battista Capuano mentions the spherical shape of the shadow of the earth during a lunar eclipse, he is referring to the eclipse which he observed on August 15, 1505. Thus his commentary must postdate that event.

Capuano seems to have had a critical and paradoxical mind preoccupied with finding objections, sometimes rather strange ones, to the arguments of his predecessors. Sacrobosco, for example, as so many before him, proves the sphericity of the ocean by remarking that a signal sent from a coast cannot be seen from the ship's bridge, although it can be seen from the top of its mast. Capuano questions the validity of this observation supporting the sphericity of the ocean. He explains it by the presence of fog on the ocean's surface.

We can also find him denying that the circular shape of the shadow which eclipses the moon proves anything at all about the shape of the oceans. Indeed, he maintains that water does not reflect a shadow. Alexander Piccolomini was to restate this strange view. While he admits that lunar eclipses prove the sphericity of firm land, which has a center coincident to the center of the universe, he considers the surface of the water also to be obviously spherical, but much greater than the surface of the land. According to Capuano, no one should believe those who maintain[127] that water exists only in small quantities, that it is divided into masses found in the valleys and the depressions of the land, that the aggregate of land and water obviously form a single sphere with its center situated at the center of the universe.

Along with Nicolas of Lyre and Gregory Reisch, Giovanni Battista Capuano of Manfredonia must be considered a partisan of the curious doctrine of which Mauro of Florence, Antonio Berga and Agostino Michele are the most ardent defenders.

One last remark. After having explained the theory which assumes a center of gravity within the earth distinct from a center of magnitude, and after having used this theory to explain the existence of underwater continents, Capuano adds the following words: *Haec causa attribuitur Campano.*[128] We have stated that nothing in the *Commentary* of Campanus on the treatise *On the sphere* by Sacrobosco, justifies this assertion. Therefore, if Campanus were truly the author of this doctrine, he must have explained it in some work unknown to us and — even harder to believe — he must have omitted it completely from his *Commentary.*

Yet, there is more. In the *Commentary* of Capuano, the analysis of this doctrine about two centers of the earth is preceded by the following lines:

> Given the fact that water does not cover all of the parts of land . . . one traditionally assumes multiple efficient causes, as the Conciliator states in the first article of the 13th Difference. The following is the first of these causes.

Thus it seems that Capuano has borrowed the explanation of the theory of two centers from Peter of Abano, who was called the "Conciliator of Differences."[129] Since the latter was born in 1250 and died in 1316, we must conclude that the theory about the two centers had assumed its final form long before the time of Albert of Saxony. We were unable to scrutinize the famous work of Peter of Abano: *Conciliator philosophorum et praecipue medicorum.*[130] Therefore, we were unable to form a final judgment. But if the distinction between the center of magnitude and the center of gravity of the earth had already been formally stated in the work of Peter of Abano, it would be surprising to see that no other author we consulted and who wrote prior to Albert of Saxony, had made any mention of this theory. In particular, it would be surprising to find that such a theory was unknown to John of Jandun, who taught at the University of Padua several years after Peter of Abano.[131] It seems more likely that Peter of Abano had merely expounded the doctrine of Campanus and that

Capuano replaced this doctrine with the doctrines of Albert of Saxony, without recognizing what the latter doctrine added to the former.

### N. ON THE THEORY OF THE INCLINED PLANE AS CONCEIVED BY LEONARDO DA VINCI

On several occasions we have discussed[132] the curious demonstration of the law of the inclined plane as conceived by Leonardo da Vinci. This demonstration, which resembles the argumentation of Pappus, consists of analyzing the power which causes a disk or a sphere to roll down an inclined plane. Furthermore, we quoted[133] a passage from Albert of Saxony which contains the germ of the principle of this demonstration. Moreover, we hesitated to affirm that Leonardo da Vinci was inspired by this passage. However, it does indeed come from the *Questions on the Physics of Aristotle* written by Albert of Saxony. Even though we learn from his own account that Leonardo had in his possession the *Tractatus proportionum* as well as the *Quaestiones in libros de Caelo* by Albertutius, and even though his notes are filled with numerous references to these two works, nothing shows us that the great painter knew of the *Quaestiones in libros Physicorum.*[134]

We have found elsewhere a passage which might have suggested to da Vinci his theory of the inclined plane. This passage can be found in the treatise *De distributionibus ac de proportione motum*[135] of Alexander Achillini, a famous professor from Bologna.

Achillini objects to a stated rule, with the following device:[136]

Two balls of equal weight are put into contact with two plane surfaces. One of these balls touches a plane surface which forms a right angle with the earth. I assume the surface of the earth to be plane upon which the ball falls vertically. The second ball touches another plane surface which forms an acute angle with the earth.

Achillini makes the following observation about this device:[137]

The vertical plane along which the ball would vertically descend does not prevent the descent. On the contrary, the plane which is not vertical prevents the descent and imparts a rotational motion to the ball. Consequently, the plane closer to being vertical offers less of an obstacle to the motion than the plane further from being vertical. It offers less of an obstacle to the motion, but it imparts a faster rotational motion to the ball than another plane forming with the earth an angle more remote from a right angle.

The affinity between the idea expressed in this passage and the principle of the theory of Leonardo cannot be disregarded. What makes this parallel so interesting is that Leonardo must have read the *De proportione motuum* of Achillini. By his own account[138] he had borrowed it from Fazio Cardano, father of the well-known Jerome Cardan: *Le proporzioni d'Alchino colle considerazioni di Marliano da Meser Fazio.*[139]

## O. LEONARDO'S DISCOVERY OF THE LAW OF THE COMPOSITION OF CONCURRENT FORCES

In Section 2 of Chapter VIII,[140] we have shown how Leonardo was the first to give a very elegant solution to the problem of the composition of forces. However, it seemed to us that this great genius did not fully realize his discovery at that time and that he soon returned to an erroneous solution to the problem. We saw in this a further proof of the inconsistency and indecisiveness so often reproached in this great genius.

In this case, the accusation turned out to be unjustified. It disappears as soon as one assumes that Notebook E, like several other account books left by Leonardo, was not only written from left to right but also backwards, in a sequence opposite to the pagination given. We know for a fact that this is so, at least in the part of Notebook E in which Leonardo discovers the Law of the Composition of Concurrent Forces.

Indeed, from sheet 69 to sheet 71, all the notes pertaining to this memorable invention can be found. The two following facts prove beyond a doubt that in order to follow the thought of Leonardo, one must read this part of the notebook backwards.

At the end of sheet 61 verso, Leonardo, unable to complete a line of reasoning, writes: "Turn the page." At the top of the same page recto, we read: "This continues what is missing earlier at the bottom."

On sheet 77 verso, a crossed-out passage is followed by the following note, apparently put there as an afterthought: "This is expressed better on the third page after this one." We have to go to sheet 75 recto in order to find a new version of the same passage. There it is preceded by these words: "This completes what is missing on the third page before this one."

These remarks oblige us to read Notebook E in the opposite direction to its page numbers. Thus, if we turn our attention to the important problem raised by the Law of Composition of Concurrent Forces, we see the genius of Leonardo gradually move from error to a clearer perception of the truth and once discovered, stay with that truth.

In another work[141] we have shown how reading the treatise *De ponderibus*, written by his Precursor, had led Leonardo to reflect upon the composition of concurrent forces.

## P. ON THE SHAPE OF THE EARTH AND THE OCEANS ACCORDING TO JEAN FERNEL

We have seen (Vol. II, p. 295) that Marsilius of Inghen had refused to accept the doctrine according to which the surface of the oceans forms a uniformly spherical surface which has as its center the center of the Universe. He preferred the following view to the doctrine described above: water forms a definite number of isolated masses which are contained within the cavities of solid land.

This opinion was doubtlessly shared at the beginning of the XVIth century by many astronomers and physicists. Among these was the French astronomer and physician Jean Fernel (1497—1558), who was the first modern thinker to attempt to measure the arc of terrestial meridian of one degree. He chanced upon the correct result by a procedure which was far from being precise.

In the treatise on astronomy[142] in which he explains his geodesy, Jean Fernel first analyzes the configuration of land and the oceans.[143] He furnishes a very precise summary of the theory of Albert of Saxony, who, according to Fernel, is in favor with the modern philosophers (*philosophi juniores*). Fernel reminds the reader that according to this theory the earth has two distinct centers, one of magnitude and the other of gravity. Furthermore, the latter is at the center of the Universe, while the former is at a considerable distance from that center, since the immersed part of land is weighed down by its humidity, while the uncovered part is continually dried by the sun.

Our author does not accept this doctrine. According to a view which he attributes to Aristotle, he claims that the surface of land and the surface of oceans form a more or less spherical surface. Is it not true that the lands and the numerous islands discovered by navigators in the most diverse regions are proof enough that the surface of land is never much further from the center than the surface of the oceans? We should, therefore, imagine the earth as a wooden ball in which certain cavities have been carved out and admit that water has filled these cavities.

If one were to draw a plane through the center of the Universe, this plane would cut the earth into two halves. These two halves might not have exactly the same volume — one half might have more cavities than the other half — but both halves would have the same weight. Indeed, the half containing cavities filled with water would be weighed down by humidity and by the weight of the water. This fact would have to be taken into account.

The Earth arranged in this way remains absolutely immobile. Thus Fernel rejects the opinion of our philosophers, "according to whom and contrary to Aristotle the earth could move away from its center."

It is clear that Fernel does not believe that the volume of water is greater than the volume of land. As a matter of fact, he sharply attacks[144] this view as well as the other which ranks the volumes of the elements in a geometric progression.

## Q. ON THE SHAPE OF EARTH AND THE OCEANS ACCORDING TO MELANCHTHON

Philip Melanchthon is known as one of the first to have attacked the system of Copernicus in the name of theology. However, in the book as well as in the chapter[145] in which he attacked the heliocentric system, Melanchthon accepts without reservation the doctrine formulated by Copernicus as far as the configuration of land and water is concerned. He expresses his view in the following way:

> The reader must be warned here that the aggregate of land and water must be considered as a single globe which in reality forms an integral whole. Even though many people distinguish between a center of magnitude and a center of gavity, there is, in reality, only one center, which is both the center of magnitude and of gravity. The continent which was recently discovered is proof that land is not completely surrounded by ocean, as the Ancients believed. Nor is it true to say that the sphere of water is ten times larger than the sphere of land. They assumed this because they believed that a given volume of land could engender ten similar volumes of water, because they believed these spheres to be in the ratio of the cubes of their diameters.

## R. TARTAGLIA

Moritz Cantor and R. Marcolongo have done me the favor, for which I now thank them, of bringing to my attention the existence of various documents on the subject of Nicolo Tartaglia. These documents were unknown to me or had not been published when I gave my account of this geometer in Volume I (pp. 138—139).

Prince Boncompagni, who found the last will and testament of Tartaglia, proved[146] that Tartaglia died on December 14, 1557.

At the International Congress of the Historical Sciences, held in Rome in 1903, Vincenzo Tonni-Bazza presented a paper[147] which contains a good deal of hitherto unpublished information about the life and work of Tartaglia.

## S. ON THE ORTHOGRAPHY OF THE NAME OF GUIDOBALDO DAL MONTE

We have consistently referred to the benefactor of Galileo as Guido Ubaldo del Monte. We followed the example of Pigafetta, who published during the lifetime of the Marquis del Monte an Italian translation of his book on mechanics. But, as we said before (Vol. I, p. 148):

> Other authors spell his name differently. In particular, Favaro writes: Guidobaldo dal Monte.

Favaro was kind enough to send us some very interesting information on this subject. We obtained permission from the very learned editor of the works of Galileo to include that information here.

> Most authors when reading the title, Guidi Ubaldi e Marchionibus Montis which appears at the beginning of this work, believed Guido to be the Christian name and Ubaldi the family name. But the Christian name (a traditional name in the family Del Monte) is Guidobaldo, divided in two parts in the Latin translation. All you need do to confirm this, is to look at the signature at the end of the letters in the 10th volume of my edition. The facsimilie (p. 38) shows: "Guidobaldi d Marchesi d'Monte." Elsewhere, he signs either "Guidobaldo de Marchesi de Monte" or "Guidobaldo dal Monte" (Cf. pp. 39, 41, 43, 45, 47). Consequently, it is not I but he himself who used the name "Guidobaldo dal Monte" and I can not see how one can write his name in any other way.

# FOOTNOTES

## FOOTNOTES TO THE FOREWORD

[1] A full discussion of Duhem's Catholicism, the continual target of misguided and at times openly hostile appraisals, is the subject of my most recent book, *Pierre Duhem: Homme de foi et de science* (Paris: Beauchesne, 1991).
[2] See ch. 10, "The Historian," in my *Uneasy Genius: The Life and Work of Pierre Duhem* (2nd. ed.: Dordrecht: Martinus Nijhoff, 1987).
[3] See Illustration 138 in my *The Physicist as Artist: The Landscapes of Pierre Duhem* (Edinburgh: Scottish Academic Press, 1988).
[4] A preliminary report of that shocking story was given in my "Science and Censorship: Hélène Duhem and the Publication of the *Système du monde*," *Intercollegiate Review* 12 (Winter 1985—86), pp. 41—47. A full treatment will be given in the book I am now writing, "Reluctant Heroine: The Life and Work of Hélène Duhem."

## FOOTNOTES TO THE TRANSLATORS' INTRODUCTION

[1] Cf. Volume I, Chapter VII of the *Origins of Statics*.
[2] Cf. Moody and Clagett, *The Medieval Science of Weights*, pp. 151ff and 293ff.
[3] Mach, E. *Science of Mechanics*, Open Court Publishing Company, Sixth American Edition, 1960, p. 59.
[4] Clagett, Marshall, *The Science of Mechanics in the Middle Ages*, The University of Wisconsin Press, Madison, 1959, p. 150. This quote is taken from the *Mechanics* of Galileo.
[5] According to the article on Varignon in the *Dictionary of Scientific Biography*, it is Lagrange who discovered and called attention to this letter in the works of Varignon. *Dictionary of Scientific Biography*, Charles Scribner's and Sons, New York, 1973.

# FOOTNOTES TO THE ORIGINS OF STATICS (VOLUME I)

## FOOTNOTES TO CHAPTER I

[1] T. N.: This work is now commonly attributed to either Theophrastus (372?—287? B.C.) or Strato (?—271 B.C.).
[2] T. N.: This is Duhem's translation from the original Greek, using the French edition of Aristotle's *On the Heavens*, Edition Didot, Vol. II, p. 414. Cf. the English edition: Aristotle, *On the Heavens*, Loeb Edition, III, ii, 301b, pp. 278—9.
[3] T. N.: This is Duhem's translation from the original Greek, using the French edition

of Aristotle's *Mechanical Problems*, Edition Didot, Vol. IV, p. 58. Cf. the English edition: Aristotle, *Mechanical Problems*, Loeb Editon, 3, 850b, pp. 352—3.

[4] T. N.: This is Duhem's translation from the original Greek, using the French edition of Aristotle's *Mechanical Problems*, Edition Didot Vol. IV, p. 55. Cf. the English edition: Aristotle, *Mechanical Problems*, Loeb Edition, 1, 848a, pp. 334—5.

[5] At one time it was fashionable to consider as useless the science of Aristotle and his commentators. This prejudice sufficed to render incomprehensible many extremely important intellectual achievements. Thus in an otherwise admirable opening chapter of the *Mécanique analytique*, in which Lagrange gives us a historical perspective, one finds the following written about the Principle of Virtual Velocities: "For one who examines the conditions of equilibrium of levers and other machines, it is easy to recognize this law stating that the weight and the force are always in inverse ratio to the distances which both can travel through in the same time. Yet, it appears that the Ancients never understood this law. Guido Ubaldo was perhaps the first to recognize it in the lever and pulley blocks."

[6] Aristotle, *Mechanical Problems*, Edition Didot, Vol. IV, 1, 848a, p. 55.

[7] *Oeuvres d'Archimèdes*, translated literally with a commentary by F. Peyrard, Paris 1807, p. 275.

[8] Loc. cit., pp. 280—2.

[9] T. N.: The words "commensurable" and "incommensurable" denoted to the Greeks what we call today rational and irrational numbers.

[10] Descartes, Letter to Mersenne dated November 15, 1638 (*Oeuvres de Descartes*, published by Ch. Adam and P. Tannery, Vol. II, p. 433).

[11] T. N.: Latin for "how things are" but not "why they are that way."

## FOOTNOTES TO CHAPTER II

[1] Libri, *Histoire des Sciences mathématiques en Italie*, from the Renaissance of Letters to the End of the 17th century, Vol. III, p. 11, Paris, 1840.

[2] T. N.: The Italian title reads, *Treatise on Painting.*

[3] T. N.: The Italian title reads, *Treatise on the Motion and Measure of Water.*

[4] The detailed history of these manuscripts can be found at the beginning of the first volume of the handsome edition by Charles Ravaisson-Mollien: *Les Manuscrits de Léonard de Vinci*, Paris, A. Quantin, 1881.

[5] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Paris, A. Quantin, Vol. I (1881): Ms. A from the Bibliothèque de l'Institut; Vol. II (1883): Ms. B of the Bibliothèque de l'Institut; Vol. III (1888): Mss. C, E and K of the Bibliothèque de l'Institut; Vol. IV (1889): Mss. F and I of the Bibliothèque de l'Institut; Vol. V (1890): Mss. G, L and M of the Bibliothèque de l'Institut; Vol. VI (1891): Ms. H of the Bibliothèque de l'Institut, and Mss. No. 2037 and No. 2038 in Italian from the Bibliothèque Nationale (Acq. 8070, Libri).

[6] *I Manoscritti di Leonardo da Vinci Codice sul volo degli uccelli e varie altre materie* (T. N.: The Italian reads, "Codex on the flight of birds and several other topics"), published by Teodoro Sabachnikoff; transcription and notes by Giovanni Piumati: translated into French by Charles Ravaisson-Mollien; Paris, Edouard Rouveyre, editor, 1893.

[7] T. N.: The meaning of the French term is vague. In technical terms, it probably refers to a hydraulic jump.
[8] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. E. of the Bibliothèque de l'Institut, folio 8, verso Paris, 1888. (T. N.: The French translation of this passage by Ravaisson-Mollien is imprecise and should read, "Mechanics is the paradise of mathematical science because with it one attains the rewards of mathematics.")
[9] Venturi, *Essai sur les ouvrages de Léonard de Vinci*, Paris, 1797.
[10] Venturi, Loc. cit., pp. 17 and 18.
[11] Libri, *Histoire des Sciences mathématiques en Italie*, from the Renaissance of Letters to the End of the 17th Century, Vol. III, pp. 10—60, Paris, 1840.
[12] Ravaisson, Félix, *La Philosophie en France au XIX siècle*, p. 5; *Recueil de Rapports sur les progrès des lettres et des sciences*, 1868.
[13] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. F of the Bibliothèque de l'Institut, folio 26, recto. Paris, 1889.
[14] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. F of the Bibliothèque de l'Institut, folio 51, verso. Paris, 1889.
[15] T. N.: Duhem's text contains an obvious error here. ". . . half the space" should read "twice the space." Compare proposition one.
[16] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. A of the Bibliothèque de l'Institut, folio 45, recto. Paris, 1881.
[17] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. E of the Bibliothèque de l'Institut, folio 58, verso. Paris, 1888.
[18] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. A of the Bibliothèque de l'Institut, folio 33, verso; entitled "The Capacity of the Force to Push and Pull," Paris 1881.
[19] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. E of the Bibliothèque de l'Institut, folio 20, recto. Paris, 1883.
[20] T. N.: A "brasse" is approximately five feet.
[21] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. A of the Bibliothèque de l'Institut, folio 30, recto. Paris, 1881.
[22] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. I of the Bibliothèque de l'Institut, folio 14, verso. Paris, 1889.
[23] T. N.: Although Duhem uses the term "resistance," "moment" would be more appropriate in modern usage.
[24] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. E of the Bibliothèque de l'Institut, folio 72, verso. Paris, 1883.
[25] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. E of the Bibliothèque de l'Institut, folio 64, recto. Paris, 1888.
[26] The text reads erroneously, "are *not* inclined."
[27] That is to say, "closer to the vertical."
[28] Léonard de Vinci, ibid., folio 65, verso.
[29] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. I of the Bibliothèque de l'Institut, folio 30, recto. Paris, 1889.
[30] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. M of the Bibliothèque de l'Institut, folio 40, recto. Paris, 1890.

[31] T. N.: From the Latin "circumvolvo," meaning to turn around or rotate.
[32] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. M of the Bibliothèque de l'Institut, folio 50, recto and verso. Paris, 1890.
[33] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. A of the Bibliothèque de l'Institut, folio 52, recto. Paris, 1881.
[34] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. G of the Bibliothèque de l'Institut, folio 75, recto. Paris, 1890. Cf. Ibid., folio 76, verso.
[35] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. G of the Bibliothèque de l'Institut, folio 76, verso; folio 77, recto. Paris, 1890.
[36] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. E of the Bibliothèque de l'Institut, folio 6, recto. Paris, 1888.
[37] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. G of the Bibliothèque de l'Institut, folio 39, verso. Paris, 1890.
[38] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. E of the Bibliothèque de l'Institut, folio 1, verso. Paris, 1888.
[39] *I Manoscritti di Leonardo da Vinci*, Codice sul volo degli uccelli e varie altre materie; Published by Teodoro Sabachnikoff; Transcription and notes by Giovanni Piumati; translated into French by Charles Ravaisson-Mollien; Paris, Edouard Rouveyre, editor, 1893, folio 4, recto. (T. N.: The Italian reads, Codex on the Flight of Birds and Various Other Topics.)
[40] T. N.: In order to demonstrate this claim, consider the equilibrium of point $o$ where the component forces due to weights $p$ and $q$ are horizontal.

## FOOTNOTES TO CHAPTER III

[1] M. E. Wohlwill expressed the opinion quite casually and unemphatically that Tartaglia and Cardan might have been influenced directly or indirectly by Leonardo da Vinci. Cf. E. Wohlwill, *Die Entdeckung des Beharrungsgesetzes* (*Zeitschrift für Völkerpsychologie und Sprachwissenschaft*, Vol. XIV, p. 386, in the footnote; 1883).
[2] T. N.: Duhem tried very hard to establish an uninterrupted filiation throughout the development of science. He refused to admit that a researcher could independently make the same discovery which had already been made in the work of a predecessor. He has been rightly criticized by historians for this predilection.
[3] T. N.: Duhem is referring here to the Parisian Schools of Buridan and Jordanus. The latter is discussed in Chapters VI and VII.
[4] Libri, *Histoire des Sciences mathématiques en Italie*, Vol. III, P. 33, Paris, 1840.
[5] Pacioli, *De divina proportione*, folio I, Venice, 1509.
[6] Vasari, *Vite. . .*, vol. VII, p. 57, Florence, 1550.
[7] Lomazzo, *Trattato della pittura*, p. 652. Milan, 1585, *Idea del tempio della pittura*, p. 17 and p. 106. Milan, 1590. (T. N.: The Italian titles read: *Treatise on Painting and Ideas on the Temple of Painting.*)
[8] Cf. on the subject, Libri, *Histoire des Sciences mathématiques en Italie*, Vol. III, pp. 148ff. Paris, 1840.
[9] T. N.: The Italian and Latin read, "I swear to you, by the Holy Gospel of God and

as a true gentleman, that I will not only never publish your solution if you teach it to me . . ."

[10] Hieronymi Cardani medici Mediolanensis, *De Subtilitate libri XXI.* Ad illustrissimum Principem Ferrandum Gonzagam, Mediolanensis provinciae praefectum. Lugduni, apud Guglielmum Rouillium, sub Scuto Veneto, in-8°, 1551. (T. N.: The Latin title reads, *Twenty-one Books On Subtlety* by the Milanese physician Jerome Cardan. To the illustrious Prince Ferdinand Gonzaga, Governor of the Province of Milan. Lyon, Guillaume Rouille, under the Seal of Venice, in octavo, 1551.)

[11] This edition is only known to me by the reference made to it by Cardan in his *Apology*, which was appended to the Basel edition in 1560.

[12] The Books of Jerome Cardan, Milanese physician, entitled *On Subtlety and Subtle Inventions*, including their occult causes and reasons, translated from Latin into French by Richard Le Blanc, published in Paris, in quarto by Charles l'Angelier in 1556.

[13] In 1557, the first edition of *On Subtlety* had been sharply criticised in: Julii Caesaris Scaligeri exotericarum exercitationum Liber XV; De Subtilitate ad Cardanum, Lutetiae, apud Vascosanum, 1557, in quarto. (T. N.: The Latin title reads, *Book XV of Exoteric Exercises On Subtlety* to Cardan by Julius Caesar Scaliger. Paris, Vascosanus 1557, in quarto.) To the criticism addressed to him by Julius Caesar Scaliger, Cardan replied in 1560 in the *Apology* which closes the following edition: Hieronymi Cardani, Mediolanensis medici, *De Subtilitate* libri XXI, ab authore plus quam mille locis illustrati, nonnulli etiam cum additionibus. Addita insuper Apologia adversus calumniatorem, qua vis horum librorum aperitur. Basileae, ex officina Petrina. Anno MDLX, Mense Martio, in octavo. (T. N.: The Latin title reads, *Twenty-one Books on Subtlety* by the Milanese physician Jerome Cardan, with more than a thousand illustrations by the author including numerous additions and a defense against his calumniator, in which the virtue of these books is demonstrated. Basel, Petrina, March 1560, in octavo.) Besides the editions just cited, we also found in the Municipal Library and in the University Library of Bordeaux: First, two other Latin editions of the *De Subtilitate* by Cardan, Nuremberg, Petreius, 1560, in-folio and Lyon, Stephen Michel, 1580, in octavo. Second, three other editions of the *Books on Subtlety*, translated into French by Richard Le Blanc: Paris, Lenoir, 1556 (in-4°); Paris, Lenoir, 1566 (in-8°). Third, three other editions of the *Exercitationes* by Scaliger: Francofurti, apud Claudium Marnium et haeredes Joannis Aubrii, 1607 (in octavo); Francofurti, apud A. Wechelum, 1612 (in octavo); Lugduni, apud A. de Harsy, 1615 (in octavo). (T. N.: The Latin titles read, Frankfurt, Claudius Marnius and the heirs of Johann Aubrius, 1607 (in octavo); Frankfurt, A. Wechelus, 1612 (in octavo); Lyon, A. de Harsy, 1615 (in octavo)). This enumeration alone clearly demonstrates the extraordinary popularity enjoyed by Cardan's work.

[14] Hieronymi Cardani Mediolanensis, civisque Bononiensis philosophi, medici et mathematici clarissimi, *Opus novum de proportionibus* numerorum, motuum, ponderum, sonorum aliarumque rerum mensurandarum, non solum geometrico more stabilitum, sed etiam variis experimentis et observationibus rerum in natura, solerti demonstratione illustratum, ad multiplices usus accommodatum, et in V libros digestum . . . Basileae, ex officina Henricpetrina, Anno Salutis MDLXX, Mense Martio. (T. N.: The Latin title reads, *A New Work on the Proportions* of numbers, motions, weights, sounds and other measurable phenomena, suitable for diverse purposes, not only established by a geometrical method, but also illustrated with various experiments and observations of natural phenomena, and with a skillful demonstration condensed into five books by the

eminent philosopher, physician, and mathematician, a citizen of Bologna, the Milanese Jerome Cardan. Basel, Henricpetrina, March 1570.)

[15] Cardan, *The Books on Subtlety*, translated from Latin into French by Richard Le Blanc. Paris, l'Angelier, 1556, p. 32.

[16] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. A of the Bibliothèque de l'Institut, folio 20, recto. Paris, 1881.

[17] *cd* should be understood as the surface of the luminous image formed inside the focal plane of the mirror.

[18] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. G of the Bibliothèque de l'Institut, folio 89, verso. Paris, 1890.

[19] T. N.: The text reads "pyramid" but the word "cone" would be more appropriate if one assumes a spherically concave mirror as opposed to a cylindrical mirror.

[20] Cardan, *The Books on Subtlety*, translated from Latin into French by Richard Le Blanc, Paris, l'Angelier, 1556, p. 83.

[21] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. F of the Bibliothèque de l'Institut, folio 67, verso. Paris, 1889.

[22] T. N.: The construction of an unequal arm balance called a "statera" by the Romans has a unique ratio between the short and long arms. The weight that goes on the short arm is called in the vernacular of Paris a "sledge".

[23] Cardan, *De Subtilitate*, Book I, first edition, p. 31.

[24] Cardan, *The Books on Subtlety*, translated from Latin into French by Richard Le Blanc, Paris, l'Angelier, 1556, p. 17.

[25] Cardan, *Opus novum*, Proposition XCII, Basilae, 1570, p. 84.

[26] Cardan, *Opus novum*, loc. cit.

[27] T. N.: The Latin reads. "This is something, he says, Archimedes left unattempted, although it is very necessary and, moreover, he demonstrates more abstruse things than useful ones, I would say, in due deference to him."

[28] Cardan, *The Books on Subtlety*, translated from the Latin into French by Richard Le Blanc, Paris, l'Angelier, 1556, pp. 16 and 17. (T. N.: The first book of the original Latin work has been translated into English by Myrtle Marguerite Cass as a dissertation for the degree of Doctor of Philosophy at Columbia University. The following is her rendering of Cardan's own definition of subtilitas/subtlety:

> Now *subtilitas* is a certain intellectual process whereby sensible things are perceived with the senses and intelligible things are comprehended by the intellect, but with difficulty.)

[29] T. N.: Here and below, Cardan is referring to the center of the earth and not to the center of the balance.

[30] T. N.: Although Duhem uses the term "liaisons," i.e., "connections," modern usage would require "constraints."

[31] Cardan, *Opus novum*, Propositio XCVIII. Basileae, 1570, p. 92.

[32] In the *Opus novum*, written during his old age, Cardan sometimes seems to forget the transformation which he had given to Aristotle's axiom and refers to it in its initial form. For example, the theory of the lever (a) is expounded by a reasoning analogous to the one found in the *Mechanical Problems*. Furthermore, the influence of this latter work is noticeable throughout the *Opus novum*, where Cardan makes many references to the treatise of the Stagirite. (a) Cardan, *Opus novum*, Propositio XLV: Rationem

staterae ostendere, Basileae, 1570, p. 34. (T. N.: Latin for Proposition XLV, To Demonstrate the System of the Roman Balance.)

[33] Cardan, *The Books on Subtlety*, translated from Latin into French by Richard Le Blanc, Paris, l'Angelier, 1556, p. 333 (Book XVII, On the Arts and Artful Inventions. A Method to Easily Lift Loads).

[34] The translator says: "Concerning screws, as in presses." He adds a little later: "Some call them block and tackle." Cardan uses the term "trochleis". (T. N.: "trochleis" is the Greek word for pulley. Neither the text nor the figure illustrating it leave any doubt that Cardan means a block and tackle.)

[35] T. N.: Cardan also disregards the weight of the bottom pulley and any other attachments.

[36] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms. A of the Bibliothèque de l'Institut, folio 35, verso. Paris, 1881.

[37] Cardan, *The Books on Subtlety*, translated from Latin into French by Richard Le Blanc, Paris, l'Angelier, 1556, p. 333.

[38] Cardan, *Opus novum*, Propositio LXXII: Proportionem levitatis ponderis per virgam torcularem attracti ad rectam suspensionem invenire. Basileae, 1570, p. 63. (T. N.: The Latin reads, To determine the ratio of the weight of a heavy sphere to its ascent (sic) along an inclined plane.)

[39] Cardan, *The Books on Subtlety*, translated into French by Richard Le Blanc, Paris, l'Angelier, 1556, p. 334.

[40] Cardan, *Opus novum*, Propositio LXXII: Proportionem ponderis sphaerae pendentis ad ascensum per acclive planum invenire. Basileae, 1570, p. 63. (T. N.: The Latin reads, To determine the ratio of the weight of a heavy sphere to its ascent (sic) along an inclined plane.)

[41] We will not bother to translate this obscure part of the sentence.

[42] Libri, *Histoire des Sciences mathématiques en Italie*, Vol. III, p. 174, Paris, 1840, wrote the following: "In his Paralipomenes, Cardan for the first time stated the parallelogram of forces for the case where the components act at right angles (*Cardani Opera*, Vol. 10, p. 516). Lagrange attributes this proposition to Stevin." — I have been unable to verify this statement by Libri. But it would be imprudent to accept the statements by this author without any verification. Too often he read ancient texts rather superficially and in the hope of finding in these texts modern ideas not yet conceived when these texts were written. Thus, for example (loc. cit. p. 41), on the subject of Leonardo da Vinci's manuscripts he claims that "within them is developed with great accuracy the theory of the inclined plane." We have seen what one should think of such a statement. Even if Libri's statement about Cardan's Paralipomenes were correct, it is certain that Stevin, who knew the *Opus novum* when he wrote his statics could not have known the other work.

## FOOTNOTES TO CHAPTER IV

[1] T. N.: Leibnitz called the product of mass by velocity squared ($mv^2$) the living force. This quantity is identical to kinetic energy except for the factor 1/2.

[2] *Les Manuscrits de Léonard de Vinci*, published by Ch.Ravaisson-Mollien, Ms. A of the Bibliothèque de l'Institut, folio 21, verso. Paris, 1881.
[3] T. N.: The Latin reads, "No violent motion can be perpetual."
[4] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A of the Bibliothèque de l'Institut, folio 34, verso. Paris, 1881.
[5] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A of the Bibliothèque de l'Institut, folio 35, verso. Paris, 1881.
[6] Leonardo knew of the accelerated fall of heavy bodies and he discussed this at length in several passages: among others, Ms. M of the Bibliothèque de l'Institut.
[7] Here, too, Leonardo only develops what was taught in the School. "Motus simplex terminatur ad quietem," (T. N.: The Latin reads, "Simple motion ends in rest.") was the saying in the School.
[8] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E of the Bibliothèque de l'Institut, folio 20, recto. Paris, 1888. Cf. Ms. E, folio 58, verso.; Ms. G, folio 81, recto., and folio 82, recto. Paris, 1890.
[9] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A of the Bibliothèque de l'Institut, folio 22, verso. Paris, 1881.
[10] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E of the Bibliothèque de l'Institut, folio 21, recto. Paris, 1888.
[11] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A of the Bibliothèque de l'Institut, folio 22, verso. Paris, 1881.
[12] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A of the Bibliothèque de l'Institut, folio 22, verso. Paris, 1881.
[13] Cardan, *Les Livres de la Subtilité*, translated from Latin into French by Richard Le Blanc, Paris, l'Angelier, 1556, p. 339. The quotes which follow have been translated directly from the Latin text and are not from Richard Le Blanc's translation which proves to be very obscure in this passage.
[14] It should be pointed out that Cardan avoids deciding whether perpetual motion could possibly be produced with the help of a magnet. In Cardan's time, the very strange properties of magnets preoccupied to a unique degree all those hoping to create a *perpetuum mobile*. In 1558, Achilles Grasser used one of the numerous manuscript copies in circulation among physicists to print for the first time in Augsburg the famous document written by Pierre de Maricourt (Petrus Peregrinus) in Charles of Anjou's camp before the battle of Lucera on August 8, 1269. In his essay (a), Pierre de Maricourt, after having established the laws of magnetic actions in the fashion of the true logician versed in experimental methods, attempts to produce a *perpetuum mobile* with the aid of magnets.

(a) Petri Peregrini Maricurtensis, *De magnete, seu rota perepetui mobilis libellus.* Divi Ferdinandi Rhomanorum imperatoris auspicio per Achillem P. Grasserum L. num primum promulgatus Augsburgi in Suevis, Anno Salutis 1558. (T. N.: Pierre de Maricourt, *Manual on the Magnet, or the Wheel of Perpetual Motion*, first printed in Augsburg, Swabia by Achilles Grasser in the year of our salvation 1558, under the auspices of the divine Roman Emperor Ferdinand.) This work has been reprinted in, *Neudrücke von Schriften und Karten über Meterologie und Erdmagnetismus*, edited by G. Hellman. No. 10, *Rara Magnetica*, Berlin, 1896.
[15] Cardan intends to exclude the movements in the heavens, which are perpetual by nature.

[16] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A of the Bibliothèque de l'Institut, folio 35, recto. Paris, 1881.
[17] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. C of the Bibliothèque de l'Institut, folio 6, verso. Paris, 1888.
[18] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. F of the Bibliothèque de l'Institut, folio 27, recto; folio 26, verso, and folio 30, verso. Paris, 1889.
[19] Cardan, *Les Livres de la Subtilité* translated from Latin to French by Richard Le Blanc, Paris, l'Angelier, 1556, pp. 12 and 13. — This passage is not in the first edition of the *De Subtilitate* but it was added in the second edition.
[20] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. F of the Bibliothèque de l'Institut, folio 84, recto. Paris, 1889.

## NOTES TO CHAPTER V

[1] Dr. Woepcke, *Notice sur des traductions arabes de deux ouvrages perdus d'Euclide: Journal Asiatique*, 4th series, Vol. XVIII, p. 217, 1851. (T. N.: The French title reads, *Note on the Arabic Translations of Two Lost Works by Euclid.*)
[2] T. N.: The French title reads, *The Book of Euclid on the Balance.*
[3] T. N.: The Beni Mouca or the Banu Musa, as the name is sometimes transcribed, were a family who devoted their lives to mathematics and translation.
[4] Maximilian Curtze, *Das angebliche Werk des Euklides über die Waage*, Zeitschrift für Mathematik und Physik, XIXth, 1874, p. 263. (T. N.: The German title reads, *The Alleged Work of Euclid on the Balance.*)
[5] Heiberg, *Literargeschichtliche Studien über Euklid*, Leipzig, 1882, p. 11. (T. N.: The German title reads, *Literary and Historical Studies on Euclid.*)
[6] Steinschneider, *Intorno al Liber Karastonis*, Letter from Dr. Baldassare Boncompagni, *Annali di Matematica*, Vol. V, p. 54, 1863. (T. N.: The Italian reads, *About the Liber Karastonis.*)
[7] Cf. Heiberg, *Literargeschichtliche Studien über Euklid*, p. 10.
[8] Maximilian Curtze, *Zwei Beiträge zur Geschichte der Physik*, (Bibliotheca Mathematica, 3rd edition, Vol. I, p. 51, 1900.)
[9] T. N.: The Latin title reads, *The Book of Euclid on the Heavy and Light and on the Relation of Bodies to one Another.*
[10] Aristotle, *The Physics*, Book 7.
[11] T. N.: The Latin title reads, *Jordanus' Treatise on Heaviness.*
[12] T. N.: The Latin title reads, *Here begins Euclid's Book on the Relative Heaviness and Lightness of Bodies.*
[13] Bibliothèque Nationale, Ms. 10260 (Latin collection).
[14] T. N.: The Latin reads, "This is the end because nothing more can be found."
[15] We hope to be able to publish in the near future the text of these propositions as well as the diverse unpublished texts discussed in this chapter.
[16] T. N.: The Latin reads, "In this volume are contained the following books, in chapters and with figures."

[17] T. N.: The Latin reads, "Here begin the *Elements of Jordanus on the Demonstration of Weights* with charts and figures. Here begin excerpts from *Thâbit on Weights*. Here begins the *Book of Euclid on Weights* according to the circumference described by the extremities. *Divinations. On Time-Reckoning.*

[18] Bibliothèque Nationale, Ms. 16649 (Latin collection).

[19] T. N.: The Latin reads, Master François Guillebon of Paris, a Fellow of the Sorbonne and Doctor of Theology.

[20] T. N.: The Latin reads, *The Book of the Philosopher Arsamides on the measure of the circle.*

[21] T. N.: The Latin reads, "Here ends the Book of Arsamides. Written in 1519."

[22] T. N.: Cf. footnote # 17.

[23] Bibliothèque Nationale, Ms. 11247 (Latin Collection).

[24] Montucla, *Histoire des Mathématiques*, Paris, Year VII, Vol. I, p. 217.

[25] I was able to find this work in five compilations belonging to the Latin collection of the Bibliothèque Nationale as Mass. 7310, 7377B, 7434, 8680A, 10260. Steinschneider (a) found another copy in Ms. 184 of the Library of the Convent of Saint Mark in Florence and published its beginning and end. Maximilian Cürtze (b) pointed out the existence of the same work in two other manuscripts located in the Vatican Library: the Ms. Regina Suecorum 1233 and Ms. 2975. He found it again in Ms. R. No 402 of the Library of the Thorn gymnasium. He published the formulations of the theorems based on this last copy. We intend to provide an edition of the complete treatise.

(a) Steinschneider, *Intorno al Liber Karastonis*, Lettera a D. Baldassare Boncompagni (Annali de Matematica, Vol. V, 1863, p. 54).

(b) Maximilian Cürtze, *Über die Handschrift R. 4°2*, Problematum Euclidis explicatio des Königl. Gymnasial Bibliothek zu Thorn (*Zeitschrift für Mathematik und Physik* XIIIter Jahrgang. Supplément, p. 45, 1868).

[26] T. N.: The Latin title reads, *The Book of Karaston, edited by Thâbit, the Son of Cora.*

[27] Wuestenfeld, *Geschichte der Arabischen Aerzte und Naturforscher*, Sr. 29, No. 71; Göttingen, 1840. — Moritz Cantor, *Vorlesungen über die Geschichte der Mathematik*, Bd. 1, p. 603; Leipzig, 1880.

[28] B. Boncompagni, *Della vita e delle opere di Gherardo Cremonese*, Rome, 1851. (T. N.: The Italian title reads, *On the Life and Work of Gerard of Cremona.*)

[29] Steinschneider, loc. cit.

[30] T. N.: The Latin reads, "Eratosthenes is written in the title. But, at the beginning of the book the author is named differently by the man by whom the book was translated and toward the end, he is expressly called Karaston."

[31] T. N.: In his *History of Mechanics* René Dugas believes like Duhem that the Greek geometer Charistion , a contemporary of Philo of Byzantium in the second century B.C., is the author.

[32] Steinschneider, *Intorno al Liber Karastonis.* Lettera a D. Baldassare Boncompagni (*Annal di Matematica*, Vol. V, 1863, p. 54).

[33] Heiberg, *Literargeschichtliche Studien über Euklid*, Leipzig, 1882, p. 11.

[34] Maximilian Curtze, "Das angebliche Werk des Euklides über die Waage" (*Zeitschrift für Mathematik und Physik*, XIXth Jahrgang, p. 263, 1874.)

[35] *Montucla*, Vol. I, p. 314; Paris Year VII.

[36] T. N.: Cf. footnote 39 below.
[37] J. Graesse, *Trésor de livres rares et précieux*, Vol. V.
[38] Geschichte der Künste und Wissenschaften seit der Wiederherstellung derselben bis an das Ende des achtzehnten Jahrhunderts. VIIts Abtheilung: *Geschichte der Mathematik*. von A. G. Kastner, Bd. II, p. 688; 1797.
[39] T. N.: The Latin title reads, *A Book on Diverse Matters* of Astronomy by Ptolemy, which he dedicated to his son Heriston, dealing compendiously with diverse matters contained in the table at the beginning of that book, 1508. Edited by Peter Liechtenstein.
[40] T. N.: The Latin reads, Here ends the *Book on Diverse Matters* by Ptolemy of Alexandria, illustrious Prince of Geometers. 1509. Venice. Ed. by Peter Liechtenstein of Cologne, Germany.
[41] Steinschneider, *Hebraic Bibliography*, Vol. VII, p. 92, 1864. Cf. Moritz Cantor, *History of Mathematics*, Vol. I, p. 604; Leipzig, 1880.
[42] Bailly, *Dictionnaire grec-français*, Paris, 1895.
[43] *Simplicii in Aristotelis Physicorum libros quatuor posteriores commentaria*; Commentaria in Physicorum VII, 5 (Edition Diels, Berlin, 1895, p. 1110). (T. N.: The Latin title reads, *Simplicius, Commentaries on the Last Four Books of Aristotle's Physics*, Commentary on the Physics, VII, 5.)
[44] Bailly, *Dictionnaire grec-français*, Paris, 1895.
[45] T. N.: The usual English translation is, "Give me a place to stand and I will move the earth."
[46] Tzetzes, *On the Millennia* (Corpus Poetarum Graecorum, Vol. II, Geneva 1614) — Tzetzes lived in Constantinople from 1120 to ca. 1180.
[47] Pappi Alexandrini, *Collectionis quae supersunt*, edidit F. Hultsch. Lib. VIII, Propos. XI, p. 1060; Berlin. 1888.
[48] A. J. Vincent, *Géométrie pratique des Grecs* (Notices et Extraits des Manuscrits de la Bibliothèque Impériale, Vol. XIX, 2nd partie, p. 330) — Carra de Vaux, *Les Mécaniques ou l'Elévateur de Héron d'Alexandrie*, published for the first time based on the Arab version of Qustâ ibn Lûkâ. Book I, art. 1, Paris, 1894.
[49] *Mathematicorum Hypomnematum de Statica*, conscriptus a Simone Stevino brugensi, Liber III, *De Staticae praxi*, p. 101. (T. N.: The Latin title reads, *The Mathematical Memoires on Statics* of Simon Stevin of Brugge. Book III, *On the Practice of Statics*, p. 101.)
[50] T. N.: The Latin reads, "Moreover, this chapter is based upon the work called *The Book of Euclid*." There is some disagreement between translations of this Latin phrase. In the *Science of Weights* on pp. 88 and 89, Moody and Clagett's translation implies that the *Liber Karastonis* is physically "joined" to the *Liber Euclidis* while Duhem's translation says that the *Liber Karastonis* is "based upon" the *Liber Euclidis*. Duhem's rendering is truer to the Latin.
[51] Thurot, "Recherches historiques sur le Principe d'Archimède" (*Revue Archéologique*, nouvelle série, Vol. XIX, 1869, p. 117.)
[52] The treatise by the pseudo-Archimedes might also be a fragment of the *On Weights*.
[53] Bibliothèque Nationale, Ms. 16649 (Latin collection).
[54] T. N.: Neither term is a legitimate Latin word.
[55] And even "tetragonium" in the 13th century text, No. 3642 of the Bibliothèque Mazarine.

[56] T. N.: The Latin reads, "It has been shown in the books which speak about these things that there is no difference whether a weight *db* is uniformly distributed along the whole line *db* or whether it is suspended at a point in the middle of the interval."
[57] T. N.: The Latin reads, "It has been demonstrated thus by Euclid, Archimedes and others and this is the point around which everything else turns."

## FOOTNOTES TO CHAPTER VI

[1] Montucla, *Histoire des Mathématiques*, Vol. I, p. 506; Paris, an VII.
[2] Chasles, "Histoire de l'Algèbre. Sur l'époque où l'Algèbre a été introduite en Europe." (*Comptes Rendus*, Sept. 6, 1845, Vol. XIII, p. 507).
[3] In hoc opere contenta: Jordani Nemorarii *arithmetica decem libris demonstrata; Musica libris demonstrata quatuor*, per Jacob. Fabrum Stapul.; *Epitome in libros arithmeticos* divi Severini Boetii; *Ritmachie ludus* qui et pugna numerorum appellatur. Parisiis. Jo. Higman et Volg. Hopil. 1496, in-folio, Gothic 72 ff. (T. N.: The Latin reads, Contained in this work: *Arithmetic demonstrated in 10 books by Jordanus de Nemore; The Art of Music Demonstrated in 4 Books* by Jacob Faber Stapulensis: *Abstract for the Mathematical Books* of the Divine Severinus Boetius: *The Game of "Rithmomachie"* which is also called the "Battle of Numbers." Paris, 1496 in Gothic folio. pp. 71dd.) (Cf. Graesse, *Trésor de livres rares et précieux*, vol. III—J. Ch. Brunet, *Manuel du libraire et de l'amateur de livres*, p. 566).
[4] Regiomontanus, *Oratio in proelectiones Afragani*, Norimbergae, 1537, in-4°. (T. N.: The Latin title reads, *A Speech against the lectures of Afraganus*, Nuremberg, 1537, in quarto.) Cf. Chasles, "*Histoire de l'Algèbre*. Sur l'époque où l'Algèbre a été introduite en Europe," (Comptes Rendus, Vol. XIII, 1845, p. 507).
[5] T. N.: The Latin reads, "Jordanus published three very fine books on the properties of numbers."
[6] D. Francisci Maurolyci, *Opuscula mathematica*, Venetiis, 1575. Index lucubrationum. (N.T.: The Latin title reads, Francesco Maurolico, *Treatise on Mathematics*, Venice, 1575. With an Index of Erudite Works.) This list is reproduced in Libri, *Histoire des Sciences mathématiques en Italie*, Paris, 1840, Vol. III, p. 243.
[7] Chasles, *Histoire de l'Algèbre*. Note sur la nature des opérations algébriques (dont la connaissance a été attribuée à tort à Fibonacci). Des droits de Viète méconnus (Comptes Rendus, 5 Mai 1841, Vol. XII, p. 743).
[8] Treuttlein, *Zeitschrift für Mathematik und Physik*, Vol. XXIV, Supplément, pp. 135 and 136, 1879.
[9] M. Curtze, *Zeitschrift für Mathematik und Physik*, Vol. XXXVI, Histor. litterar. Abtheilung, pp. 1, 41, 81, 121; 1891.
[10] Cf. on this subject Treuttlein, *Zeitschrift für Mathematik und Physik*, Vol. XXIV, Supplément, p. 132; 1879. — Only recently was the attribution of the *Algorithmus demonstratus* to Jordanus questioned by M. G. Eneström in a work entitled, "Ist Jordanus Nemorarius Verfasser der Schrift 'Algorithmus demonstratus?" (Bibliotheca mathematica, 3 Folge, Vol. V, p. 9; 1904).
[11] Maximilian Curtze, *Jordani Nemorarii de triangulis libri quatuor* (*Mittheilung des Copernicus-Vereins für Wissenschaft und Kunst zu Thorn*, 1887, Heft VI.). (T. N.: The Latin reads, *Four Books on Triangles* by Jordanus Nemorarius.)

[12] Chasles, *Aperçu historique*, p. 516 — Weidler, *Historia Astronomiae*, 1741, p. 276.
[13] Heilbronner, *Historia matheseos universae*, 1742, p. 604. — Chasles, *Aperçu historique*, p. 517.
[14] Moritz Cantor, *Vorlesungen über die Geschichte der Mathematik*, Vol. II, p. 54, 1892.
[15] T. N.: The Latin reads, "Here begins the Treatise of Jordanus on Mirrors with a Commentary on the same."
[16] T. N.: The Latin reads, "Here ends the Book on Mirrors — Here begins the Elements of Jordanus on Weights."
[17] T. N.: The Latinized Greek reads, "a private person."
[18] Daunou, *Histoire littéraire de la France*, Vol. XVIII, p. 140, "Art. Jourdain le Forestier."
[19] T. N.: The Latin title reads, *A Chronology of Famous Mathematicians.*
[20] Cf. on this subject, Moritz Cantor, *Vorlesungen über die Geschichte der Mathematik*, Vol. II, p. 599.
[21] T. N.: The Latin title reads, *The Third Work.*
[22] Chasles, *Histoire de l'Algèbre.* "Note sur la nature des opérations algébriques (dont la connaissance a été attribuée à tort à Fibonacci). — Des droits de Viète méconnus" (*Comptes Rendus*, May 5, 1841, Vol. XII, p. 743).
[23] Libri, *Histoire des Sciences mathématiques en Italie*, Vol. IV, p. 490; 1841.
[24] Chasles, *Histoire de l'Algèbre. Sur l'époque où l'Algèbre a été introduite en Europe*" (*Comptes Rendus*, Sept. 6, 1841, Vol. XIII, p. 107).
[25] T. N.: The Latin title reads, The Book on Weights by the Illustrious Jordanus Nemorarius.
[26] These two quotes can be found on the verso of the eighteenth sheet (including the title) of the work, which has no pagination.
[27] Treuttlein, *Zeitschrift für Mathematik und Physik. Supplement zur historisch-litterarischen Abtheilung des XXIV Jahrganges. Abhandlungen zur Geschichte der Mathematik*, 1879, p. 125.
[28] Maximilian Curtze, *Mittheilung des Copernicus-Vereins für Wissenschaft und Kunst zu Thorn*, 1887, Heft VI.
[29] Id., ibid. (T. N.: The Latin title reads, *Four Books on Triangles by Jordanus Nemorarius.*)
[30] T. N.: "Nemorarius" perhaps derives from the Latin word for forest, "nemus, (gen.) nemoris."
[31] T. N.: Duhem probably means the city of Hildesheim near Dassel.
[32] T. N.: The Latin title reads, *Mathematical Delights.*
[33] R.P. Denifle, a letter addressed to Maximilian Curtze and included by the latter in his work.
[34] Moritz Cantor, *Vorlesungen über die Geschichte der Mathematik*, Vol. II, p. 53.
[35] Bibliothèque Nationale (Latin collection): No. 16644 (XIIIth century) *Jordani de Nemore Arismetica* — No. 7364 (XIVth century) *Jordani de Nemore Elementorum Arismetice distinctiones decem* — No. 16198 (XIV century) *Jordani de Nemore Elementorum Arismetice* — No. 14737 (XV century) *Jordani de Nemore Elementa Arismetice.* (T. N.: The Latin title reads, *The Elements of Arithmetic of Jordanus de Nemore.*)

[36] M. Curtze, "Die angebliche Werke des Euklides über die Waage" (*Zeitschrift für Mathematik und Physik*, XIX. Jahrgang, p. 263, 1874).
[37] Nemus, the Latin form for "Nemi." Cf. De Vit, *Totius latinitatis onomasticon*, Prati, MDCCCLXXXLVII, Vol. IV, p. 651.
[38] T. N.: "Moment" in this context is to be understood, in the manner of Archimedes, as the product of force and perpendicular distance to the pivot point.
[39] T. N.: This is Duhem's translation of the Greek original. Cf. Aristotle's *Minor Works, Mechanical Problems*, 848b, 11.
[40] Aristotle, *Mechanical Problems*, III.
[41] T. N.: Due to the excess weight of the lever to the right of the point of support there exists a restoring moment.
[42] T. N.: In this case, the unbalanced moment will cause the lever to rotate about point A (Fig. 19) until it assumes the configuration shown in Figure 18, if the rotation is unimpeded.
[43] Bibliothèque Nationale (Latin collection), No. 10252.
[44] T. N.: The Latin title reads, *Algorism of Integers* by John of Sacrobosco. Finished in Naples, by Arnold of Brussels, Feb. 11, 1476, before sunrise.
[45] Bibliothèque Nationale, Latin collection, No. 10267.
[46] T. N.: The Latin reads, "The End. Completed April 1468. Here end the rules for the tables of the illustrious mathematician and Doctor of Arts, John of Blanchine, a soldier in the service of the most efficacious benefactor, the illustrious Borso, Duke of Modena and Reggio, Count of Rovigo, Marquis of Este and Ferrara, completed by Arnold of Brussels, of the Duchy of Brabant, in April 1468 in the city of Naples."
[47] T. N.: The Latin reads, Brussels, at 40° of latitude.
[48] De Saint-Genois, *Biographie Belge*, 1866.
[49] Bibliothèque Nationale, Latin collection, No. 11247.
[50] Bibliothèque Mazarine, No. 3642 (formerly 1258).
[51] Bibliothèque Nationale, Latin collection, No. 16649.
[52] T. N.: The Latin reads, "We have made this construction in the Preambles."
[53] T. N.: The Latin reads, "As was stated in the Filotegni. So we have stated in the Filotegni."
[54] We should note here that one of the references to the *Filotegni* is in a fragment of the 13th century kept in the Bibliothèque Mazarine. The copy of this text which belonged to François Guillebon reads, *Philotegne*.
[55] T. N.: The Latin reads, "This has been demonstrated in books dealing with these matters."
[56] We called attention to the fact that the translator of the *De canonio* replaced in his figures the Greek letter eta, with an *i*. In the same way, Jordanus writes *Filotegni* for the Greek.
[57] T. N.: The Latin reads, "positional gravity."
[58] T. N.: The Latin reads, The motion of every heavy body is towards the center, with its force and power tending downward and resisting contrary motion . . .
Every falling body is heavier when its motion is straight toward the center.
A body is heavier positionally when in a given position, the descent is less oblique.
The descent is more oblique to the same extent that it projects less on the vertical.

## FOOTNOTES TO CHAPTER VII

[1] Bibliothèque Nationale, Latin collection, No. 12247.
[2] T. N.: The Latin reads, "As was evident in the next to last of the above demonstrations."
[3] T. N.: The Latin reads, "As it has been demonstrated by Euclid, Archimedes and others."
[4] Bibliothèque Nationale, Latin collection, Ms. 8680 A. The text of Ms. 7378 A, when compared to that of Ms. 8680 A, shows that the demonstrations had undergone several significant emendations. The fragment preserved in the "Codex Mazarineus" is a part of the unemended text in manuscript 8680 A.
[5] T. N.: This is the book Duhem called earlier the *Filotegni.*
[6] Bibliothèque Nationale, Latin collection, Ms. No. 7378 A.
[7] T. N.: The Latin reads, "It has been demonstrated in books dealing with these matters."
[8] Ibid., Latin collection, Mss. Nos. 7310 and 10260.
[9] Ibid., Latin collection, Ms. No. 7215.
[10] Maximilian Curtze, *Das angebliche Werk des Euklides über die Waage* (Zeitschrift für Mathematik und Physik, XIX. Jahrgang, p. 263, 1874).
[11] Valentin Rose, *Anecdota graeca et graeco-latina*, IIter Heft, 1870, — VII, *Zwei Bruchstücke griechischer Mechanik. Philon und Heron.*
[12] T. N.: The Latin title reads, *The Book on Weights by Jordanus*, according to some, by Euclid. The Latin quote reads, "Here ends *The Book on Weights*, attributed by some to Euclid.
[13] Maximilian Curtze, *Zwei Beiträge zur Geschichte der Physik* (Bibliotheca Mathematica, 3e Folge, Bd. I, p. 51; 1900).
[14] Maximilian Curtze, *Ueber die Handschrift*, R. 40 2: Problematum Euclidis explicatio des Königl. Gymnasiums zu Thorn (*Zeitschrift für Mathematik und Physik*, XIIIter Jahrgang, Supplément, p. 45, 1868). One can say the same about a piece entitled *Liber de ponderibus vel de statera Jordani*, pointed out by Curtze in Ms. Db. 86, at the beginning of the XIVth century, kept in the Library of Dresden (Maximilian Curtze, *Ueber eine Handschrift der K. Bibliothek zu Dresden: Zeitschrift für Mathematik und Physik*, XXVIIIter Jahrgang, Supplément, p. 1; 1883).
[15] This is the date attributed to it by Thurot (a) and confirmed by the Gothic handwriting. The catalogue in the Bibliothèque Nationale assigns Ms. 7378 A to the 14th century. As a matter of fact, other texts follow those which interest us here and are written in a different handwriting and almost certainly were copied in the 14th century. Thus, on sheet f. 52, one reads: Expliciunt canones tabularum astronomie sive tractatus de sinibus et cordis per Magistrum Johannem de Linieriis, ordinati et completi Parisiis anno ab Incarnatione Domini 1322. The same on sheet f. 63; Explicit pronosticatio Magistri Leonis Judei facta in Anno Domini 1341. Incipit pronosticatio Magistri Johannis de Muris super eodem. (T. N.: The Latin reads, Here end the rules of the astronomical tables or the Treatise on Sines & Cords by Master John of Linierius, completed in Paris, 1322. Here ends the Prognosis of Master Leon Judeus made in 1341. Here begins the Prognosis of Master John of Murs.) Chevalier (b) gives us the following information on John of Murs: musician and mathematician, Doctor at the Sorbonne, 1321.

(a) Thurot, *Recherches historiques sur le Principe d'Archimède* (Revue Archéologique, nouvelle série, Vol. XIX, p. 117, 1869.)

(b) U. Chevalier, *Bibliographie du moyen age*, col. 1213.

[16] I am obliged to M. Goedseels, administrator-inspector of the Royal Belgian Observatory for being able to consult a copy of this work kept in the Library of the Observatory. I would like to thank M. Goedseels for his kind generosity.

[17] T. N.: The Latin title reads, *The Book on Weights* by the illustrious Jordanus de Nemore, containing thirteen propositions with their demonstrations as well as extremely elegant explications of many other matters, now published with Imperial privilege by Peter Apian, mathematician of Ingolstadt, licensed for thirty years, 1533.

[18] T. N.: The Latin reads, "Another commentary follows."

[19] Bibliothèque Nationale, Latin collection, No. 7378 A.

[20] This is the date assigned to the manuscript by Thurot (*Recherches historiques sur le Principe d'Archimède* (Revue Archéologique, nouvelle série, Vol. XIX, p. 117, 1869)). The handwriting of this manuscript is clearly from the 13th century.

[21] Bibliothèque Nationale, Latin collection, No. 8680 A.

[22] Jordani, *Opusculum de ponderositate*, Nicolai Tartaleae studio correctum novisque figuris auctum. Venetiis, apud Curtium Trojanum, MDLXV. (T. N.: The Latin reads, *A Treatise on Weight* by Jordanus, corrected and enlarged with new figures by Nicolo Tartaglia. Venice, Curtius Trojanus, 1565.

[23] T. N.: The Latin words "pondus," "mundus," "regula" and "responsa" mean "weight," "world," "rule" and "answer" respectively.

[24] T. N.: Duhem, in his *Études sur Léonard de Vinci*, changed this designation to the Precursor of Simon Stevin.

[25] Edition by Curtius Trojanus, Quaestio XXIX.

[26] Ibid., Quaestio XXXV.

[27] Ibid., Quaestio XXX.

[28] Ibid., Quaestio XXXIII.

[29] Ibid., Quaestio XLII.

[30] Ibid., Quaestio XLIII.

[31] Ibid., Quaestio XLI.

[32] Ibid., Quaestio XXXIV.

[33] T. N.: The Latin title reads, *On the Common Principles of all Natural Phenomena.*

[34] Liber primus, Prop. II. — Edition of Curtius Trojanus, Quaestio II.

[35] Liber primus, Prop. VIII — Edition Curtius Trojanus, Quaestio VIII.

[36] Liber tertius, Propositiones I et II. — Editions of Curtius Trojanus, Quaestiones XXIII et XXIV.

[37] Liber tertius, Propositio V. — Edition of Curtius Trojanus, — Quaestio XXVII. The scribe introduced errors in this proposition so that the author's ideas are sometimes rendered unintelligible.

[38] Liber primus, Propositio VIII. Edition of Curtius Trojanus, Quaestio VIII.

[39] Liber primus, Propositio VIII, et Liber tertius, Propositio III. Edition of Curtius Trojanus, Quaestiones VIII et XXV.

[40] Liber primus, Propositio IX. Edition of Curtius Trojanus, Quaestio IX.

[41] Ibid.

[42] It is clear that the author imagines the two weights being joined by a cord which at *d* passes over a pulley.

[43] T. N.: Duhem has written “el” where one should read “It”.
[44] Cf.: Affò, *Scrittori parmigiani* Parma, 1789: Vol. II. pp. 112, 118, 123. — Tiraboschi, *Storia delle lettere italiane.* 1807, Vol. VI, I, pp. 335—339. — Gherardi, *Di alcuni materiali per la storia della facoltà matematica nell'antica universita di Bologna*, Bologna. 1846.
[45] T. N.: Italian for, “A distinguished philosopher and mathematician.”
[46] Bassiani politi Laudens, *De numero modalium*; — Ejusdem, *Tractatus proportionum.* — Nicolai Horen, *Proportiones*; — Ejusdem, *De latitudinibus formarum.* — Blasii de Parma, *De latitudinibus formarum; De sex inconvenientibus.* — Joannis de Lasali, *De velocitate motus alterationis* Blasii de Parma, De tactu corporum durorum. Venetiis, mandato et sumptibus heredum Oct. Scoti Modoetiens, per Bonetium Locatellum Bergom. Kal. Sept. MDV.
[47] Bibliothèque Nationale, Latin collection, No. 10252.
[48] T. N.: The Latin reads, Song of Weights or of Weights and Measures.
[49] T. N.: The Latin reads, The Minor Latin Poets.
[50] T. N.: Latin for, “Since the science of weights is subordinate to geometry as well as to natural philosophy . . .”
[51] T. N.: Latin for, “The science of weights is rightly said to be subordinated to natural philosophy.” Duhem seems to ignore the essential difference between these two quotes.
[52] T. N.: The Latin reads, “Now, with the weight having been given, I wish to obtain a knowledge of the arms.”
[53] T. N.: The Latin reads, “However, let the philosophers consider it.”

## FOOTNOTES TO CHAPTER VIII

[1] Cf. Chapter II.
[2] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. M de la Bibliothèque de l'Institut, folio 68, verso.
[3] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A de la Bibliothèque de l'Institut, folio 5, recto.
[4] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E de la Bibliothèque de l'Institut, folio 32, verso.
[5] Cf. in particular: *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. F de la Bibliothèque de l'Institut, folio 3, verso, and folio 4, recto.
[6] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. 2038 (Italian) of the Bibliothèque Nationale (Acq. 8070, Libri) folio 2, verso.
[7] Leonardo da Vinci, loc. cit.
[8] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Millien, Ms. 2038 (Italian) Bibliothéque Nationale (Acq. 8070, Libri) folio 3, recto.
[9] These words should be replaced by the following: “if its extremities carry equal weights,” otherwise all of this passage by Leonardo would be incorrect.
[10] This idea must be interpreted in the following way: “The proportion between line *ab* and line *ac* will be the same as the proportion between the weight carried by the length *cm* and the weight carried by the length *cn*.”
[11] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E de la Bibliothèque de l'Institut, folio 58, recto.

[12] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 78, verso.
[13] The Latin title of the original Greek reads, *The Extant Collected Works* of Pappus of Alexandria, edited from the manuscripts and provided with a Latin translation and commentary by Friedrich Hultsch. Vol. III, Berlin, 1878, pp. 1032—1033.
[14] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 79, recto.
[15] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 79, verso.
[16] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E de la Bibliothèque de l'Institut, folio 57, verso. Cf. folio 58, recto.
[17] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E de la Bibliothèque de l'Institut, folio 59, recto.
[18] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 75, recto.
[19] Cf. **Figure 9** in Chapter II where this passage was quoted.
[20] T. N.: Duhem uses the word "pressure," which is the result of the historical development of statics, but modern usage requires the word "force."
[21] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 76, verso.
[22] That is to say: does not exert at each of the two points a force equal to its total weight.
[23] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E de la Bibliothèque de l'Institut, Paris, 1883.
[24] Ms. E, folio 65, recto.
[25] Ms. E, folio 60, verso.
[26] Ms. E, folio 60, verso.
[27] Ms. E, folio 60, recto.
[28] Ms. E, folio 61, verso; Cf. folio 63, recto.
[29] Ms. E, folio 63, recto.
[30] Ms. E, folio 67, verso.
[31] T. N.: The "supreme heights," or lines GF and GD of Duhem's text, can only be the moment arms.
[32] Ms. E, folio 67, verso.
[33] Ms. E, folio 66, verso.
[34] Ms. E, folio 68, recto and verso; folio 69, recto and verso; folio 70, recto; folio 71, recto.
[35] Cf., in particular, the passage quoted in Chapter II and Ms. M, folio 36, verso.
[36] This passage was pointed out by Maximilian Curtze in a note published in the *Bibliotheca Mathematica*, 3te Folge, Bd. II, 1901; p. 355.
[37] Pappi Alexandrini, *Collectiones quae supersunt e libris manuscriptis edidit*, latina interpretatione et commentariis instruxit Fridericus Hultsch. Volumen III. Berolini, MDCCCLXXVIII. (T. N.: The Latin title of the original Greek reads, *The Extant Collected Works* of Pappus of Alexandria, edited from the manuscripts and provided with a Latin translation and commentary by Friedrich Hultsch. Vol. III, Berlin, 1878).
[38] Loc. cit., p. 1029.
[39] The principle of this theory is the concept of the moment of a weight suspended at

the extremity of an arm of an oblique lever. This concept must have certainly been known by the geometers of the School of Alexandria during the time Pappus wrote. Hero (a) formulates it clearly. The part of the *Mechanics* of Hero where he uses this concept is not in the excerpt from the work which is appended to the *Collections* of Pappus. It remained unknown until its publication by M. Carra de Vaux. Thus it neither influenced Leonardo da Vinci, nor contributed to the development of modern mechanics. Hero (b) also observes concerning the windlass that this "instrument and all the devices of great force which resemble it are slow, because the weaker the force compared to the very heavy weight being moved, the more the time needed to accomplish the work. There is an equal ratio between force and time." Hero makes the same observations again concerning the block and tackle (c) and the lever (d). But these passages too, were not known until the publication of Carra de Vaux. Furthermore, they do not add anything to what the *Mechanical Problems* taught on this subject.

(a) *The Mechanics or the Elevator* of Hero of Alexandria published for the first time, based on the Arabic version by Qustâ ibn Lûkâ and translated into French by M. Carra de Vaux. Excerpt from the *Journal Asiatique*, Paris, 1894. Book I, Art 34, p. 91.

(b) Loc. cit., p. 131.

(c) Loc. cit., p. 134.

(d) Loc. cit., p. 156.

[40] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 77, verso.

[41] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 79, recto.

[42] T. N.: Duhem is convinced that Leonardo obtained this solution from Pappus althought the evidence he presents is inconclusive.

[43] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A de la Bibliothèque de l'Institut, folio 21, verso.

[44] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. A de la Bibliothèque de l'Institut, folio 33, recto.

[45] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. M de la Bibliothèque de l'Institut, folio 42, recto.

[46] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. H de la Bibliothèque de l'Institut, folio 81 (33), verso.

## FOOTNOTES TO CHAPTER IX

[1] *Quesiti et Inventioni diverse di Nicolo Tartaglia*, Venetia, Vent. Ruffinelli, 1546 — Various editions of this work follow rapidly. One can cite the following: *Quesiti et Inventioni diverse. La nova Scientia*, Venetia, Nic. de Bascarini, 1550; — *Quesiti et Inventioni diverse, Ragionamenti sopra la travagliata Inventione*, con supplemento, Venetia, Nic. de Bascarini, 1551; — *Quesiti et Inventioni diverse, Regola generale de solevare con ragione e misura non solamente ogni affondata nave, ma una torre solida di metallo*, Venetia, Nic. de Bascarini, 1551; — *Quesiti et Inventioni diverse, con una giunta al sesto libro, nella quale si mostra duoi modi di redur una città inespugnabile*, Venetia, Nic. de Bascarini, 1554. — Furthermore, one can mention; *Opere del famosis-*

*simo Nicolo Tartaglia, cioè Quesiti, Nova Scientia, Travagliata Inventione, Ragionamenti sopra Archimede*, etc., Venetia, 1606. (T. N.: The Italian reads respectively, *Diverse Questions and Inventions of Nicolo Tartaglia; Diverse Questions and Inventions. The New Science; Diverse Questions and Inventions, Reflections on burdensome inventions, with supplements; Diverse Questions and Inventions, General Rules to raise with logic and control not only any sunken ship but a solid metal tower; Diverse Questions and Inventions with an addition to the sixth book, in which is demonstrated two methods for breaching an impregnable city; Works of the famous Nicolo Tartaglia, that is, Questions, New Science, burdensome Inventions, Reflections on Archimedes*, etc.

[2] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. G de la Bibliothèque de l'Institut, folio 77, recto.

[3] Tartaglia, *Quesiti et Inventioni diverse*, edition of 1544, p. 8, verso.

[4] *Quesiti et Inventioni diverse*, Libro ottavo, Quesiti XXVII, Petitione VI.

[5] *Quesiti et Inventione diverse*, Libro ottavo, Quesiti XLI, XLII; Propositioni XIV, XV.

[6] T. N.: The Italian reads, "I am his protegé."

[7] *I sei cartelli di matematica disfida primamenti intorno alla generale risoluzione delle equazioni cubiche di Ludovico Ferrari, Coi sei contro-cartelli in riposta di Nicolo Tartaglia comprendenti le soluzioni de' quesiti dall' una et dall' altra parte propositi.* Raccolti, autografati e publicati da Enrico Giordani, Bolognese. Premesse notizie bibliografiche ed illustrazioni sui Cartelli medesimi, estratte da documenti già a stampa ed altri manoscritti favoriti dal Comm. Prof. Silvestro Gherardi. Milano, 1876. (T. N.: The Italian reads, The six letters of the mathematical challenge primarily concerned with the general solution to cubic equations by Ludovico Ferrari, with six letters of reply by Nicolo Tartaglia concerning the solution of problems posed by both parties. Collected, printed and published by Enrico Giorodani of Bologna. Prefaced by a bibliographic reference and illustrations of the same letters, taken from documents previously published and other manuscripts given by Commendatore Professor Silvestro Gherardi. Milano, 1876.) These six challenges by Ferrari and Tartaglia's six ripostes remained unknown to mathematicians until Silvestro Gherardi had the good fortune to reassemble the entire collection. The six challenges by Ferrari which he found all bore in the author's handwriting the address: Al Signor Nicolo Simo: Nicolo Simo was actually one of the geometers to whom the two adversaries sent their works. This collection is quite unique. Only the second of Ferrari's challenges is in the St. Mark Library in Venice. Gherardi published the twelve texts in Bologna in 1846 appended to his own book entitled: *Di alcuni materiali per la storia della facoltà matematica in Bologna.* (T. N.: The Italian reads, *Some Reference Material on the History of the Mathematical School in Bologna.*) He later lent them to Libri. They were sold in London in 1861 together with the library of Libri. In 1876 this collection was reproduced in facsimile by Enrico Giordani and was dedicated to Prince Baldassare Boncompagni. (Cf. *Catalogue of mathematical, historical, bibliographical, and miscellaneous portion of the celebrated library of Guglielmo Libri*: Part I: A-L, London 1861, no. 178, pp. 19 and 20: On page 1 there is a facsimile of the signature of Ferrari. — J. Ch. Brunet, *Manuel du Librarie et de l'amateur de livres*, Vol. V, 1864, column 661 — and the announcement preceding the reprinting of 1876.)

[8] Ferrari, Primo cartello, p. 2.

[9] Ferrari, Secondo cartello, p. 6.

[10] Seconda riposta data da Nicolo Tartalea Brisciano, pp. 7 and 8.
[11] *Jordani Opusculum de ponderositate*, Nicolai Tartaleale studio correctum, novisque figuris auctum. Cum privilegio. Venetiis, apud Curtium Trojanum, MDLXV. (T. N.: The Latin title reads, *A Treatise on Weights by Jordanus*, corrected and enlarged with new figures by Nicolo Tartaglia. With Imperial Privilege. Venice, Curtius Trojanus, 1565.)
[12] Hieronymi Cardani *De numerorum proprietatibus liber unicus*; Caput LXVI, de Ponderibus. (T. N.: The Latin title reads, *A Single Book on the Properties of Numbers*, by Jerome Cardan, Chapter 66, "On Weights.") According to Niceron (a) this work was printed for the first time after the death of its author in: Hieronymi Cardani *Opera omnia*, tomus IV. (T. N.: The Latin title reads, *The Complete Works of Jerome Cardan*, Vol. IV.)

(a) Niceron, *Mémoires pour servir à l'histoire des hommes illustres*, Vol. XIV, p. 271; Paris, 1731.

[13] In the manuscript (b) which we studied, Blasius of Parma actually says that a single grain of millet can maintain in equilibrium a weight a thousand times heavier. Cardan, doubtlessly, must have had a different version of that same treatise.

(b) Bibliothèque Nationale, Latin collection, Ms. 10252.

[14] *Les Livres de Hierome Cardanus, médecin milannois, intitulés de la Subtilité et subtiles Inventions*, ensemble les causes occultes et raisons d'icelles, traduis de Latin en François par Richard Le Blanc, Paris, Charles l'Angelier, 1556. (T. N.: The French title reads, *The Book on Subtlety and Subtle Inventions*, Together with the Occult Causes and their Reasons, by Jerome Cardan, Milanese physician, translated from Latin into French by Richard Le Blanc, Paris, Charles l'Angelier, 1556.)
[15] Alexandri Piccolominei *in mechanicas quaestiones Aristotelis paraphrasis paulo quidem plenior*, ad Nicolaum Ardinghellum Cardinalem amplissimum. (On the last page: Excussum Romae apud Antonium Bladum Asulanum, Tertio Non. Januarii MDXLVII). — The same work was re-edited: Venetiis, apud Curtium Trojanum, MDLXV. — It was also translated into Italian under the title: *A. Piccolomini, Sopra le mecaniche d'Aristotile*, translated by O. V. Biringucci. Roma, Zanetti, 1582. (T. N.: The Latin reads, *A Somewhat Fuller Paraphrase of the Mechanical Problems of Aristotle by Alexander Piccolomini*, dedicated to the illustrious Cardinal Nicolas Ardinghello, published at Rome by Antonius Baldus Asulanus, the third of January 1547.)
[16] Edition of 1547, p. 22, verso.

## FOOTNOTES TO CHAPTER X

[1] Other authors spell his name differently. Favaro, for example, writes, Guidobaldo dal Monte. We follow the spelling adopted by Pigafetta in the translation of the *Mechanicorum liber* (T. N.: The Latin title reads, *The Book on Mechanics*) which Pigafetta published in Italian in 1581, during the author's lifetime.
[2] Hieronymi Cardani Mediolanensis civisque Bononiensis *Opus novum de proportionibus numerorum* . . . Propositio CLXXVI, p. 197; Basileae, MDLLXX. (T. N.: The Latin title reads, *A New Work on the Ratios of Numbers* by the Milanese Jerome Cardan, a citizen of Bologna . . . Proposition 176, p. 197; Basel, 1570).
[3] *Admiranda Archimedis monumenta omnia quae exstant*, ex traditione D. Francisci

Maurolyci. Panormi, ap. Cyllenium Hispanicum, DMCLXXXV. (T. N.: The Latin title reads, *All the Extant and Admirable Monuments of Archimedes*, expounded by Francesco Maurolico. Palermo, Cyllenius Hispanicus, 1685).

[4] Federici Commandini *Liber de centro gravitatis solidorum*, Bononiae, MDLXV. (T. N.: The Latin title reads, *A Book on the Center of Gravity of Solids*, by Frederico Commandino, Bologna, 1565).

[5] Lucae Valerii *De centro gravitatis solidorum libri III*, MDCIV. (T. N.: the Latin title reads, *Three Books on the Center of Gravity of Solids*, by Luca Valerio, 1604)

[6] Galileo Galilei, *Dialoghi delle Scienze nuove* . . . Giornata seconda. (T. N.: The Italian reads, *Dialogues on New Science* . . . Second Day. Duhem must be referring to the *Dialogues Concerning Two New Sciences.*)

[7] Guido Ubaldi e Marchinonibus Montis *De cochlea libri quatuor*, superiorum permissu et privilegio. Venetiis, apud Evangelistam Deuchinum, MDCXV. (T. N.: The Latin title reads, *Four Books on the Screw* by Guido Ubaldo, Marquis del Monte, with the permission and privilege of his Superiors. Venice, Evangelista Deuchinus, 1615)

[8] Guidi Ubaldi e Marchionibus Montis *In duos Archimedis aequiponderantium libros paraphrasis*, scholiis illustrata. Pisauri, apud Hieronymum Concordiam, MDLXXXVIII. (T. N.: The Latin title reads, *A Paraphrase of Two Books of Archimedes on Equilibrium* illustrated with scholia. Pesaro, Jerome Concordia, 1583.)

[9] Guidi Ubaldi e Marchionibus Montis *Mechanicorum liber*, in quo haec continentur: De libra, de vecte, de trochlea, de axe in peritrochio, de cuneo, de cochlea. Superiorum permissu et privilegio. Pisauri, apud Hieronymum Concordiam, MDLXXVII. (T. N.: The Latin title reads, *The Book on Mechanics* of Guido Ubaldo, Marquis del Monte, which contains the following: On the Balance. On the Lever, On the Block and Tackle, On the Windlass, On the Wedge, On the Screw, With the permission of his Superiors. Persaro; Jerome Concordia, 1577.) — The same work has been reprinted in: Venetiis, apud Evangelistam Deuchinum, MDCXV. — It has also been translated into Italian under a title of rather curious grandiloquence: *Le mechaniche* dell'illustriss. Sig. Guido Ubaldo de Marchesi del Monte, tradotte in volgare dal Sig. Filippo Pigafetta. Nelle quali si contiene la vera dottrina di tutti gli istrumenti principali di mover pesi grandissimi con piccola forza. A beneficio di chi si diletta di questa nobilissima scienza; et massimamente di capitani di guerra, ingegnieri, architetti, et d'ogni artefice, che intenda per via di machine far opre maravigliose, e quasi sopranaturali. Et si dichiarano i vocabili, et luoghi piu difficili. In Venetia, appresso Francesco di Franceschi Sanese, MDLXXXI. — A second edition of this translation was published in Venice in 1615. (T. N.: The Italian reads, *The Mechanics* of the very illustrious Guido Ubaldo, Marquis del Monte, translated into Italian by Filippo Pigafetta. In which is included the true doctrine of all the principal instruments for moving very large weights with a small force. For the benefit of those who take pleasure in this extremely noble science and most of all for captains of war, engineers, architects and every artisan who intends to do marvelous and almost supernatural things with the help of a machine. With an explantion of the most difficult words and passages. In Venice, by Francesco di Franceschi Sanes, 1581.

[10] Guidi Ubaldi *Mecanicorum liber*, ad Franciscum Mariam II, Urbinatum ducem, praefatio. (T. N.: The Latin title reads, *The Book on Mechanics* by Guido Ubaldo, to Francesco Maria II, Duke of Urbino, Preface.)

[11] Guidi Ubaldi *Mecanicorum liber*, de libra, Propositio IV. (T. N.: The Latin title reads, *The Book on Mechanics* by Guido Ubaldo, "On the Balance, Proposition IV.")
[12] We purposely omit, while presenting Guido Ubaldo's argumentation, the convergence of verticals, which troubles the author.
[13] T. N.: The lever arm is pivoting about point D.
[14] *Les Méchaniques de Galilée*, mathématicien et ingénieur du duc de Florence, avec plusieurs additions rares et nouvelles, utiles aux architectes, ingénieurs, fonteniers, philosophes et artisans; traduites de l'Italien par L. P. M. M. (le P. Mersenne, Minime). A Paris, chez Henry Guenon, MDCXXXIV; 2e addition, p. 23. (T. N.: The French titles read, *The Mechanics of Galileo*, mathematician and engineer to the Duke of Florence, with several rare and new additions useful to architects, engineers, excavators, philosophers and craftsmen; translated from Italian By L. P. M. M.: Father Mersenne, of the Order of the Minims. Paris, Henry Guenon, 1634, 2nd edition, p. 24.)
[15] *The Book on Mechanics* by Guido Ubaldo, "On the Lever," Proposition X.
[16] The geometers who reproduced the theory of Guido Ubaldo were careful to specify and correct it on this point. Pierre Hérigone (a) understands by the word "force" a weight suspended from the arm of a lever, as the drawings which he used show. In this way, Guido Ubaldo's theorems are corrected. Even more careful is Jacques Rohault. His posthumous treatise (b) deals separately with a case in which the force is represented by a weight freely suspended and with a second case in which the force always remains perpendicular to the arm of the lever. The care used by these authors in order to clarify Guido Ubaldo's thought is proof of how uncertain, if not erroneous, his ideas were.

(a) Pierre Hérigone, *Cours de Mathématique*, Vol. III: *les Méchaniques*, Proposition VI, Paris, 1634. (T. N.:The French title reads, Pierre Hérigone, *Course on Mathematics*, Vol. III.; *Mechanics*, Proposition VI, Paris 1634).

(b) *Oeuvres posthumes* de J. Rohault (publiées par Clerselier). *Traité des Méchaniques.* Proposition XI. Paris, 1682. (T. N.: The French title reads, *Posthumous Works* of J. Rohault (published by Clerselier). *Treatise on Mechanics, Proposition XI. Paris, 1682.*)
[17] Lagrange, *Mécanique analytique*, 1re partie, Section I, Sur les différents principes de la Statique, Art. 4.
[18] Lagrange, loc. cit. Art. 16.
[19] Guidi Ubaldi *Mecanicorum liber*, de vecte. Propositio III. (T. N.: The Latin title reads, *The Book on Mechanics* by Guido Ubaldo, "On the Lever," Proposition III.)
[20] Guidi Ubaldi *Mecanicorum liber*, de trochlea, Propositiones X ad XXVIII. (T. N.: The Latin title reads, *The Book on Mechanics* by Guido Ubaldo, "On the Block and Tackle," Propositions X to XXVIII.)
[21] Guidi Ubaldi *Mecanicorum liber*, de trochlea, Propositio XXVI, Corollarium. (T. N.: The Latin title reads, *The Book on Mechanics* by Guido Ubaldo. "On the Block and Tackle." Proposition XXVI, Corollary.)
[22] Id., ibid., de trochlea, Propositio XXVIII, Corollarium II.
[23] Guidi Ubaldi *Mecanicorum liber*, de trochlea, de axe in peritrochio. (T. N.: The Latin title reads, *The Book on Mechanics* by Guido Ubaldo, "On the block and tackle, On the Windlass.")
[24] Id., ibid., de cuneo (T. N.: The Latin title reads, "On the Wedge,")
[25] Id., ibid., de cochlea. (T. N.: The Latin title reads, "On the Screw.")

[26] Jo. Baptistae Benedicti, patritii Veneti, philosophi, *Diversarum speculationum mathematicarum et physicarum liber*, quarum seriem sequens pagina indicabit. Ad serenissimum Carolum Emmanuelem Allobrogum et Subalpinorum ducem invictissimum. Taurini, apud haeredem Nicolai Beveliquae, MDLXXXV. (T. N.: The Latin title reads, *A book of Diverse Speculations on Mathematics and Physics*, with the sequence indicated on the following page, by Giovanbattista Benedetti, citizen of Venice and philosopher. To his most Serene Charles Emmanuel, the invincible Duke of Savoy and the Sub-Alpines. Turin, the Heir of Nicolò Bevilaqua, 1585.)

[27] J. B. Benedicti *Diversarum speculationum* . . . p. 141. De mechanicis.

[28] T. N.: The Latin quote reads, "And by this means alone, I would have left clear proof that I had lived among mortals."

[29] *De resolutione omnium Euclidis problematum aliorumque ad hoc necessario inventorum una tantummodo circuli data apertura*, per Joannem Baptistam de Benedictis inventa. On the last page: Venetiis, apud Bartholomaeum Caesanum. MDLIII. (T. N.: The Latin title reads, *On the Resolution of all the Problems of Euclid* and of others by a single setting of the Compass, invented by Giovanbattista Benedetti, Venice, Bartholomaeus Caesanus, 1553.)

[30] This passage was reproduced by Libri, *Histoire des Sciences mathématiques en Italie*, note XXV. vol. III. p. 258.

[31] J.B. Benedicti *Diversarum speculationum* . . . Disputationes de quibusdam placitis Aristotelis, Caput, X, p. 174. — This passage is also reproduced by Libri, loc. cit., p. 264. (T. N.: The Latin title reads, Giovanbattista Benedetti's *Diverse speculations* . . . Disputations on certain views of Aristotle, Chapter X, p. 174.)

[32] Hieronymi Cardani Mediolanensis, civisque Bononiensis, philosophi, medici et mathematici *Opus novum de proportionibus*; Basileae, ex officina Henricpetrina, Anno Salutis MDLXX, Mense Martio. Liber V, Propositio CX, p. 104.

[33] Joannis Taisnierii Hannonii *Opusulum perpetuum memoria dignissimum de natura magnetis et ejus effectibus. Item de motu continuo, demonstratio proportionum motuum localium contra Aristotelem et alios philosophos; de motu alio celerrimo hactenus incognito, atque de fluxu et reflexu maris.* Coloniae Agrippinae, MDLXII. (T. N.: The Latin title reads, *A treatise Most Worthy of Lasting Memory on the Nature of the Magnet and its Effects.* Also on continuous motion: A Demonstration of the Ratios of Local Motions against Aristotle and other Philosophers; On other rapid motion hitherto unknown, and on the tides of the ocean by Joannes Taisnier. Cologne, 1562.)

[34] Simonis Stevini *Mathematicorum Hypomnematum Statica.* Appendix Statices. Caput II: Res motas impedimentis suis non esse proportionales, p. 151. Lugodini Batavorum, MDCV. (T. N.: The Latin reads, Simon Stevin, Mathematical Memoires on Statics. Appendix on statics. Chapter II: Moving Bodies are not proportional to their resistances, p. 151. Leyden, 1605.)

[35] J. B. Benedicti *Diversarum speculationum* . . . De mechanicis, Caput XI, p. 153.

[36] Id., ibid., Caput VII et Caput VIII.

[37] J. B. Benedicti *Diversarum speculationum* . . . De mechanicis, Caput I.

[38] J. B. Benedicti *Diversarum speculationum* . . . De mechanicis, Caput II.

[39] Id., ibid., De mechanicis, Caput III.

[40] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. I of the Bibliothèque de l'Institut, folio 30, recto. Cf. chapter II.

[41] T. N.: This quote is not clear as it appears in the French.
[42] Cf. Chapter VIII, Section I and Figure 39.
[43] J. B. Benedicti *Diversarum speculationum*. . . . De mechanicis, Caput XII.
[44] T. N.: Although Benedetti uses the term "gravity," the text implies "positional gravity."
[45] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E. of the Bibliothèque de l'Institut, folio 57, verso; folio 58, recto; folio 59, recto. — Cf. Chapter VIII, Section 1.
[46] Cf. Chapter IX, Section 2.
[47] J. B. Benedicti *Diversarum speculationum* . . . De mechanicis, Caput XXI.
[48] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. E. of the Bibliothéque de l'Institut, folio 55, recto.
[49] J. B. Benedicti *Diversarum speculationum* . . . De mechanicis, Caput V.

## FOOTNOTES TO CHAPTER XI

[1] T. N.: Giovanni de Medici had proposed the construction of a large dredging machine of his own design to clear the harbor of Leghorn. Galileo criticized the project with characteristic candor.
[2] Libri, *Histoire des Sciences Mathématiques en Italie*, Paris, 1841; Vol. IV, pp. 176—180.
[3] T. N.: In English, this is commonly known as a Sector or Military compas.
[4] This work is known in English as the "*Discourses and Demonstrations Concerning Two New Sciences.*"
[5] Viviani, *Vita di Galileo Galilei*, cavati da *Fasti consolari* dall'Accademia Fiorentina di Salvino Salvini. Firenze, MDCCXVII. (T. N.: The Italian reads, *Life of Galileo Galilei*, taken from the *Fasti consolari* of the Academy of Florence by Salvino Salvini. Florence, 1717.)
[6] This short treatise is also contained in the following edition: *Le Opere di Galileo Galilei*, ristampate fedelmente sopra la Edizione nazionale con approvazione del Ministerio della publica Instruzione. Vol. I (the only volume published). Firenze, Successori Le Monnier, 1890. (T. N.: The Italian reads, *The Works of Galileo Galilei*, accurately reprinted from the National Edition with approval of the Ministry of Public Instruction.)
[7] On p. 122 of the preceding edition.
[8] On pp. 76 and 77 of the preceding edition.
[9] On p. 61 of the preceding edition.
[10] T. N.: The Latin reads, "Simple motion ends in rest." "Nothing violent is perpetual."
[11] These two versions of *De Motu* are also in Vol. 1 of the 1890 edition. (T. N.: The Latin title reads, *On Motion.*)
[12] The dialogue *De Motu* is reproduced in Vol. I of the 1890 edition.
[13] T. N.: The Italian reads, *On Mechanics*, read in Padua by Galileo Galilei in the year 1594.
[14] *Delle Meccaniche lette in Padova l'anno 1594* da Galileo Galilei, per la prima volta pubblicate ed illustrate da Antonio Favaro (Memorie del R. Instituto Veneto di Scienze, Lettere ed Arti. Vol. XXVI, No. 5, 1899.)

[15] T. N.: The two titles read, *Preludes to the Universal Harmony* and *Theological, Physical Moral and Mathematical Questions.*
[16] *Les Méchaniques de Galilée*, mathematician and engineer to the Duke of Florence, with several rare and new additions, useful for architects, engineers, excavators, philosophers and artisans, translated from Italian by L.P.M.M.; Paris, Henry Guenon, rue St. Jacques, near the Jacobins, under the sign of St. Bernard, 1634.
[17] *Le Opere di Galileo Galilei*, Florence, 1890, vol. 1, p. 256. (T. N.: The Italian title reads, *The Works of Galileo Galilei*, Florence, 1890, Vol. I. p. 256.)
[18] T. N.: Latin for "which is inconsistent."
[19] *Le Opere di Galileo Galilei*, Florence, 1890, Vol. I, 296.
[20] T. N.: Duhem is referring to the arc formed by point *a* rotating about point *s*.
[21] *Le Opere di Galileo Galilei*, Florence, 1890, Vol. I, p. 297.
[22] T. N.: The Latin reads, "When the moving body is at point *s* and at the moment it touches *s*, its descent will be as though along the line *gh*; therefore, the motion of the moving body along line *gh* will be according to the gravity which the moving body has at point *s*."
[23] T. N.: Duhem translates Stevin's *De Beghinselen der Weeghconst* as *Les Eléments de Statique*, although Albert Girard gives a more literal translation in his French version of 1634: *L'Art Pondéraire ou la Statique.*
[24] T. N.: The Flemish title reads, *The Principles of the Art of Weighing.*
[25] *Le Opere di Galileo Galilei*, Florence, 1890, Vol. I, p. 260.
[26] *Le Opere di Galileo Galilei*, Florence, 1890, Vol. I, p. 272. Cf. Ibid., p. 296.
[27] T. N.: The context implies "apparent" weight.
[28] Ibid., p. 301.
[29] *Le Opere di Galileo Galilei*, Florence, 1890, Vol. I, p. 401.
[30] Ibid., pp. 401 and 402.
[31] I purposefully avoid translating into French this word "momento," because the word moment in today's mechanics denotes a different notion from "momento." Moment, which is a product of a force times a length, is not, despite what Lagrange says (*Mécanique analytique*, Part I, Section 1, number 4), a particular case of the "momento," which is the product of a force and its velocity.
[32] This word has the same meaning here that it has for Leonardo da Vinci. The meaning comes close to what Leibniz calls the *force vive.*
[33] "Momento," has here the same meaning as the Latin "momentum," namely, importance or significance.
[34] *Dialogo delle due massimi Sistemi del Mondo*, giornata secunda.
[35] Galileo Galilei, *Discorsi* . . . , giornata terza, Theorem II, Prop. II, Scholium.
[36] *Della Meccaniche lette in Padova* l'anno 1594 da Galileo Galilei . . . (Memorie del R. Istituto Veneto di Scienze, Lettere ed Arti, Vol. XXVI, No. 5, 1899.)
[37] Ibid., Chapt. 5.
[38] Ibid., Chapt. 8.
[39] Ibid., Chapt. 9.
[40] Ibid., Chapt. 15.
[41] Ibid., Chapt. 16.
[42] Ibid., Chapt. 15.
[43] Ibid., Chapt. 12.
[44] Ibid., Chapt. 13.

[45] *Les Méchaniques de Galilée*, p. 7.
[46] T. N.: The Greek word means downward momentum or pull.
[47] This quote is from the treatise *Della Scienza Meccanica*; the same considerations can be found, slightly less developed, in the *Mechanics*.
[48] T. N.: Galileo implies here that work is the decisive factor for equilibrium. The entire concept of the Principle of Virtual Displacements is elucidated by this observation. Later, Torricelli will generalize this observation and give it status as a principle.
[49] T. N.: The Italian reads, *Discourses and mathematical demonstrations about two new sciences related to mechanics and to local motion.*
[50] Galileo Galilei, *Lettere al P. Ab. D. Benedetti Castelli*, December 3rd, 1639; reproduced in all the editions of Galileo's works.
[51] T. N.: The Italian reads, ". . . which was fully and conclusively demonstrated in an old treatise on mechanics already written in Padua by our Accademician solely for the use of his students . . .".
[52] *Eorum quae vehuntur in aquis experimenta* a Joanne Bardio Florentino *ad Archimedis trutinam examinata.* Romae, 1614. (T. N.: The Latin title reads, *Experiments on Bodies Moving in Water, studied using the Balance of Archimedes*, by the Florentine, Giovanni Bardi. Rome, 1614.)
[53] T. N.: The Latin reads, "The gravity under consideration here is that which some distinguish from dead weight, and which Galileo, like Jordanus, rightly calls a specific weight."
[54] Thurot, *Recherches historiques sur le principe d'Archimède* (Revue Archéologique, nouvelle série, Vol. XIX, 1869, p. 117).
[55] T. N.: The reference is to the *De incidentibus in humido* which is appended to the 1565 Tartaglia edition of the *De ratione ponderis*. It contains a definition of specific weight. Bardi must have mistakenly concluded that the *De incidentibus in humido* was also written by Jordanus.

## FOOTNOTES TO CHAPTER XII

[1] The statics of Stevin was first published in Flemish as *De Beghinselen der Weeghconst*, beschreven dver Simon Stevin van Brugghe. Tot Leyden, inde Druckerye van Christoffel Plantijn, bij Françoys van Raphelinghen. MDLXXXVI. (T. N.: The Flemish title reads, *The Principles of the Art of Weighing*, written by Simon Stevin of Brugge. Printed at Leyden by François van Raphelinghen at the press of Christoffel Plantijn, 1586.) It was divided into two parts, the first dealing with the general principles of statics and the second with the determination of various centers of gravity. Two other works were appended to it. One of them, entitled *De Weeghdaet*, dealt with the applications of statics, while the other, *De Beghinselen des Waterwichts*, dealt with the principles of hydrostatics.

Later, Simon Stevin published all of his mathematical works in Flemish under the title: *Wisconstige Gedachtenissen*, inhoudende t'ghene daer hem in gheoeffent heeft den Doorluchtichsten Hoochgheboren Vorst ende Heere, Mavrits Prince van Orengien, Grave van Nassau, . . . , Beschreven deur Simon Stevin van Brugghe. Tot Leyden, inde

Druckerye van Jan Bouvvensz, Int Iaer MDCVII. (T. N.: The Flemish title reads, *Mathematical Memoires* of Simon Stevin of Brugge containing as well those practiced by his most illustrious Prince and Lord, Prince Maurice of Orange, Count of Nassau, Leyden, in the press of Jan Bouvvensz, in the year 1608.)

The second volume of this collection contains his statics and is entitled: *Vierde Stvck der wisconstighe Gedachtnissen wande Weeghconst.* Tot Leyden, bij Jan Bouvvensz, Anno MDCV. (T. N.: The Flemish title reads, *The fourth Part of the Mathematical Memoires on the Art of Weighing.* Leyden, Jan Bouvvensz, 2605.) The first four books reproduced the work on statics published in 1586. Stevin adds a fifth book entitled: *Vanden Anwang der Waterwichtdaet,* (T. N.: The The Flemish title reads, *On the Origins of the Applications of Hydrostatics*) which deals with the applications of hydrostatics; an appendix titled, *Anhang der Weeghconst,* inde welcke onder anderen vveerleyt vvorden ettlicke dvvalinghen der wichtige ghedaenten. (T. N.: The Flemish title reads, *Appendix to the Art of Weighing,* in which among other things numerous errors pertaining to heavy bodies are refuted.) Finally there is a kind of supplement called *Byvorgh der Weeghconst.* (T. N.: The Flemish title reads, *Supplement to the Art of Weighing.*) This supplement has four parts, the first *Van het Tavwicht* (T. N.: The Flemish title reads, *On Spartosotatics*), deals with spartostatics (the equilibrium of ropes) and the second *Vant Catrolwicht* (T. N.: The Flemish title reads, *On Trochleostatics*), deals with trochleostatics or the equilibrium of pulleys.

This collection was also translated into Latin by Willebrord Snell and published under the title of *Hypomnemata mathematica.* The part which is of interest to us is entitled: *Mathematicorum Hypomnematum de Statica,* quo comprehenduntur ea in quibus se exercuit Illustrissimus illustrissimo atque antiquissimo stemmate ortus Princeps, ac Dominus, Mauritius Princeps Auraicus, comes Nassoviae, . . . conscriptus a Simone Stevino, Brugensi, Lugodini Batavorum, ex officina Joannis Patti; Anno MDCV. (T. N.: The Latin title reads, *Mathematical Memoires on Statics* by Simon Stevin of Brugge, containing as well those practiced by his most Illustrious Prince, born of the most illustrious and ancient parentage, Prince Maurice of Orange, Count of Nassau, . . . Leyden, Johann Patius; 1605.)

Finally, this collection was translated into French under the title: *Oeuvres mathématiques* de Simon Stevin de Burges, ou sont inserées les Memoires mathématiques esquelles s'est exercé le Très-haut et Très-illustre Prince Maurice de Nassau, Prince d'Aurenge, Gouverneur des Païs-bas unis, Général par Mer et par Terre, etc. Le tout revu, corrigé et augmenté par Albert Girard Samielois, Mathématicien. A Leyde, chez Bonaventure et Abraham Elsevier, Imprimeurs ordinaires de l'Université. Anno MDCXXXIV. Quatriesme volume traitant de l'art pondéraire ou de la Statique. (T. N.: The French title reads, *The Mathematical Works* of Simon Stevin of Brugge, which contain Mathematical Memoires of the most noble and illustrious Prince Maurice of Nassau, Prince of Orange, Governor of the Provinces of the United Netherlands, General at Sea and Land, etc. Reviewed, corrected and augmented by the mathematician, Albert Girard, St. Mihiel. Leyden, Bonaventure and Abraham Elsevier, Official Printers to the University, 1634. The fourth volume deals with the Art of Weighing or Statics.) Another translation in French, made by J. Tuning and published in Leyden in 1608, did not contain the part on statics. For further details refer to the *Bibliographie des Oeuvres de Simon Stevin* by Ferdinand Vanderhaegen in the Biblioteca Belgica. This work also provides the biographical information which we give in the text.

[2] T. N.: The "Vrije van Brugge" was a rural district surrounding the city of Brugge.

[3] Simonis Stevinis, *Mathematicorum Hypomnematum de Statica*, p. 81; Liber tertius de Staticae praxi; ad Lectorem. (T. N.: The Latin title reads, *Mathematical Memoires on Statics*, p. 81. Book IV, "On the Application of Statics; To the Reader.")

[4] Simon Stevin, Ibid., p. 150; Appendix Statices, ubi inter alia errores quidam *Staticon Idiomaton* refelluntur. (T. N.: Simon Stevin, Ibid., p. 150; Appendix on Statics, wherre among other things certain errors in the *Staticon Idiomaton* are refuted.)

[5] Simon Stevin, *Mathematicorum Hypomnematum de Statica*, p. 150; ad Lectorem. (T. N.: The Latin reads, To the Reader.)

[6] Simon Stevin, Ibid., p. 151; Caput I: Causam aequilibritatis situs non esse in circulis ab extremitatibus radiorum descriptis. (T. N.: Cf. footnote 7 for the translation of the Latin.)

[7] T. N.: Apparently, this is Albert Girard's translation from the Latin edition of Stevin contained in footnote 6. A closer rendering of the Latin in footnote 6 reads: Simon Stevin, Ibid., p. 151; Chapter I: The cause of the state of equilibrium is not in the arcs described by the extremities of the radii.

[8] In his awkward French, Albert Girard formulates this syllogism in the following way: What remains tranquil when suspended, does not describe a circumference. Two weights suspended in equilibrium are tranquil. Two weights in equilibrium thus do not describe a circumference.

[9] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, p. 151; Caput II: Res motas impedimentis suis non esse proportionales. (T. N.: The Latin reads, Simon Stevin, *Mathematical Memoires on Statics*, p. 151; Chapter II: Moving Bodies are not proportional to their resistances.)

[10] Simon Stevin, *Mathematicorum Hypomnematum de Statica*, Liber tertius, de Staticae praxi, p. 81; ad Lectorem. (T. N.: The Latin read, Simon Stevin, *Mathematical Memoires on Statics*, Book III, "On the Applications of Statics; p. 81; To the Reader.")

[11] Simonis Stevini, Ibid.

[12] Simon Stevin, *Mathematicorum Hypomnematum de Statica*.

[13] T. N.: The Latin reads, "The force of resistance, I say, since it is not a universal property, ought to be excluded from the precepts of statics because its ratio to the motor force is unique and not at all certain."

[14] T. N.: Most resistances, such as those due to friction, are small, and statical solutions which omit them generally provide solutions which are accurate enough for almost all practical problems.

[15] T. N.: The Latin "petitio principii" means "to beg the question."

[16] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, Liber primus Staticae elementis. (T. N.: The Latin title reads, Simon Stevin, *Mathematical Memoires on Statics*, Book I, "On the Elements of Statics.)

[17] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, pp. 12—13.

[18] Simon Stevin, Ibid., Liber primus Staticae elementis, p. 34.

[19] Add: "of equal size and weight."

[20] Stevin means by this: "arranged in such a manner that the descent of one forces the other to ascend." Taken literally, this statement by Stevin would contradict the developments which follow.

[21] T. N.: The word *sacoma* comes from the Greek word for weight. It is used by Willebrord Snell in place of the word "staltwicht" which is used by Stevin himself.
[22] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, Liber primus Staticae, de Staticae elementis, p. 35.
[23] Simon Stevin, loc. cit., 6 Consectarium, pp. 36 and 37. (T. N.: The Latin expression "Consectarium" means conclusions.)
[24] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, Liber primus Staticae, de Staticae elementis. 16 Theorema, 25 Propositio; p. 46.
[25] Simon Stevin, Ibid., *Additamentum Stasticae.* Pars prima: De Spartostatica; 3 Consectarium, p. 161. (T. N.: The Latin title reads, Simon Stevin, Ibid., *Addendum on Statics.* Part I: On Spartostatics; 3rd Conclusion, p. 161.)
[26] Simon Stevin, *Mathematicorum Hypomnematum de Statica*, Liber primus Staticae, de Staticae elementis; 9 Consectarium, p. 39.
[27] This title page is reproduced in: Mach, *Die Mechanik in ihrer Entwicklung*, 2. Auflage, Fig. 21, p. 28; Leipzig, 1889.
[28] "The miracle is no miracle."
[29] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, Liber primus Staticae, de Stasticae elementis, Postulata, p. 35.
[30] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, Additamentum Staticae. Additamenti Staticae pars secunda: De Trochleostatica; p. 169.
[31] Simon Stevin, loc. cit., p. 172.
[32] T. N.: The Latin reads, "As the displacement of the motive force is to the displacement of the resisting force, so is the force of the resisting body to the force of the moving body."
[33] T. N.: Galileo assumed that the Principle of Virtual Displacements was applicable to a moving body at an instant in time. Since the displacements can all be divided by this small unit of time, this is tantamount to applying the Principle of Virtual Velocities.
[34] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, Liber tertius Staticae, de Staticae Praxi, Prima Propositio: Infinitae potentiae formas et accidentia exponere; p. 107. (T. N.: The Latin reads, Simon Stevin, *Mathematical Memoires on Statics*, Book III, "On the Applications of Statics; First Proposition: To expound the forms and accidental qualities of an infinite force; p. 107.)
[35] Simon Stevin, loc. cit., p. 101.
[36] Heron of Alexandria, *Les Mécaniques ou l'Elévateur*, published for the first time in French and translated by Carra de Vaux from the Arabic version of Qostâ ibn Lûkâ, Paris, 1894, p. 39.
[37] Pappi Alexandrini, *Collectiones quae supersunt*, edidit F. Hultsch; Berolini, 1878. Volumen III, p. 1060. (T. N.: *The Extant Collections of Pappus of Alexandria*, edited by F. Hultsch, Berlin, 1878. Volume III, p. 1060.)
[38] Simonis Stevini, *Mathematicorum Hypomnematum de Statica*, Liber primus Staticae, de Staticae elementis, p. 6.
[39] Pappi Alexandrini, *Collectiones quae supersunt*, edidit F. Hultsch; Berolini, 1878. Volumen III, p. 1032.
[40] We indicated at the end of Chapter III that Libri claimed the invention of this law for Cardan, and we warned the reader against accepting this assertion. After Chapter III had gone to press, we were able to check the assertion by Libri and we found that in the passage which he quotes, Cardan is not talking about the law of the composition of

forces, but about the law of the composition of velocities, which was already known to the author of the *Mechanical Problems*. Curiously enough, Cardan thinks that this law is only valid for velocities which are perpendicular to one another. This example demonstrates what value should be given to information furnished by Libri.

[41] This principle never ceased to preoccupy the geometers of Antiquity and the Middle Ages. One of them attempted to justify it directly by a sort of generalization of the demonstration which the *Mechanical Problems* had given on the law of the lever. This generalization, which is very much influenced by Peripatetic thinking, rests on an attempt to define what must be understood as the magnitude of the motion of a segment of a line. It is contained in an anonymous fragment from the 13th century and was inserted after the *Liber Charastonis* in Ms. 8680A (Latin Collection) of the Bibliothèque Nationale (folio 6, recto to folio 7, recto).

[42] Bibliothèque Nationale (Latin Collection), Ms 7377B.

## FOOTNOTES TO CHAPTER XIII

[1] It goes without saying that all men of science were familiar with Latin and that the treatise written abroad could easily be read by French mechanicians thanks to the use of this truly marvelous universal language. The *Mecanicorum liber* of Guido Ubaldo was, in particular, one of those works with which men of science became familiar. In 1599, Henri Monantholius, physician and professor of mathematics, wrote a commentary (a) on the *Mechanical Problems* of Aristotle, where he not only quotes Cardan and the *Exercitationes* by Scaliger, but also frequently quotes the treatise of Guido Ubaldo.

(a) *Aristotelis Mechanica*, graeca, emendata, latina facta, et commentariis illustrata ab Henrico Monantholio, medico, et mathematicarum artium professore regio, ad Henricum III, Galliae et Navarrae regem christianissimum. Parisiis, apud Jeremiam Perier, via Jacobaeam, sub signo Bellerophontis. MDXCIX. (T. N.: The Latin title reads, *The Mechanics of Aristotle*, in Greek, corrected, translated into Latin and illustrated with commentary by Henri Monantholius, Royal Physician and Professor of Mathematics to Henry III, His Most Christian King of France and Navarre. Paris, Jeremia Perier, rue St. Jacques, under the sign of Bellerophon, 1599.)

[2] *Les raisons des forces mouvantes avec diverses machines tant utilles que plaisantes aus quelles sont adioints plusieurs desseings de grotes et fontaines*, by Salomon de Caus, engineer and architect to His Highness, the Palatine Electoral. Frankfort, in the shop of Jean Norton, 1615. (T. N.: The French title reads, *The Basis of the Moving Forces in Diverse Machines Both Useful and Amusing to which are added several Designs for Grottos and Fountains.*)

[3] In folio 4, verso, and in folio 5, recto.

[4] Salomon de Caus, *Les raisons des forces mouvantes*, folio 6, recto.

[5] Id., ibid., folio 7, recto.

[6] Salomon de Caus, *Les raisons des forces mouvantes*, folio 7, recto.

[7] *Les Méchaniques de Galilée*, translated by L.P.M.M. Espitre (sic) for Mr. de Reffuge, Counselor of the King to the Parliament.

[8] *Les Méchaniques de Galilée*, translated by L.P.M.M., p. 87.

[9] *Seconde partie de l'Harmonie universelle*, par F. Marin Mersenne; Paris,

MDCXXXVII. Nouvelles observations physiques et mathémathiques; Vth observation, p. 17.

[10] T. N.: Duhem has assumed that Jean Benoist is Mersenne's Gallicization of the name Giovanbattista Benedetti.

[11] *Les Méchaniques de Galilée*, translated by L.P.M.M., p. 87.

[12] *Synopsis mathematica*, ad clarissimum virum D. Jacobum Laetus, Doctorem medicum Parisiensem. Lutetiae, ex officina Rob. Stephani. MDCXXVI, cum privilegio Regis. — Le privilège royal est accordé au P. Marin Mersenne, religieux minime, dont le nom ne figure pas en titre. (T. N.: The Latin and French read, *Mathematical Synopsis*, to the distinguished Dr. Jacob Laetus, Dr. Med. at Paris. Paris, Robert Stephanus, 1626, with Royal privilege. — The Royal privilege was accorded to Father Marin Mersenne of the Minims, but his name does not appear in the title.)

[13] Nicéron, *Mémoires pour servir à l'histoire des hommes illustres*, Paris, 1736, vol. XXXIII, p. 150.

[14] T. N.: The Latin reads, *Euclid's Elements* and *On Spheres and Cosmography by Theodosius, Menelaus and Maurolycus.*

[15] T. N.: The Latin reads, "A spherical machine is the most mobile, and the bigger, the more mobile."

[16] T. N.: The Latin reads, "Wherefore, let us be encouraged in hope for this divine sphere whose center is said to be everywhere and whose circumference nowhere and which commands time to come from eternity, and while remaining stable allows the whole to be moved."

[17] Pascal, *Pensées*, Edition Havet, Art. I, I. E. Havet says: "Pascal probably took this expression from the preface (of Mademoiselle de Gournay to her edition of) the *Essays* of Montaigne. There she uses this expression, if we are to believe what Rabelais says, under the name of Trismegistus." It is obvious that Mersenne was familiar with this expression as early as 1626 since he frequently visited Étienne Pascal. Mersenne seems to have taken it from Nicolas Müller, who published in 1617 an annotated edition of the book of Copernicus, *On the Revolutions of the Celestial Orbs*. In this book, which Mersenne seems to have known, to judge by several passages from the *Synopsis mathematica*, Nicolas Müller expresses himself in the following way (a): "Forma rotunda omnium capacissima existit, perfectissima, motui aptissima, atque adeo, sola locum replet in quo movetur. Quoniam igitur mundus omnia capere debebat, seipsum motu assiduo conservare, et quidquid loci erat replere, merito formam rotundam illi attribuit summus Opifex ac Demiurgus. Rogatus quidam ut Deum definiret, haud inscite respondit: Deum esse sphaeram, cujus centrum sit ubique, superficies nusquam." (T. N.: The Latin reads, "The sphere is the most capacious of all forms, the most perfect, the most susceptible to motion, and moreover, it alone fills entirely the space in which it moves. And since the Universe had to contain everything and to maintain itself in constant motion and to fill any space it occupied, the Highest Maker and Demiurge rightly gave it a spherical form. And when someone was asked to define God, he not unskillfully responded: God is a sphere whose center is everywhere and whose surface is nowhere."

(a) Nicolai Copernici Torinensis *Astronomia instaurata, libris sex comprehensa, qui De revolutionibus Orbium Coelestium inscribuntur*, nunc demum post 75 ab obitu authoris annum integritati suae restituta, notisque illustrata, opera et studio D. Nicolai

Mulerii, Medicinae ac Matheseos professoris ordinarii in Nova Academia quae est Groningae. Amstelrodami, Excudebat Wilhelmus Jansonius, sub Solari aureo. Anno MDCXVII, p. 1: Notae breves, authore Nicolao Mulerio. (T. N.: The Latin reads, by Nicolas Copernicus of Thorn: Astronomy Restored, contained in six books entitled *On the Revolutions of the Celestial Orbs*: now at last restored to its integral form 75 years after the author's death and illustrated with notes through the efforts of Dr. Nicolas Müller, Professor of Medicine and Mathematics at the New Academy in Groningen. Amsterdam, Wilhelm Janson under the Sign of the Golden Sun, 1617, p. 1: "Brief Notes by the author Nicolas Müller.")

[18] T. N.: The Latin title reads, *On Hydrostatics and Things Pertaining to Water.*

[19] T. N.: The French title reads, *Useful and Marvelous Applications of the Circle to Mechanics.*

[20] T. N.: The Latin title reads, *On Oblique Weights and on the Forces of the Lever, the Balance, and other Similar Machines, also on Navigation and on the Mechanical Problems of Aristotle.*

[21] Father Marin Mersenne, *Synopsis mathematica, Mechanicorum libri*, p. 137.

[22] Id., ibid., p. 138.

[23] Father Marin Mersenne, *Synopsis mathematica, Mechanicorum libri*, p. 141.

[24] The triangle which has as its sides a line parallel to the inclined plane, the vertical and the horizontal.

[25] Paris, Morin and Libert, 1634.

[26] T. N.: The French title reads, "Letters written to Lord Morin by the most celebrated astronomers in France in approval of his invention to measure longitude and against the final verdict rendered on this subject by the gentlemen Pascal, Mydorge, Beaugrand, Boulanger and Hérigone, deputies commissioned to judge in this matter."

[27] *Cursus mathematicus*, nova, brevi, et clara methodo demonstratus, per notas reales et universales, citra usum cujuscunque idiomatis, intellectu faciles. — Cours mathématique démontré d'une nouvelle, briefve et claire méthode, par notes réelles et universelles, qui peuvent estre entenduës facilement sans l'usage d'aucune langue: par Pierre Hérigone, mathématicien. Paris, MDCXXXIV. (T. N.: The Latin and French titles read, *A Course on Mathematics*, demonstrated in a new, brief and clear method by means of real and universal symbols easily understood without the knowledge of any language, by Pierre Hérigone, mathematician. Paris, 1634.)

[28] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery: *Correspondance*, vol. V. p. 532.

[29] T. N.: The French title reads, *A Course on Mathematics*, containing the construction of tables of sines and logarithms, with their application for computing interest and for measuring right angle triangles; applied geometry; fortifications; militia; and mechanics.

[30] Hérigone, loc. cit., proposition I.

[31] Id., ibid., proposition II.

[32] Hérigone, loc. cit., propositions XV and XVI.

[33] Id., ibid., proposition VIII.

[34] T. N.: The Latin reads, "They will be in positional equilibrium."

[35] Bibliothèque Nationale, Ms. 8680 A (Latin collection).

[36] Hérigone, loc. cit., proposition VIII, corollary.

[37] Hérigone, loc. cit., p. 306.

[38] Hérigone, loc. cit., proposition XII.

[39] Joh. Alphonsi Borelli neapolitani, matheseos professoris, *Du motu animalium*; Paris

prima, Cap. XIII, Digressio ad Propositionem LXIX; Romae, MDCLXXX. (T. N.: The Latin title reads, *On the Motion of Animals*, by Giovanni Alphonso Borelli of Naples, Professor of mathematics; Part I, Chapter XIII "Digression on Propoosition LXIX," Rome, 1680.)

[40] Varignon, *Nouvelle Mécanique ou Statique* dont le projet fut donné en MDCLXXXVII. Tome second, p. 453. Paris, MDCCXXV.

[41] *Aristarchii Samii de Mundi systemate, partibus et motibus cujusdem, libellus*. Adjectae sunt A. E. P. de Roberval, Mathem. Scient. in Collegio Regio Franciae professoris, notae in eundem libellum. Parisiis, sumptibus vir. ampliss. Vaeneunt apud Antonium Bertier, via Jacobea, sub signo Fortunae; MDCXLIV. A second edition is added to the: *Novarum observationum physico-mathematicarum* F. Marini Mersenni, Minimi, tomus III; quibus accessit *Aristarchus Samius, de Mundi Systemate*; Parisiis, sumptibus Antonii Bertier, via Jacobea, sub signo Fortunae: MDCXLVII. (T. N.: The Latin title reads, *A Brief Treatise of Aristarchus of Samos on the System of the World, its Parts and Motions*, annotated by A. E. P. de Roberval, Professor of Mathematics in the Royal College of France, Paris, under the patronage of an illustrious gentleman; for sale with Antoine Bertier, rue St. Jacques, under the Sign of Fortuna; 1644 and *New Observations on Physics and Mathematics* by Father Marin Mersenne, of the Order of the Minims, Vol. III; to which is attached: Aristarchus of Samos' *On the System of the World*; Paris, under the patronage of Antoine Bertier, rue St. Jacques, under the Sign of Fortune, 1647.)

[42] Cf. Nicéron, *Mémoires pour servir à l'histoire des hommes illustres*, Paris, 1736; vol. XXXIII, p. 150.

[43] T. N.: The French title reads, *A Treatise on Universal Harmony Containing the Theory and Practice of Music among the Ancients and Moderns*.

[44] A Paris, chez Henry Guenon, ruë S. Jacques, près les Jacobins, à l'image S. Bernard, MDCXXXIV.

[45] T. N.: The French title reads, *Preludes to Universal Harmony or Curious Questions Useful for Preachers, Theologians, Astrologers, Physicians and Philosophers*.

[46] T. N.: The Latin title reads, *Books on Harmony by Father Marin Mersenne of the Order of the Minims*.

[47] T. N.: The Latin reads, *Four Books on the Harmony of Instruments*.

[48] T. N.: The Latin reads, "an expanded edition."

[49] A very detailed account of the *Harmonicorum libri* and the *Harmonie universelle* by Mersenne can be found in Brunet, *Manuel du Libraire et de l'Amateur de Livres*, 5th Edition, 1862, article "Mersenne," p. 1662. We owe this note to Mr. Paulin Richard, of the Bibliothèque Nationale. The copy in the Bibliothèque Municipale de Bordeaux enabled us to check the detailed accuracy of this note. Certain parts of the *Harmonie universelle* were printed or at least written before 1636. On the last page of his translation of the *Mechanics* of Galileo, printed in 1634, Mersenne refers to a passage of the first part of the *Harmonie universelle*.

[50] Since this part is of particular interest to us, we shall give its full title: *Harmonie universelle, contenant la théorie et la pratique de la Musique, où est traité de la nature des sons, et des mouvemens, des consonances, des genres, des modes, de la composition, de la voix, des chants et de toutes sortes d'instruments harmoniques*; par F. Marin Mersenne de l'ordre des Minimes. A Paris chez Sébastien Cramoisy, Imprimeur ordinaire du Roy, ruë S. Jacques, aux Cicognes, MDCXXXVI. (T. N.: The French title reads, *Universal Harmony, containing the theory and practice of music and treating the*

*nature of sound, motion, consonances, categories, modes, composition, voice, songs, and of all sorts of instruments of harmony*, by F. Marin Mersenne, of the Order of the Minims. Paris, Sébastian Cramoisy, official printer to the King, rue St. Jacques, at the sign of the Storks, 1636.

[51] T. N.: The French title reads, *Treatise on Mechanics; on weights supported by forces on inclined planes; forces which support a weight held by two ropes, by G. Persone de Roberval, Royal Professor of Mathematics at the College de Maistre Gervais and holding the chair of Ramus at the Collège Royal de France.*

[52] G. P. de Roberval, *Traité de Méchanique*, pp. 7 and 13.

[53] G. P. de Roberval, *Traité de Méchanique*, p. 21.

[54] G. P. de Roberval, *Traité de Méchanique*, pp. 24, 27, and 28.

[55] Cf. Chapter VIII, Section 2.

[56] G. P. de Roberval, *Traité de Méchanique*, p. 35.

[57] The two forces describe two different paths. Roberval is certainly talking about the average path which would be the path of the center of gravity of the two weights K and E.

[58] Marin Mersenne, *Harmonie universelle. A. Traitez de la nature des sons, et des mouvements de toutes sortes de corps.* Livre second. Des mouvements de toutes sortes de corps. Paris, MDCXXXVI. This proposition and the book on *Harmonie universelle* which contains it, are quoted by Mersenne on the last page of his *Mechanics* of Galileo, that is to say, as early as 1634. (T. N.: The French title reads, Marin Mersenne, *Universal Harmony. A. Treatise on the Nature of Sound and on the Motion of all Sorts of Bodies.* Book II, Motion of all Types of Bodies. Paris 1636.)

[59] *Les Préludes de l'Harmonie universelle*, ou Questions Curieuses, utiles aux prédicateurs, aux théologiens, aux astrologues, aux médecins et aux philosophes. By L.P.M.M. (Father Marin Mersenne). A Paris, chez Henry Guenon, rue S. Jacques, près les Jacobins, à l'image S. Bernard. MDCXXXIV. — Preface to the reader: "I have given the name of Preludes to the book, because it has essentially the same relation to the treatises on the other parts of music, which I shall publish soon, with the help of God, as the preludes on the lute . . ."

[60] Cf. Paul Tannery, *La Correspondance de Descartes dans les inédits du fonds Libri*; Paris, 1893.

[61] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. IV (July 1643 to April 1647), p. 391.

[62] F. Marini Mersenni, Minimi, *Tractatus mechanicus theoricus et practicus.* Parisiis, sumptibus Antonii Bertier, via Jacobea, sub signo Fortunae, MDCXLIV, p. 47. (T. N.: The Latin title reads, *Treatise on the Theory and Application of Mechanics* by F. Marin Mersenne of the Order of the Minims. Paris, under the Patronage of Antoine Bertier, rue St. Jacques, under the sign of Fortune, 1644, p. 47).

## FOOTNOTES TO CHAPTER XIV

[1] Descartes, *Oeuvres*, published by Ch. Adam and P. Tannery, Paris, 1897; *Correspondance*, Vol. I (April 1622 to February 1638), p. 393.

[2] T. N.: The Latin reads, "until you have repaid me."
[3] T. N.: The Latin terms are, "libra — balance, vectis — lever, and trochleon — block and tackle."
[4] Descartes, op. cit., p. 435.
[5] T. N.: The French title reads, *An Explication of Machines by means of which one can lift with a small force a very heavy load.*
[6] T. N.: Descartes uses *force* throughout this chapter when *work* would be more appropriate.
[7] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. I (April 1622 to February 1638), p. 461.
[8] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II (March 1638 to December 1639), p. 222.
[9] *Les Méchaniques de Galilée*, mathématicien et ingénieur du duc de Florence, avec plusieurs additions. Traduites de l'Italien par L.P.M.M. A Paris, chez Henry Guenon, MDCXXXIV, p. 57.
[10] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II (March 1638 to December 1639), p. 433.
[11] T. N.: The Latin reads, "How things are" but not "why they are that way."
[12] Id., ibid., p. 388.
[13] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II (March 1638 to December 1639), p. 247.
[14] Id., Vol. IV, Additions, p. 696.
[15] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. IV, Additions, p. 694.
[16] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. I, p. 443.
[17] T. N.: Descartes assumes that the weight at point F hangs vertically and the force at point A is tangent to the circle and remains tangent as it raises the weight at point F.
[18] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II (March 1638 to December 1639), p. 233.
[19] T. N.: Descartes is saying that the forces must remain constant in magnitude and direction during a virtual displacement. Hence, the requirement that it be an infinitesimal displacement.
[20] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II (March 1638 to December 1639): Letter to Mersenne of September 12th, 1638, p. 352.
[21] T. N.: Latin for "in the same quantity."
[22] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II (March 1638 to December 1639), p. 432.
[23] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II (March 1638 to December 1639), p. 352.
[24] Letter from Descartes to Mersenne, September 12th, 1638 (*Oeuvres*, published by Ch. Adam and Paul Tannery, Vol. II, p. 352).
[25] Letter from Descartes to Mersenne, September 12th, 1638 (*Oeuvres*, published by Ch. Adam and Paul Tannery, Vol. II, p. 433).
[26] Letter from Descartes to Mersenne, September 12th, 1638 (*Oeuvres*, published by Ch. Adam and Paul Tannery, Vol. II, p. 352).

[27] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. III (January 1640 to une 1643), p. 613.
[28] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. IV, Additions, p. 685.
[29] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. IV, Additions, p. 685.
[30] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II, p. 390: Letter from Descartes to Mersenne, October 11th, 1638.
[31] T. N.: The sense of the Latin is, "he misses the mark completely."
[32] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. III (January 1640 to June 1643), p. 243.
[33] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. II, p. 354: Letter to Mersenne, September 12th, 1638.
[34] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; *Correspondance*, Vol. I, p. 461: Letter from Constantine Huygens, November 23rd 1637.
[35] Pascal, *Pensées*, Edition Havet, Art. XXIV, no. 68.

## FOOTNOTES TO THE NOTES OF VOLUME I

### FOOTNOTES TO NOTE A

[1] T. N.: The Latin title reads, *The Book on Diverse Topics.*
[2] T. N.: The Latin reads, "to his son Heriston."
[3] T. N.: The Latin reads, "to Ariston."
[4] Valentin Rose, *Anecdota graeca et graeco-latina*, Vol. 2, Berlin, 1870; p. 299. — *Le livre des appareils pneumatiques et des machines hydrauliques par Philon de Byzance*, edited and translated into French by Baron Carra de Vaux, Paris, 1902. (T. N.: The Latin reads, *Greek and Greco-Latin Anecdotes*. The French title reads, *The Book on Pneumatic Apparati and Hydraulic Machines of Philo of Byzantium.*
[5] This is the hypothesis introduced by Steinschneider (*Mathematische Handschriften der amplonianischen Sammlung* in *Bibliotheca Mathematica*, Series 2, Vol. V, p. 46, 1891).
[6] Steinschneider, *Die Söhne der Musa ben Schakir* (*Bibliotheca Mathemtic*, Series 2, Vol. I, p. 71, 1887).

### FOOTNOTES TO NOTE B

[7] Rogerii Baconis Angli, viri eminentissimi, *Specula mathematica*, in qua de specierum multiplicatione earumque in inferioribus virtute agitue. Liber omnium scientiarum studiosis apprime utilis, editus opera et studio Johannis combachii, Philosophiae professories in Academia Marpurgensi ordinarii. Francofurti, typis Wolffgangi Richteri, sumptibus Antonii Hummii, MDCXIV. (T. N.: The Latin title reads, *The Mathematical Watch-Tower*, by the distinguished English scholar, Roger Bacon. This work treats of the multiplication of species and of their virtue in lower forms. An extremely useful book for students of all sciences, edited by Johann Combach, Professor at Marburg. Type-set by Wolfgang Richter, under the patronage of Anton Humm, Frankfurt, 1614.
[8] T. N.: Bacon uses the Latin term "distinctiones."

[9] T. N.: The Latin reads, "Therefore, Jordanus says, in his book on weights . . ."
[10] T. N.: The Latin reads, "this is against the doctrine of Jordanus and contrary to common sense."
[11] T. N.: The Latin title reads, *The Elements of Jordanus on the Demonstration of Weights.*
[12] T. N.: The Latin word translates as "invention."

## FOOTNOTES TO NOTE C

[13] Cf. on this subject: E. Mach, *La Mécanique, exposé historique et critique de son dévelopment*, translated into French by E. Bertrand, Paris 1904, p. 17ff. (Published in English as: *The Science of Mechanics. A Critical and Historical Account of Its Development*, translated by Thomas J. McCormack, The Open Court Publishing Co., 1960.)

O. Hölder, *Anschauung und Denken in der Geometrie*, Leipzig, 1909, p. 64.

G. Vailati, *La dimostrazione del principio della leva data da Archimede nel libro primo sull'equilibrio delle figure piane* (Atti del Congresso Internazionale di Science Storiche, Roma I-9 Aprile 1903, Vol. XII, p. 243). (T. N.: The Italian reads, "The Demonstration of the Principle of the Lever given by Archimedes in the First Book on the Equilibrium of Plane Figures," International Congress of Historical Science, *Proceedings*, Rome, 1—9 April 1903, Vol. 12, p. 243.)
[14] Lagrange, *Mécanique analytique*, First Part, Section 1, Art. 1.
[15] Vide supra, p. 198.

# FOOTNOTES TO THE ORIGINS OF STATICS VOLUME II

## FOOTNOTES TO THE PREFACE

[1] Giovanni Vailati, *Il principio dei lavori virtuali da Aristotele a Erone d'Alessandria* (*Accademia Reale Delle Scienze Di Torino*, vol. XXXII, la sessione dal 13 giugno, 1897). (T. N.: The Italian title reads, *The Principle of Virtual Work from Aristotle to Hero of Alexandria.*)
[2] See below, Note F.
[3] T. N.: The Latin title reads, *A demonstration of machines and instruments by which very heavy weights are lifted.*
[4] T. N.: The Latin designation reads, *Authorities on Weights.* It is sometimes translated as *Authors on Weights.*
[5] T. N.: Duhem is referring to the lines of force of the gravitational field.

## FOOTNOTES TO CHAPTER XV

[1] Lagrange, *Mécanique analytique*, 1[e] Partie, Section 1, No. 15.
[2] *Opera geometrica* Evangelistae Torricellii: *De solidis sphaeralibus: De motu; De dimensione parabolae; De solido hyperbolico, cum appendicibus de cycloide et cochlea.* (T. N.: The Latin title reads, *Works on Geometry* by Evangelista Torricelli: *On spherical solids; On motion: On the measurement of the parabola; On the hyperbolic solid with appendices on the cycloid and spiral.*) On the second page, the title *De sphaera et solidis sphaeralibus libri duo* is followed by this note: Florentiae, typis Amatoris Massae et

Laurentii de Landis: 1614. (T. N.: The Latin title reads, *Two books on the sphere and spherical solid*: Florence, type set by Amator Massa and Laurentis de Landis, 1614) The section which interests us in particular is entitled: *De motu gravium naturaliter descendentium et projectorum libri duo*, in quibus ingenium naturae circa parabolicam lineam ludentis per motum ostenditur, et universa projectorum doctrina unius descriptione semicirculi absolvitur. (T. N.: The Latin reads, *Two books on the Motion of Freely Falling Bodies and Projectiles*, in which is demonstrated the tendency of undisturbed nature to move along a parabolic path and in which is summarized the general doctrine of projectiles by means of the description of one semi-circle.) We shall also quote this other section: *De dimensione parabolae solidique hyperbolici problemata duo, antiquum alterum*, in quo quadratura parabolae XX modis absolvitur, partim geometricis, mecanicisque; partim ex indivisibilium geometria deductis rationibus; *novum alterum*, in quo mirabilis cujusdam solidi ab hyperbola geniti accidentia nonnulla demonstrantur. *Cum appendice, de dimensione spatii cycloidalis et cochleae.* (T. N.: The Latin reads, *Two Problems on the Measurement of the Hyperbolic and Solid Parabola*: An older one, in which the quadrature of the parabola is solved in 20 ways, some geometrical and mechanical, some by reasoning deduced from the geometry of indivisibles: A newer one, in which certain accidental properties of a solid marvelously derived from a hyperbolic are demonstrated. With an Appendix on the measurement of cycloidal bodies and spirals.)

3 Evangelistae Torricellii *De motu gravium naturaliter descendentium*, liber primus, p. 99. (T. N.: The Latin title reads, *First Book on the Motion of Freely Falling Bodies.*)

4 *De motu gravium naturaliter descendentium*, liber primus, Propositio I, p. 99 (T. N.: The Latin reads, *First Book on the Motion of Freely Falling Bodies*, Proposition I.)

5 Evangelistae Torricellii *De dimensione parabolae* . . . , Suppositiones et definitiones, p. 11 (T. N.: The Latin reads, *On the measurement of the parabola*, suppositions and definitions.)

6 Torricellii, loc. cit., p. 15.

7 Torricellii, loc. cit., p. 15.

8 Pappi Alexandrini *Collectiones quae supersunt e libris manuscriptis edidit Fridericus Hultsch*: Berolini, 1878. Liber VIII, Propos. I et II; Tomus III, p. 1301. (T. N.: The Latin reads, *The Extant Collected Works of Pappus of Alexandria edited from the Manuscripts by Friedrich Hultsch*: Berlin, 1878. Book VIII, Proposition I and II, Vol. III, p. 1301.)

9 Guidi Ubaldi e Marchionibus Montis *In duos Archimedis aequiponderantium libros paraphrasis, scholiis illustrata*. Pisauri, apud Hieronymum Concordiam, MDLXXXVIII, p. 9. (T. N.: The Latin reads, *A Paraphrase In Two Books of Archimedes' Equilibrium of Planes illustrated with Scholia*, by Guido Ubaldo, Marchese del Monte. Pesaro, published by Girolamo Concordia, 1588, p. 9).

10 Cf. Pappus, loc. cit., p. 1043.

11 Pappus, loc. cit., p. 1035.

12 P. Duhem, *Archimède a-t-il connu le paradoxe hydrostatique*? (*Bibliotheca Mathematica*, 3e Folge, Band I., p. 15: 1900.)

13 Pappus, loc. cit., p. 1030. (T. N.: The original Greek reads, . . . to the center of the Cosmos.)

14 Aristotle, *On the Heavens* Book II, Chapter XIV. Edition Didot, vol. II, pp. 407–409.

[15] Simplicius, *Commentaries on Aristotle's On the Heavens*, ed. by S. Karsten, 1865.
[16] T. N.: Centrobaric means literally, the heaviness of the center since in Greek "baros" means weight. Thus *centrobarics* refers to the science of the center of weight.
[17] Sancti Thomae Aquinatis Doctoris Angelici *Opera omnia* jussu impensaque Leonis XIII, P. M., edita. Tomus XIII. Romae MDCCCLXXXVI. *Commentaria in libros Aristotelis de Caelo et Mundo*. In librum II lectio XVII, p. 124. (T. N.: The Latin title reads, *The Complete Works of St. Thomas Aquinas*, published at the order and expense of Pope Leo XIII: Vol. XIII, Rome, 1886. *Commentaries on the Books of Aristotle's On the Heavens*. Study XVII on Book II, p. 124.)
[18] Aristotelis *De Caelo, de generatione et corruptione, meteorologicorum, de plantis*. Averrois Cordubensis cum variis in eosdem commentariis . . . Venetiis, apud Iuntas, MDLXXIIII. — De Caelo lib. II: Summa quarta: De Terra: Cap. 6: Terrae locum causamque quietis ejus exponit, p. 163. (T. N.: The Latin title reads, *Aristotle's On the Heavens, On Generation and Corruption, On Meteors, On Plants*. With various commentaries on the same by Averroes of Cordoba. Venice, 1574.) — *On the Heavens, Book II, First Part; On the Earth*, Chapter 6: Explanation of the location of the Earth and the cause of its immobility, p. 163.
[19] Beati Alberti Magni, Ratisbonensis Episcopi, ordinis Praedicatorum, *Physicorum lib.* VIII, *De Caelo et Mundo lib. IV. De generatione et corruptione lib. II, De meteoris lib. IV, De mineralibus lib. V*, recogniti per R. A. P. F. Petrum Iammy, sacrae theologiae doctoris, conventus Gratianopolitani, ejusdem ordinis, nunc primum in lucem prodeunt. Operum tomus secundus. Lugduni, sumptibus Claudii Prost, Petri et Claudii Rigaud frat., Hieronymi De la Garde, Joan. Ant. Duguetan filii, via mercatoria. MDCLI. De Caelo et Mundo, lib. II: Tractatus IV: De motu et quiete Terrae: Cap. X, p. 144. (T. N.: The Latin title reads, *The Blessed Albertus Magnus*, Bishop of Regensburg, of the Order of Preachers, *Book VIII on the Physics; Book IV On the Heavens; Book II On Generation and Corruption; Book IV On Meteors; Book V On Minerals*: ed., by Peter Jammy, Doctor of Theology of the Convent in Gratianopolis, of the same order, published now for the first time. Second volume of his works. Lyon. Printed by Claude Prost, the Brothers Peter and Claude Rigaud, Jerome De la Garde, Jean Anthony Duguetan: Via Mercatoria, 1651. *On the Heavens, Book II: Treatise IV, On the Motion and Immobility of the Earth*, Chapter X, p. 144.)
[20] Burleus *Super octo libros Physicorum*. Colophon: Et in hoc finitur expositio excellentissimi philosophi Gualterii de Burley Anglici in libros octo de physico auditu Aristotelis Stagerite (sic) emendata diligentissime. Impressa arte et diligentia Boneti Locatelli Bergomensis, sumptibus vero et expensis nobilis viri Octaviani Scoti Modoetiensis . . . Venetiis, anno salutis 1491, quarto nonas decembris. Folio 93. (T. N.: The Latin title reads, Burley, *On Eight Books of the Physics*. Colophon: This includes an exposition by the most excellent philosopher Walter Burley of England on eight books of the *Physics* by Aristotle of Stagira, most diligently emended and printed through the skill and diligence of Bonetus Locatellus of Bergamo and at the expense of the Nobleman Octavianus Scotus Modoetiensis . . . Venice, December 1491. Folio 93.)
[21] Jo. Duns Scoti Doctor. Subtilis, *in VIII lib. Physicorum Aristotelis Quaestiones et Expositio*, in celeberrima et pervetusta Parisiensium Academia ab ipso authore publice ex cathedra perlectae, nunc primum ex antiquissimo manuscripto exemplari, abstersis omnibus mendis, in lucem editae, et accuratis annotationibus illustratae a B. Adm. P. F. Francisco de Pitigianis Arretino, ord. Minorum . . . Venetiis, MDCXVII, apud Joannem

Guerilium, p. 382. (T. N.: The Latin title reads: *By the Subtle Doctor John Duns Scotus: Questions on and an Exposition of the Eight Books of the Physics of Aristotle*, first read publicly *ex cathedra* by the same author at the ancient and renowned University of Paris and now published for the first time from a copy of an ancient manuscript expurged of all errors and provided with accurate annotations by Francisco de Pitigianis Arretino of the Minorite Order . . . Venice, 1617 with Johan Guerilius, p. 382.) — The *Questions* attributed to Duns Scotus in this edition are certainly not by him, but were written at the end of the XIVth century by Marsilius of Inghen. We will analyze them in section 5.

[22] Joannis de Janduno *In libros Aristotelis de Caelo et Mundo quaestiones subtilissimae, quibus nuper consulto adjecimus Averrois sermonem de substantia orbis cum ejusdem* Joannis *commentario ac quaestionibus* . . . Venetiis, apud Hieronymum Scotum, 1552; p. 31. Quaest. XIV: An terra sit in medio mundi? (T. N.: The Latin title reads: *John of Jandun: Very Subtle Questions on Books from Aristotle's On the Heavens*, to which we have added by recent decision Averroes' discourse on the substance of the Universe together with his commentary or *Questions on John*. Venice, with Girolamo Scoto 1552; p. 31. Question XIV: Is the earth at the center of the Universe?)

[23] T. N.: The reference is to the previous assertion by Aristotle.

[24] B. Boncompagni, *Intorno al Tractatus Proportionum de Alberto de Sassonia*, Bulletino de Bibliografia e di Storia delle Scienze Matematiche e Fisiche, t. IV, p. 498; 1871.

[25] T. N.: The Latin reads, Here begins the *Parisian Treatise on Proportions* edited by Master Albert of Saxony. Praise be to God.

[26] *Acutissimae Quaestiones super libros de physica Auscultatione* ab Alberto de Saxonia editae. In quartum Physicorum quaestio V. (T. N.: The Latin title reads, *Very penetrating Questions on Books of the Physics*, edited by Albert of Saxony. *Question V on the 4th Book of the Physics.*

[27] Bibliothèque Nationale, fonds Latin, Ms. No. 14723. — Cf. Thurot, "Recherches historiques sur le Principe d'Archimède," 3e Article (*Revue Archéologique*, nouvelle série, vol. XIX, p. 119; 1869).

[28] Bulaeus (Du Boulay), *Historia Universitatis Parisiensis*, MDCLXVIII, vol. IV, pp. 361 et 958.

[29] Cf. Thurot, "Analyse d'un ouvrage de Ueberweg" (Revue Critique d'Histoire et de Littérature, vol. VI, p. 251: 1868).

[30] J. T. Graesse, *Lehrbuch einer Literargeschichte der berühmtesten Völker des Mittelalters*, 2te Abth., 2te Hälfte, p. 656.

[31] J. C. Adlung, *Fortsetzung und Ergänzungen zu C. G. Jöchers allgemeinen Gelehrten Lexico*, Bd. I., col. 450—456.

[32] U. Chevalier, *Répertoire des sources historiques du moyen âge*, Bióbibliographie, Paris, 1883. Colonne 59.

[33] U. Chevalier, loc. cit.

[34] Sbaralea, *Supplementum scriptorum Franciscanorum*, p. 723; 1806.

[35] T. N.: The Latin reads, *A Subtle Inquiry into Heaviness and Lightness.*

[36] T. N.: The Latin reads, *Questions on Books from the Physics of Aristotle.*

[37] T. N.: The Latin reads, "Little Albert."

[38] Egidius cum Marsilio et Alberto *De generatione* — *Commentaria* fidelissimi expositoris B. Egidii Romani *in libros de generatione et corruptione Aristotelis cum textu intercluso singulis locis.* — *Questiones item subtilissime* ejusdem doctoris *super primo libro de generatione*; nunc quidem primum in publicum prodeuntes — *Questiones quoque clarissimi doctoris Marsilii Inguem in prefatos libros de generatione — Item questiones subtilissime* magistri Alberti de Saxonia in *eosdem libros de gene*; nusquam alias impresse. — Omnia accuratissime revisa, atque castigata; ac quantum ars eniti potuit fideliter impressa. Colophon: Impressum Venetiis mandato et expensis nobilis viri Luceantonii de Giunta Florentini, Anno Domini 1518, Die 12 mensis Februarii.

En dépit des indications du titre, ce recueil avait été déja imprimé au moins deux fois: à Venise, en 1504 (B. Locatellus) et 1505 (G. de Gregoriis).

(T. N.: The Latin and French read, Egidus with Marsilius and Albertus *On Generation — A Commentary on Books from On Generation and Corruption of Aristotle*, including selected passages from that text, by the faithful expositor Egidus of Rome — Also Very Subtle Questions by the same Doctor on the First Book of On Generation, now published for the first time. — Also Questions on the Introductory Books of On Generation by the illustrious Doctor Marsilius of Inghen. also Very Subtle Questions on the same books of On Generation by Master Albert of Saxony; not appearing elsewhere in print. Everything most accurately revised and corrected; printed with greatest possible accuracy. Colophon: Printed in Venice at the mandate and expense of the Nobleman Lucis Antonio of Giunta of Florence, 12 Feb. 1518. Despite the claims in the title, this collection had already been printed at least twice: In Venice in 1504 by B. Locatellus and again in 1505 by G. de Gregorius.)

[39] T. N.: The Latin reads, *The Very Useful Logic of Albertucius. The Logic of the Most Excellent Professor of Sacred Theology, Master Albert of Saxony of the Order of St. Augustine*, by Master Aurelius Sanatus of Venice. Venice, at the expense and skill of the heirs of O. Scotus, 1522.

[40] T. N.: The Latin reads, *Treatise on proportions.*

[41] Cf. Boncompagni (*Bulletino di bibliografia e di storia delle scienze mathematiche e fisiche*, t. IV, p. 493; 1871); the *Tractatus proportionum* appeared in ten editions. Graesse (*Trésor de livres rares et précieux.* Vol. 1, p. 57) states that the *Questiones super quatuor libros Aristotelis de Caelo et Mundo* was printed in Pavia in 1481, in Venice in 1492 and 1497, in Paris in 1516, again in Venice in 1520. — Besides the collection just cited, we were able to consult the following three editions:

1st: *Questiones subtilissime* Alberti de Saxonia *in libros de Celo et Mundo.* Colophon: Expliciunt questiones preclarissimi Alberti de Saxonia super quatuor libros de celo et mundo Aristotelis diligentissime emendate per eximium artium et medicine doctorem Magistrum Hieronymum Surianum Venetum filium Domini Magistri Jacobi Suriani physici prestantissimi. Impresse autem Venetiis arte Boneti de Locatellis Bergomensis. Impensa vero nobilis viri Octaviani Scoti civis Modoetiensis. Anno Salutis nostre 1492, nono kalendarum novembris, ducante inclite principe Augustino Barbadico.

2nd: *Acutissime Questiones super libros de physica auscultatione ab* Alberto de Saxonia *edite; jamdiu in tenebris torpentes: nuperrime vero quam diligentissime a vitiis purgate: ac summo studio emendate*; et quantum eniti ars potuit fideliter impresse. —

Nicoleti Verniatis Theatini philosophi perspicacissimi contra perversam Averrois opinionem de unitate intellectus: et de anime felicitate Questiones divini: nuper castigatissime in lucem predeuntes — Ejusdem etiam de gravibus et levibus questio subtilissima — On the last page: Venetiis sumptibus heredum q.D. Octaviani Scoti Modoetiensis ac Sociorum 21 Augusti 1516.

3rd: *Quaestiones et decisiones physicales insignium virorum*:

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| Alberti de Saxonia in | *Octo libros physicorum,*<br>*Tres libros de caelo et mundo,*<br>*Duos lib. de generatione et corruptione.* |
| Thimonis in | *Quatuor libros meteorum,* |
| Buridani *in Aristotelis* | *Lib. de sensu et sensato.*<br>*Librum de memoria et reminiscentia,*<br>*Librum de somno et vigilia,*<br>*Lib. de longitudine et brevitate vitae.*<br>*Lib. de juventute et senectute.* |

*Recognitae rursus et emendatae summa accuratione et judicio Magistri* Georgii Lokert Scoti: *a quo sunt tractatus proportionum additi.* Venumdantur in aedibus Jodoci Badii Ascensii et Conradi Resch. — Au verso du titre, se trouve une *Epistola nuncupatoria et paraenetica* de Georges Lokert, avec ces deux dates: Ex praeclaro Montisacuti collegio idibus Januarii ad supputationem Curiae Romanae MDXVI. Et rursus e Sorbona ad kalen. Octo. MDXVIII. — L'ouvrage eut, en effet, à Paris, deux éditions, l'une en 1516, l'autre en 1518. (T. N.: 1st: *Very Subtle Questions on Books from On the Heavens* by Albert of Saxony. Colophon: Here are developed questions of the Most Illustrious Albert of Saxony on Four Books of On the Heavens of Aristotle, most diligently emended by the distinguished Doctor of Arts and Medicine, Master Hieronymus Surian of Venice, son of the outstanding physician, Master Jacob Surien. Printed in Venice by Bonetus de Locatellus of Bergamo at the expense of the Nobleman Octavian Scotus, 9 Nov. 1492 under the regime of the Illustrious Prince Augustinus Barbadicus.

2nd: *Very Keen Questions on Books of the Physics*, edited by Albert of Saxony; long since languishing in darkness, but most recently carefully corrected of errors and diligently emended and printed with the greatest possible accuracy. — Nicole Vernias, most brilliant philosopher: *Against the False View of Averroes on the Unity of the Mind*, Divine Questions on the Happiness of the Soul: recently corrected and published. By the same author: A Very Subtle Question on Heavy and Light Bodies. On the last page: Venice, the Heirs of Octavian Scotus, 21 Aug 1516.

3rd: *Questions and Judgements on Physical Matters* by Eminent Men:

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| Albert of Saxony on | *Eight Books of the Physics*<br>*Three Books of On the Heavens*<br>*Two Books of On the Generation and Corruption* |
| Themon on | *Four Books on Meteorology* |
| Buridan on Aristotle's | *Book on Sense and the Sensible*<br>*Book on Memory and Recollection* |

*Book on Sleep and Waking*
*Book on the Length and Shortness of Life*
*Book on Youth and Old Age*

Newly annotated and emended with utmost accuracy and judgement by Master George Lokert, and appended are Treatises on Proportions. For sale at the Office of Jodocus Badius Ascensius and Conrad Resch. On the reverse side of the title page is to be read: *Dedicatory Epistle* of George Lokert, with the following dates: 1516 and 1518. The book appeared in fact in Paris in two editions, one in 1516, the other in 1518.

[42] Alberti de Saxonia *Quaestiones in libros de physico Auditu*; in librum IV quaestio X. (T. N.: The Latin title reads, *Questions on Books from the Physics* of Albert of Saxony; Book IV, Question X.)

[43] The opinion rejected here by Albert of Saxony had been stated and asserted by Roger Bacon (a), who had quoted it as an example of a successful application of mathematics to the physical sciences.

Rogerii Baconis Angli, viri eminentissimi, *Specula mathematica* in qua de specierum multiplicatione, earumdemque in inferioribus virtute agitur. Liber omnium scientiarum studiosis apprime utilis, editus opera et studio Johannis Combachii. Philosophiae professoris in Academia Marpurgensi ordinarii. Francofurti, typis Wolffgangi Richteri, sumptibus Antonii Hummii. MDCXIV. — Distinctio IV. Caput XIV: An motus gravium et levium excludat omnem violentiam? Et quomodo motus gignat calorem? Itemque de duplici modo sciendi. — Cet ouvrage est un fragment, imprimé séparément, de l'*Opus majus* dédié, vers 1267, au pape Clément IV (Fratris Rogeri Bacon, ordinis Minorum, *Opus majus ad Clementem Quartum, Pontificem Romanum*, ex MS Codice Dubliniensi edidit S. Jebb, M. D.; Londini, ex typis Gulielmi Bowyer, MDCCXXXIII; pp. 103 et 104, marquées par erreur 99 et 100). Cf. Vol. I, p. 240.

[44] See Chapter X, Section 2.

[45] T. N.: Duhem is referring to the Nominalist School.

[46] Alberti de Saxonia, *Quaestiones in libros de physico Auditu*; in librum IV quaestio V.

[47] Alberti de Saxonia, *Quaestiones in libros De Caelo et Mundo*; in librum II quaestio XXIII.

[48] For the Scholastics, in general, "magnitude" meant what modern geometers mean by the word "volume." By "center of magnitude," Albert undoubtedly meant in a vague way, what we call today the "center of gravity of volume." (T. N.: Duhem's footnote confuses the issue. It seems clear that what Albert of Saxony calls "center of magnitude" is the geometrical center of the volume.)

[49] Albertus de Saxonia, loc. cit.

[50] Burleus, *Super octo libros Physicorum*, Venetiis, 1491; folio 93, col. d. (T. N.: The Latin title reads, Burley, *On Eight Books of the Physics*, Venice, 1491; folio 93, col. d.)

[51] Alberti de Saxonia, *Quaestiones in libros de physico Auditu*: in librum IV, quaestio V.

[52] Alberti de Saxonia, *Quaestiones in libros de physico Auditu*: in librum IV, quaestio V. *Quaestiones in libros de Caelo et Mundo*; in librum II, quaestio X.

[53] Alberti de Saxonia, *Quaestiones in libros de Caelo et Mundo*; in librum II, quaestio X.

[54] Alberti de Saxonia, *Quaestiones in libros de Caelo et Mundo*; in librum II, quaestio XXIII.
[55] Alberti de Saxonia, *Quaestiones in libros de physico Auditu*; in librum IV, quaestio VI.
[56] Id., ibid., quaestio VI.
[57] Alberti de Saxonia, *Quaestiones in libros De Caelo et Mundo*; in librum II, quaestio XXV.
[58] Alberti de Saxonia, *Quaestiones in libros de physico Auditu*; in librum IV, quaestio V.
[59] T. N.: Geographers thought that one of the earth's hemispheres was covered by water and the other was land.
[60] Thurot, "Recherches historiques sur le Principe d'Archimède," 3e article. (*Revue Archéologique, nouvelle série*, t. XIX, p. 119; 1869). (T. N.: The French title reads, *Historical Inquiry on the Principle of Archimedes.*)
[61] Alberti de Saxonia, *Quaestiones in libros De Caelo et Mundo*; in librum III quaestio III. — Cf. ibid., in librum I quaestio X.
[62] Alberti de Saxonia, *Quaestiones in libros De Caelo et Mundo*; in librum III, quaestio III.
[63] Alberti de Saxonia, *Quaestiones in libros de physico Auditu*; in librum IV, quaestio X.
[64] Alberti de Saxonia, *Quaestiones in libros De Caelo et Mundo*; in librum I, quaestio X.
[65] T. N.: The Latin reads, A falling body is heavier the straighter its motion is toward the center.
[66] *Liber* Jordani Nemorarii, viri clarissimi, *de ponderibus*, propositiones XIII, et earumdem demonstrationes, multarumque rerum rationes sane pulcherrimas complectens, nunc in lucem editus. Cum gratia et privilegio imperiali, Petro Apiano mathematico Ingolstadiano ad XXX annos concesso. MDXXXIII. Pages six and seven (title included) of the work, printed without pagination. (T. N.: Cf. Vol. I, p. 96.)
[67] Alberti de Saxonia, *Quaestiones in libros De Caelo et Mundo*; in librum III, quaestio XI.
[68] Cf. Chapter X, Section 1.
[69] Aristotle, *On the Heavens*, II, XIV.
[70] Cf. P. Tannery, "Recherches sur l'Histoire de l'Astronomie ancienne" (*Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux*, 4e série, t. I, p. 110: 1893).
[71] Aristotle, Book II, Chapter IV, Edition Didot, vol. II, p. 394. (T. N.: Cf. the Loeb edition *On the Heavens*, W. K. C. Guthrie, pp. 160—161.)
[72] Theon of Smyrna, Platonic philosopher, *An Exposition of mathematical knowledge useful for reading Plato*, translated for the first time from Greek to French by J. Dupuis: Paris, 1892. Part three: Astronomy. *On the spherical shape of the earth*, pp. 198ff.
[73] P. Duhem, "Archimède a-t-il connu le paradoxe hydrostatique?" (*Bibliotheca Mathematica*, 3te Folge, Bd. I., p. 15, 1900).
[74] Heronis Alexandrini *Spiritalium liber*, a Federico Commandino Urbinate ex graeco nuper in latinum conversus: Urbini, MDLXXV; p. 12, verso, et p. 13, recto. (T. N.: The Latin title reads, Hero of Alexandria, *Book on Pneumatics*, recently translated from Greek into Latin by Frederico Commandino of Urbino, 1575; p. 12 verso, and p. 13, recto.)

[75] C. Plinii Secundi *Historia naturalis*; lib. II. (T. N.: The Latin title reads, Pliny the Elder, *Natural History*; Book II.)
[76] *The Almagest*, Book I, Chapt. III.
[77] Simplicii *Commentarius in IV libros Aristotelis de Caelo*, recensione Sim. Karsteni; Trajecti ad Rhenum, MDCCCLXV; pp. 242 et suiv. (T. N.: The Latin title reads, Simplicius' *Commentary on Four Books of Aristotle's On the Heavens*, ed. by Sim. Karsten; Utrecht, 1865, pp. 242ff.)
[78] Simplicii *Commentarius in IV libros Aristotelis de Caelo*, recensione Sim. Karsteni; Trajecti ad Rhenum, MDCCCLXV; p. 186.
[79] Aristotelis *De Caelo, de generatione et corruptione, meteorologicorum, de plantis, cum* Averrois Cordubensis *commentariis*; Venetiis, apud Juntas, MDLXXIII. *De Caelo*, lib. II; Summa quarta: de Terra; Cap. 7, pp. 165—172. (T. N.: Cf. Footnote 18, p. 8, Chapter XV.)
[80] Averroes, *Op. cit., De Caelo*, lib. II; Summa secunda: de circulari corpore; Quaesitum tertium, pp. 114—115. (T. N.: The Latin reads, Second part: On a circular body; 3rd Question, pp. 114—115.)
[81] Johannes de Sacro-Bosco, *De Sphaera*, Cap. I.
[82] T. N.: Duhem must be referring here to Albert's commentaries on Aristotle's *On the Heavens*.
[83] Sancti Thomae Aquinatis, Doctoris angelici *Opera omnia* jussu impensaque Leonis XIII. P. M., edita. Tomus III. Romae, MDCCCLXXXVI. *Commentaria in libros Aristotelis de Caelo et Mundo*: in lib II lectio XXVII, p. 224.
[84] Id., ibid., in lib. II lectio VI, p. 143.
[85] Rogerii Baconis *Specula mathematica*. Distinctio IV. Caput IX: De figura mundi. — *Opus majus*, édit. Jebb, p. 93 (T. N.: The Latin title reads, Roger Bacon, The Mathematical Watch-Tower. Distinction IV. Chapt. IX: On the Shape of the World. *Opus Majus*, ed. by Jebb, p. 93).
[86] Id., ibid., Caput X: Quod plus aquae contineat vas inferiori, quam superiori loco positum. *Opus majus*, édit. Jebb, p. 97 (T. N.: The Latin reads, That a vase contains more water the lower it is positioned.).
[87] Alberti de Saxonia *Questiones in libros de Caelo et Mundo*; in librum II, quaestio XXVII (Ed. 1492) vol. XXV (Ed. 1508).
[88] Cf. Paul Tannery, "Recherches sur l'Histoire de l'Astronomie ancienne" (*Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux* 4[e] série, t. I, p. 104; 1893).
[89] Alberti de Saxonia *Quaestiones in libros de Caelo et Mundo*; in librum III, quaestio ultima.
[90] T. N.: To make sense, the first trajectory should refer to the semi-circumference and the second to the diameter.
[91] T. N.: The Latin reads, *Dedicatory Epistle*.
[92] T. N.: The Latin title reads, *Questions on Four Books on Meteorology. Compiled by that learned Professor of Philosophy, Themon Judaeus*.
[93] Bulaeus, *Historia Universitatis Parisiensis*, MDCLXVIII, t. IV, p. 991.
[94] Thimonis *Quaestiones in libros Meteorum*; in librum I quaestio V. (T. N.: The Latin title reads, Themon, *Questions on Books in the Meteorology*; Book I, Question V.)
[95] Id., ibid., in librum II quaestio I.
[96] Themon, loc. cit.

[97] Nicolas of Lyre was born in Neuve-Lyre (Eure, about 1270): in 1291, he was a Franciscan in Verneuil; he died in Paris in 1340. His commentaries have been printed often: Nicolai Lyrani, *Postillae perpetuae in vetus et novum Testamentum*; Romae, 1471—1472. *Biblia sacra latina cum postillis* Nicolai de Lyra; Venetiis, 1481. Nicolai de Lyra *Postillae morales seu mysticae super Bibliam*; Mantuae, 1481. *Moralia super totam Bibliam* fratris Nicolai de Lira; Argentorati, circa 1479; etc. (T. N.: The Latin reads, Nicolas of Lyre, *Running Exegeses on the Old and New Testament*; Rome, 1471—1472. *The Holy Bible with Exegeses* by Nicolas of Lyre; Venice, 1481. Nicolas of Lyre, *Moral or Mystical Exegeses on the Bible*; Mantua, 1481. *Moral comments on the Entire Bible* by Brother Nicolas of Lyre; Strasburg, circa 1479; etc.)

[98] Fr. Junctini Florentini, sacrae theologiae doctoris, *Commentaria in Sphaeram Joannis de Sacro Bosco accuratissima*. Lugduni, apud Philippum Tinghium, MDLXXVIII; p. 178. (T. N.: The Latin title reads, Brother Giuntini of Florence, Doctor of Sacred Theology *Very Precise Commentaries on John of Sacrobosco's On the Sphere*; Lyon, Philipp Tinghius, 1578; p. 178.)

[99] Alberti de Saxonia *Quaestiones in libros de Caelo et Mundo*: in librum II quaestio XXV.

[100] Themon, loc. cit.

[101] *Quaestiones subtilissimae* Johannis Marcilii Inguen *super octo libros Physicorum, secondum nominalium viam*, cum tabula in fine libri posita; suum in lucem primum sortiuntur effectum. — Colophon; Expliciunt quaestiones super octo libros Physicorum magistri Johannis Marcilii Inguen secundum nominalium viam. Impressae Lugduni per honestum virum Johannem Marion. Anno Domini MDXVIII, die vero XVI mensis Julii, Deo gratias. We noted previously (p. 269 and footnote 21.) how, in 1617, the *Questions* of Marsilius of Inghen were attributed to Duns Scotus. (T. N.: The Latin title reads, John Marsilius of Inghen, *Subtle Questions on Eight Books of the Physics*, in the fashion of the Nominalists, with Tables at the End of the Book; published for the first time. Colophon; Here end the Questions on Eight Books of the Physics in the fashion of the Nominalists by Master John Marsilius of Inghen. Printed in Lyon by the Honorable John Marion. 16 July 1517, by the Grace of God.).

[102] T. N.: The Latin reads, *In the fashion of the Nominalists.*

[103] Johannis Marcilii Inguen *Quaestiones in libros Physicorum*; circa librum IV quaestio V.

[104] T. N.: The Latin reads, A fifth way is that which Campanus proposes in his treatise *On the Sphere.*

[105] Campani, *Tractatus de Sphaera*: Cap V. Quare Sphaera non sit integra. (T. N.: The Latin title reads, Why a Sphere is not uniform.)

[106] Johannis Marcilii Inguen *Quaestiones in libros Physicorum*; circa librum IV quaestio VIII.

[107] Johannis Marcilii Inguen *Quaestiones in libros Physicorum*: circa librum IV quaestio XI.

[108] T. N.: The Latin title reads: *Song of Weights.* The book was written ca. 500 A.D.

[109] Alberti de Saxonia *Quaestiones in libros de Caelo et Mundo*: in librum III quaestiones I et II.

[110] T. N.: The Latin reads, A threefold weight placed in equilibrium with another and exerting a uniform and unitary resistance, cannot be lifted by less than a threefold weight.

[111] Alberti de Saxonia *Quaestiones in libros de physico Auditu*: in librum IV quaestio X. — Johannis Marcilii Inguen *Quaestiones in libros Physicorum*: circa librum IV quaestio IX.

[112] Alberti de Saxonia *Quaestiones in libros de Caelo et Mundo*: in librum I quaestio X.

[113] The following is a documentation of the kinds of collections we consulted:

1°. Barthol. Vespuccio (Florent.) *De laudibus Astrologiae. — Textus Spherae* Joa. de Sacro-Bosco. — Capuani de Manfredonia *Expositio Sphaerae.* — Jac. Fabri Stapulenis *Comment. in Sphaeram,* — Petri de Aliaco card. *Questiones* XIIII — Roberti Linconiensis episc. *Compendium Sphaerae. — Disput.* Joa. de Regio Monte *contra Cremonensia deliramenta* — Fr. Capuani *Theoricarum novarum textus cum expositione.* Colophon: Venetiis, per Jo. Rubeum et Bern. fratres Vercelli. ad instant. Junctae de Junctis. 1508.

2°. *Sphaera, cum commentis in hoc volumine contentis, videlicet*: Cichi Esculani *cum textu* — Expositio Joan. Baptistae Capuani *in eandem.* — Jacobi Fabri Stapulensis. — Theodosii *De Sphaeris* — Michaelis Scoti — *Questiones* reverendissimi Domini Petri de Aliaco, *etc.* — Roberti Linchoniensis *Compendium. — Tractatus de sphaera solida. — Tractatus de computo majori* ejusdem. — *disputatio* Joannis de Monteregio. — *Textus theoricae cum expositione* Joannis Baptistae Capuani. — Ptolemeus *de speculis.* — Colophon: Venetiis, impensa haeredum quondam Domini Octaviani Scoti Modoetiensis ac sociorum: 19 Januarii 1528.

3°. *Sphaerae tractatus* Jo. de Sacro Busto (sic!). — Gerardi Cremon. *Theoricae planetarum.* — G. Purbachii *Theor. planet.* — Prodoscimi de Beldomando Patav. *Comm. sup. tractatu sphaerico.* — Joannis Bapt. Capuani *Expos. in sphaera.* — Mich. Scoti *Expositio in sphaera.* — Jac. Fabri Stapulensis *Annotat.* — Campani *Comp. s. tract. de sphaera. — De modo fabricandi sphaeram solidam.* — Petri card. de Aliaco *XIV quaestiones.* — Roberti Linconiensis *Tractatus de sphaera.* — Bartholomei Vesputii *Gloss.* — Lucae Gaurici *Castigat.* — Ejusdem *Num quid sub aequatore sit habitatio.* — Ejusdem *De inventoribus Astrologiae.* — Alpetragii Arabi *Theor. planetarum.* — Venetiis, Luc. et Ant. Juntae, 1531.

(T. N.: The Latin titles read,

1°. Bartholomo Vespuccio of Florence, *On the Merits of Astrology* — John of Sacro-Bosco, *A Text on the Sphere.* — Capuano of Manfredonia, *Description of the Sphere.* — Jacob Faber Stapulensus, *A Commentary on the Sphere.* — Pierre d'Ailly, Cardinal, *Fourteen Questions.* — Johannes Regiomontanus, *Argument against the Absurdities of Cremona.* — Brother Capuano, *Text with an Exposition of New Speculations.* Colophon: Venice, by Johannes Rubeus and the Vercelli Brothers with the support of the Giunta. 1508.

2°. *On the Sphere*, with the following contents in this volume: a *Text* of Cichus Esculanus. — *An Exposition on the Sphere* by Giovanbattista de Capuano. — Jacob Faber Stapulensis. — Theodosius, *On Spheres* — Michael Scotus. — The Most Reverend Pierre d'Ailly, *Questions*, etc. — Robert Linconien, *Compendium. — Treatise on the Solid Sphere. — Treatise on the Calculation of the Sphere.* Johannes Regiomontanus, *Argument.* Giovanbattista de Capuano, *Text with an Exposition of Speculations.* Ptolemy, *On Mirrors.* — Colophon: Venice, at the expense of the Heirs of Octavian Scotus Modoetiensis and Associates: 19 Jan 1528.

3°. John of Sacro-Bosco, *A Treatise on the Sphere.* — Gerard of Cremona, *Specula-*

*tions on the Planets.* — G. Purbach, *Speculations on the Planets.* — Prosdocimo de Beldamandi of Padua, *Commentary on the Treatise on the Sphere.* — Giovanbattista de Capuano, *Exposition on the Sphere.* — Michael Scotus, *Exposition on the Sphere.* — Jacob Faber Stapulensis, *Annotations.* — Campanus, *Compendium on the Treatise on the Sphere.* — *On the Method of Constructing a Solid Sphere.* — Pierre d'Ailly, Cardinal, *Fourteen Questions.* — Robert Linconium, *Treatise on the Sphere.* — Bartholomo Vespuccio, *Glossary.* — Luca Gauricus, *Castigations.* — By the same author, *Is there Any Habitation below the Equator*? — The same author, *On the Discoveries of Astrology.* — Alpetragius the Arab, *Speculations on the Planets.* Venice, Luc. and Antonio Giunti, 1531.)

114 In some cosmological collections, he is called *Sipontinus*, i.e., a resident of Santa Maria di Siponto (Maria-Siponto). Sometimes instead of *Giovanni Battista*, he is given the first name *Francesco.* (See on this subject: Riccardi, *Biblioteca matematica italiana*, Part. 1, vol. 1, col. 238—240: Modena, 1870).

115 T. N.: The Latin title reads: *Most subtle questions on the Physics.*

116 Augustini Niphi philosophi Suessani *Expositiones super octo Aristotelis Stagiritae libros de physico Auditu* ... Venetiis, apud Hieronymum Scotum, MDLVIII. Physicorum liber quartus, p. 307. (T. N.: The Latin title reads, Agostino Nifo, philosopher of Sessa Aurunca, *Expositions on Eight Books of the Physics of Aristotle the Stagirite*; Venice, Girolamo Scoto, 1558. Book IV of the Physics, p. 307.)

117 T. N.: The Latin reads, "conditionally lower."

118 T. N.: The Latin reads, "actually lower."

119 Gaëtan of Tiène, born in Venice, taught philosophy in Padua where he died in 1465. He should not be mistaken for Gaëtan of Tiène, born in Vicence in 1480 and who died in 1547. The latter was the founder of the Order of the Théatins and was canonized. The former should also not be mistaken for the famous Cardinal Caietan (1469—1534).

120 *Recollectae* Gaietani *super octo libros Physicorum cum annotationibus textuum.* In fine: "Impressum est hoc Venetiis per Bonetum Locatellum, jussu et expensis nobilis viri Domini Octaviani Scoti civis Modoetiensis. Anno salutis 1496 — Lib. IV, quaestio 1. (T. N.: The Latin title reads, Gaëtan of Tiène, *Collected Works on Eight Books of the Physics*, with annotations of the texts. At the end: Published in Venice by Bonetus Locatellus, at the order and expense of the nobleman Octavian Scotus, 1496 — Book IV, Question 1.)

121 Alexandri Achillini Bononiensis *Quatuor libri de Orbibus*; Bononiae. Impensis Benedicti Hectoris Bononiensis, MCDXCVIII; Liber primus, dubium tertium. — Alexandri Achillini Bononiensis, philosophi celeberrimi, *Opera omnia, in unum colelcta* ... omnia post primas editiones nunc primum emendatiora in lucem prodeunt. Venetiis, apud Hieronymum Scotum, MDXLV; p. 29. (T. N.: The Latin title reads, Alessandro Achillini of Bologna, *Four Books on the Orbits*; Bologna, at the press of Benedict Hector of Bologna, 1498. Book I, 3rd Uncertainty. — Alessandro Achillini of Bologna, celebrated philosopher, *Complete Works in One Volume* ... the entirety revised according to the First Editions and now printed for the first time. Venice, Girolamo Scoto, 1545; p. 29.)

122 Sbaralea (*Supplementum scriptorum Franciscanorum*, pp. 312—313) et, d'après lui, U. Chevalier (*Répertoire des Sources historiques du moyen âge; Bio-bibliographie*, col.

927) font de Grégoire Reisch un franciscain. Brunet (*Manuel du libraire et de l'amateur de livres*, Paris, 1863. t. IV. col. 1200) lui attribue, par erreur, le prénom de Georges. (T. N.: The Latin title reads: *An Addendum of Franciscan Writers.*)

123 Panzer in the *Annales Typographiques* and Hain in his *Repertorium* quote an edition bearing neither date nor place of publication, but containing the following remarks inside the work: Ex Heidelberga III kal. Januarii 1496. (T. N.: The Latin reads, From Heidelberg January 1496.)

124 T. N.: The Latin reads, *The Philosophical Pearl* containing the principles of all of rational, natural and moral philosophy in twelve books of erudite dialogue.

125 Besides the edition which we just cited, Brunet (loc. cit.) mentions the editions of Freibourg, 1503, of Strasbourg, 1504, 1508, 1512 and 1515, Basel in 1534 and 1583. The edition which was consulted is in the Municipal Library of Bordeaux and is by Johannes Schottus, Basileae, 1517.

126 *Margarita filosofica* del R. P. F. Gregorio Reisch, nella quale si trattono tutte le dottrine comprese nella ciclopedia, accrescinta di molte belle dottrine da Orontio Fineo matematico Regio. Di novo tradotta in Italiano da Gio. Paolo Galluci Salodiano, Accademico Veneto, et accresciuta di molte cose. In Vinegia, 1599; presso Barezzo Barezzi e Compagni. This same edition, with only the title page changed, was also sold: in Venetia, MDC: appresso Jacomo Antonio Somascho. (T. N.: The Italian reads, *The Philosophical Pearl* in which all the doctrines included in the encyclopedia are treated, augmented by the many beautiful doctrines of Orontio Fineo, Royal mathematician. Recently translated in Italian by Giovanni Paolo Galluci Salodiano, Venetian academic, and augmented with many notes. In the province of Venice, 1599. At Barezzo Barezzi and Company.)

127 Fr. Junctini Florentini, sacrae theologiae doctoris. *Commentaria in Sphaeram Joannis de Sacro-Bosco accuratissima*; Lugduni, apud Philippum Tinghium, MDLXXVIII, p. 178.

128 Cf. P. Duhem, *Albert de Saxe et Léonard de Vinci* (*Bulletin Italien*, Vol. V, p. 1 et p. 113; 1905).

129 T. N.: The Latin reads: On the center of gravity.

130 *Les Manuscrits de Léonard de Vinci*, publiés par Ch. Ravaisson-Mollien: Ms. F. de la Bibliothèque de l'Institut. Paris, 1889.

131 T. N.: The Latin reads, Albertucco and Marliani, on computation. Albert, On the Heavens, by Brother Bernadino.

132 T. N.: The Latin reads, *On the ratio of motions with respect to velocity.*

133 Eugène Müntz, *Léonard de Vinci, l'artiste, le penseur, le savant*, p. 308 (en note); Paris, 1899.

134 *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien: Ms. F, fol. 82, recto: Ms. G, fol. 54, recto.

135 B. Boncompagni, *Intorno ad un comento di Benedetto Vittori, medico Faentino, al Tractatus proportionum di Alberto di Sassonia* (*Bulletino di Bibliografia e di Storia delle Scienze Matematiche e Fisiche*, t. IV, p. 493; 1871). (T. N.: The Italian title reads: *On a commentary of Benedetto Vittori, Physician of Faenza on the Tractatus proportionum of Albert of Saxony.*)

136 *Les Manuscrits de Léonard de Vinci*, Ms. F, fol. 26, recto et fol. 51, verso. These fragments have been reprinted in the footnote to Chapter II.

[137] *Les Manuscrits de Léonard de Vinci*, Ms. I, fol. 120 (72), recto.
[138] T. N.: The Latin title reads, *Questions on certain books from On the Heavens.*
[139] P. Duhem, *Albert de Saxe et Léonard de Vinci* (*Bulletin Italien*, t. V. p. 1 et p. 113, 1905).
[140] *Les Manuscrits de Léonard de Vinci*, Ms. F, fol. 54, recto.
[141] It seems to me easy to determine what Leonardo meant by center of accidental gravity. For many Scholastics, accidental gravity is what is usually called "impeto" by Leonardo. This vague notion corresponds, more or less, to our contemporary notions of acquired velocity, quantity of motion and kinetic energy. Just as natural gravity is situated at one point for Leonardo, so is the center of natural gravity like accidental gravity concentrated at the center of accidental gravity. If the falling body includes the center of the Universe, it remains at rest there and the accidental gravity disappears within this center. — Cf. our study on *Bernardino Baldi, Roberval et Descartes* which will soon appear in the *Bulletin Italien.*
[142] *Les Manuscrits de Léonard de Vinci*, Ms. F, fol. 70, recto.
[143] Ibid., folio 84, recto.
[144] Alberti de Saxonia, *Quaestiones in libros de Caelo et Mundo*; in librum II quaestio XXVIII (Ed. 1492) vel XXVI (Ed. 1518).
[145] T. N.: The Latin reads, *Every heavy body tends to move downward nor can it remain perpetually elevated. Therefore, the entire earth ought to be spherical and entirely covered with water.*
[146] *Les Manuscrits de Léonard de Vinci*, Ms. F. folio 84, recto.
[147] Ibid., folio 52, verso.
[148] Leonardo da Vinci, *Del moto e misura dell'acqua*; contained in: *Raccolta d'autori Italiani che trattano del moto dell'acqua*; edizione quarta, arrichita di molte cose inedite e d'alcuni schiarimenti. Tomo X, pp. 271—450. Bologna, 1826. (T. N.: The Italian reads, *On the motion and measure of water*; contained in *A Collection of Italian authors dealing with the motion of water*; Fourth Edition, augmented by many unpublished matters and with some explanations.)
[149] *Les Manuscrits de Léonard de Vinci*, Ms. F, folio 11, verso.
[150] *Les Manuscrits de Léonard de Vinci*, Ms. F, folio 69, verso.
[151] Ibid., recto. — Cf. *Del moto e misura dell'acqua*, libro I, capitolo XXIII.
[152] *Les Manuscrits de Léonard de Vinci*, Ms. F, folio 51, recto.
[153] Libri, *Histoire des Sciences mathématiques en Italie*, t. III, p. 41; 1840.
[154] *Les Manuscrits de Leonard de Vinci*, Ms. F, folio 82, verso. Cf. *Del Moto e misura dell'acqua*, libro I, capitolo V.
[155] *The Codice Atlantico* contains a list of books owned by Leonardo. Among them a book by Pliny. (Cf. E. Müntz, *Léonard de Vinci, l'artiste, le penseur, le savant*, p. 282). (T. N.: The French title reads, *Leonardo da Vinci, the Artist, the Thinker, the Scientist.*)
[156] *Les Manuscrits de Léonard de Vinci*, loc. cit. — Cf. *Del Moto e misura dell'acqua*, libro I, capitolo VI, VII et VIII.
[157] *Les Manuscrits de Léonard de Vinci*, Ms. F, folio 27, recto, and folio 26, verso. — Cf. *Del Moto e misura dell'acqua*, libro I, capitolo IV.
[158] *Les Manuscrits de Léonard de Vinci*, Ms. F, folio 52, verso.
[159] Ibid.
[160] *Les Manuscrits de Léonard de Vinci*, Ms. F, folio 83, recto.

[161] This is an inadvertent error. It ought to read: . . . first backwards, then just as much forward . . . .
[162] T. N.: In modern terms, each tower will be subjected to a moment about its base because the center of gravity of the tower extends out over the edge of the foundation. If the foundation is not able to react a tensile force the building will topple over.
[163] Compare, for instance, Ms. F, folio 1, verso and Chapter XXIV of the *Traité de la Peinture* (1651 French Edition).
[164] Cf. P. Duhem, "Thémon, le fils du Juif et Léonard de Vinci" (This article will soon be published in the *Bulletin Italien*).
[165] *Les Manuscrits de Léonard de Vinci*, Ms. A of the Bibliothèque de l'Institut, folio 20, verso.
[166] This proposition appears to contradict the one which Leonardo previously formulated (Ms. F, folio 82, verso). Here, Leonardo neglects the convergence of the verticals, which he had taken into account previously.
[167] *Les Manuscrits de Léonard de Vinci*, Ms. A, folio 22, recto.
[168] Ibid., Ms. A, folio 21, verso.
[169] *Les Manuscrits de Léonard de Vinci*, Ms. E of the Bibliothèque de l'Institut, folio 57, recto.
[170] Ibid., Ms. H, folio 115 [28] recto.
[171] Ibid., Ms. A, folio 28, verso.
[172] *Les Manuscrits de Léonard de Vinci*, Ms. A, folio 28, verso, and folio 29, recto.
[173] *Traité de la Peinture*, by Leonardo da Vinci, published and translated from Italian into French by R. F. S. D. C. (Roland Fréart, Lord of Chambray); in Paris, at the printing shop of Jacques Langlois, MDCLI; Ch. CCII, p. 66.
[174] Id., ibid., ch. CCI, p. 66.
[175] Id., ibid., ch. CCVI, p. 68.
[176] Central line = the line which goes to the center of the earth; the vertical.
[177] This sentence of Leonardo contains an obvious mistake; we corrected it in parentheses.
[178] *Traité de la Peinture*, Léonard de Vinci, Ch. CCVII, p. 68.
[179] Id., ibid.
[180] *Le Traité de la Peinture*, Léonard de Vinci, Ch. CCVIII, p. 69.
[181] Id., ibid., Ch. CCXCIX, p. 99.
[182] *I Manoscritti di Leonardo da Vinci, Codice sul volo degli uccelli*, Paris, 1893; fol. 16 [15], verso: Cf. fol. 4, verso.
[183] *Le Traité de la Peinture*, Léonard de Vinci, Ch. CCXCIX, p. 99.
[184] *Les Manuscrits de Léonard de Vinci*, Ms. A, folio 20, veso.
[185] Ibid., Ms. A, fol. 21, verso.
[186] *Alberti de Saxonia Quaestiones in octo libros Physicorum*: in librum IV quaestio XII. (T. N.: The Latin title reads, *Questions on the Eight Books of the Physics by Albert of Saxony*: Question XII on Book IV.) It does not appear that this work was printed before 1516, at which time it was printed simultaneously in Venice and in Paris.
[187] Cf. P. Duhem, "Albert de Saxe et Léonard de Vinci" (*Bulletin Italien*, t. V, p. 1; 1905).
[188] *Les Manuscrits de Léonard de Vinci*, Ms. F, folio 41, verso.
[189] Nicolai Copernici *De revolutionibus orbium coelestium libri sex*; lib. I, cap. II.

(T. N.: The Latin title reads, *Six Books on the Revolutions of the Celestial Orbs*; Book I, Chapter II.)
[190] Id., ibid., lib. I, cap. VII.
[191] Id., ibid., lib. I, cap. IX.
[192] Nicolai Copernici *De revolutionibus orbium coelestium libri sex*; lib. I, cap. III.
[193] Aristotle, *On Meteorology*, I, III. As a matter of fact, Aristotle only indicated clearly this proportionality for the volumes of air and water: "The same ratio of volume must exist between the totality of water and the totality of air, as that between a small quantity of water and the air which this water can generate. We must agree with Gaëtan of Tiène that the meaning requires a rearrangement of the words of Aristotle. (T. N.: The Greek reads, It is necessary for the same ratio which exists between a given small amount of water and the air which can be produced from it to also exist between the totality of air and the totality of water.)
[194] Cf. First Period, Section 5; p. 292.
[195] Thimonis *Quaestiones in libros Meteorum*; in librum primum quaestio VI. (T. N.: The Latin reads, Themon, *Questions on Books of On Meteorology*; Question VI on Book I.)
[196] *Libri Metheorum* Aristotelis Stagiritae cum commentariis Gaietani de Thienis; lib. I, cap. III. The first edition, of many, of this work came out of Padua in 1476 with Peter Mauser. (T. N.: The Latin title reads, *Books from On Meteorology* by Aristotle of Stagira with Commentaries by Gaëtan of Tiène: Book I, Chapter III.)
[197] Cf. First Period, Section 5, p. 299.
[198] *Sphera volgare novamente tradotta con molte notande additioni di geometria, cosmographia, arte navigatoria, et stereometria, proportioni et quantita delli elementi, distanze, grandezze et movimenti di tutti li corpi celesti, cose certamente rade et maravigliose*, autore M. Mauro Fiorentino, Phonasco et Philopanareto . . . (In fine) Anno salutis nostrae MDXXXVII, mense Ottobri, impresso in Venetia, per Bartholomeo Zanetti. Même Ouvrage: in Venetia, per Stefano di Sabio, 1537. (T. N.: The Italian reads, *Our Common World recently translated with many additional notes on geometry, cosmography, the art of navigation, and stereometry, the proportion and quantity of the elements, distance, size and motion of all the heavenly bodies, rare and marvelous matters*, authored by M. Mauro of Florence.)
[199] Nicolai Copernici *De revolutionibus orbium coelestium libri sex*, lib. I, cap III.
[200] *The Books* by Jerome Cardan, Milanese physician, *entitled On Subtlety, and Subtle inventions, collection of the occult causes and the reasons thereof*, translated from Latin into French by Richard Le Blanc, in Paris, by Charles l'Angelier, keeping shop at the First Pillar of the great Hall of the Palais, 1556, Book XVII, folio 323, verso — Copernicus' name is not mentioned in the first edition of the *De Subtilitate*, published in 1551; it was introduced by Cardan in the second edition on which is based the French translation of Richard Le Blanc.
[201] Hieronymi Cardani, medici Mediolanensis, *De subtilitate* libri XXI; Lugduni, apud Guglielmum Rouillium, sub scuto Veneto, 1551; lib. II, p. 124. In the French translation by Richard Le Blanc, fol. 63, recto. (T. N.: The Latin title reads, *21 Books on Subtlety* by the Milanese physician Jerome Cardan; Lyon, Guillaume Rouille, Under the Seal of Venice, 1551, Book II, p. 124.)
[202] *La seconda parte della filosofia naturale* di M. Alessandro Piccolomini, in Vinegia, appresso Vincenzo Valgrisio, alla Bottega d'Erasmo. MDLIIII. — *La prima parte della*

*filosofia naturale* avait paru en 1551; les deux parties ont eu, ultérieurement, plusieurs éditions. (T. N.: The Italian and French read, *The Second Part of the Natural Philosophy* of Alessandro Piccolomini; Vinegia, printed by Vincenzo Valgrisio, at the Shop of Erasmus, 1553. *The First Part of the Natural Philosophy* had appeared in 1551; Both parts appeared in general later editions.)

203 A. Piccolomini, op. cit., lib. III, cap. III, p. 279.

204 Id., ibid., lib. III, cap. IX, p. 335.

205 *Della grandezza della terra et dell' acqua*, trattato di M. Alessandro Piccolomini, nuovamente mandato in luce, all' illustr. et rever[mo] S[re] Monsig. M. Iacomo Cocco, arcivescovo di Corfù. In Venetia. MDLVIII, appresso Giordano Ziletti, all'insegna della Stella. — Le même ouvrage, sous le même titre, et par les soins du même imprimeur, fut donné de nouveau en 1561. (T. N.: The Italian and French read, *On the size of land and water*, written by Alessandro Piccolomini, recently published by the most illustrious and reverend Monsignor M. Iacomo Cocco, archbishop of Corfù. In Venice. MDLVIII, at Giordano Ziletti, at the sign of the Star. — The same work, under the same title, and under the direction of the same printer, was published again in 1561.)

206 A. Piccolomini, op. cit., cap XIV.

207 Id., ibid., p. 41.

208 Fr. Junctini Florentini, sacrae theologiae doctoris, *Commentaria in sphaeram Joannis de Sacro Bosco accuratissima*. Lugduni, apud Philippum Tinghium, MDLXXVIII.

209 Junctinus, op. cit., p. 198.

210 Id., ibid., p. 179.

211 Antonio Berga, *Discorso . . . della grandezza dell' acqua et della terra, contra l'opinione dil* (sic) *S. Alessandro Piccolomini.* In Torino, appresso gli her. del Bevilacqua, MDLXXIX. (T. N.: The Italian reads, *Discourse . . . on the size of water and land, against the opinion of Alessandro Piccolomini.* In Turin, at the Heirs of Bevilacqua, 1579.)

212 *Consideratione* di Gio. Battista Benedetti, filosofo del Sereniss. S. Duca de Savoia, *d'intorno al discorso della grandezza della terra, et dell' acqua, del Eccellent. Sig. Antonio Berga filosofo nella Universita di Torino.* In Torino, presso gli heredi del Bevilacqua, 1579. (T. N.: The Italian reads, *Considerations* of Giovanni Battista Benedetti, philosopher to the Most Serene Duke of Savoy, *about the discourse on the size of water and land*, of his excellency Antonio Berga, philosopher of the University of Turin. In Turin, at the Heirs of Bevilacqua, 1579.)

213 Jo. Baptistae Benedicti, patritii Veneti, philosophi, *Diversarum speculationum mathematicarum et physicarum liber*; Taurini, apud haeredem Nicolai Bevilaquae, MDLXXXV; pp. 215, 216, 235, 241, 242, 243, 255, 260, 261, 315. (T. N.: Giovanbattista Benedetti, *A Book of Diverse Speculations on Mathematics and Physics*, Turin, the Heir to Nicolas Bevilaqui, 1585, pp. 215, 216, 235, 241, 242, 243, 255, 260, 261, 315.)

214 Id., ibid., p. 235.

215 Id., ibid., p. 255.

216 G. B. Benedetti, *Consideratione . . .*, p. 17.

217 T. N.: The Latin reads, The center of gravity of every body is a certain point located within the body. If one imagines the body suspended from that point, while suspended it will remain immobile and retain the initial orientation and will not rotate.

218 T. N.: The Latin reads, The center of gravity of every solid figure is that point

located within, around which the parts have moment equilibrium; if indeed a plane is drawn through such a center, no matter how it cuts the figure, it will always divide it into parts of equal wight.

[219] G. B. Benedetti, *Consideratione . . .*, p. 14.

[220] *Disputatio de magnitudine terrae et aquae* . . . a Franc. Maria Vialardo *ab italico in latinum sermonem conversa*; Taurini, apud Jo. Bapt. Raterium, 1580. (T. N.: The Latin title reads, *Disputation on the Magnitude of the Earth and Water*, translated from Italian into Latin by Francesco Maria Vialardi; Turin, Giovanbattista Raterius, 1580.)

[221] *Trattato della grandezza dell' acqua e della terra* di Agostino Michele, nel quale contra l'opinione di molti filosofi, et di molti matematici illustri, dimostrasi l'acqua esser di maggior quantità della terra: (In fine) In Venetia, appresso Nicolò Moretti; MDLXXXIII. (T. N.: The Italian reads, *Treatise on the size of water and land* of Agostino Michele, in which, against the opinion of many philsophers and illustrious mathematicians, it is demonstrated that water is of a larger magnitude than land: (In fine), Venice, at Nicolo Moretti; 1583.)

[222] J. B. Benedicti *Diversarum speculationum liber*, p. 397.

[223] *Tractatus in quo adversus antiquorum, et praecipue peripateticorum opinionem terram esse aqua majorem multis efficacissimis rationibus et experientiis demonstratur*, auctore Nonio Marcello Saia a Rocca Gloriosa in Lucana . . . *Addita est etiam quatuor elementorum expositio*; Parisiis, apud Thomam Perier, vià Jacobaea, sub insigne Bellerophonte, MDLXXXV. (T. N.: The Latin title reads, *A Treatise in which, contrary to the view of the Ancients and especially of the Peripatetics it is demonstrated by many most effective reasons and experiments that there is more earth than water*, written by Nonio Marcello Saia from Lucca . . . Appended is an exposition on the four Elements; Paris, Thomas Perier, Rue St. Jacques, At the Sign of Bellerophon, 1585.)

[224] *Commentarii Collegii Conimbricensis, Societatis Jesu, in quatuor libros de Coelo Aristotelis Stagiritae*; Lugduni, ex officina Juntarum, MDXCIII. — In librum II de Caelo quaestio III: Num terra in medio mundi constituta sit, habeatque idem centrum gravitatis et magnitudinis, Arts. 1 et 2. (T. N.: The Latin title reads, *Commentaries on Four Books of Aristotle's On the Heavens*, the University of Coimbre, Society of Jesus; Lyon, 1593. — On Book II of On the Heavens, Question III: whether the earth is located at the center of the Universe and whether it has the same center of gravity and geometric center, Articles 1 and 2.)

[225] Hieronymi Cardani Mediolanensis, civisque Bononiensis, philosophi, medici et mathematici clarissimi, *Opus novum de proportionibus* . . . , Basileae, MDLXX, Prop. LX, p. 51.

[226] Ibid.

[227] *Les livres* de Hierome Cardanus, medecin Milannois, *intitulés de la Subtilité et subtiles inventions, ensemble les causes occultes, et raisons d'icelles*, traduis de latin en françois par Richard Le Blanc; à Paris, par Charles l'Angelier, tenant sa boutique au premier pillier de la grand' salle du Palais. Livre XVII, fol 343, verso. (T. N.: The French title reads, Jerome Cardan Milanese physician, *The Books entitled On Subtlety and Subtle Inventions*, including their occult causes and reasons, translated from Latin into French by Richard Le Blanc, Paris, Charles l'Angelier, keeping his shop at the first pillar of the Main Hall of the Palais. Book XVII, fol. 343, verso.)

[228] T. N.: The Latin reads, Thus, since the center of gravity is removed spontaneously

from the center of the earth, so the heavy body ascends by natural motion which is not possible. Thus the pail does not descend.

229 Translation of Richard Le Blanc, folio 351.

230 *Les Manuscrits de Léonard de Vinci*, Ms, A de la Bibliothèque de l'Institut, folio 28, verso.

231 Id., ibid., folio 33, verso.

232 *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien; Ms, A de la Bibliothèque de l'Institut, Paris, 1881. Préface, p. 2.

233 Hieronymi Cardani, Medici Mediolanensis, *De Subtilitate libri XXI*; Lugduni, 1551; p. 529. Translated into French by Richard Le Blanc, cf. fol. 318, verso.

234 Cardan, *De Subtilitate*, Edition of 1551; p. 532. Translated into French by Richard Le Blanc, fol. 322, recto.

235 Cf. P. Duhem, "Léonard de Vinci et Jérome Cardan." This article will appear soon in the *Bulletin Italien.*

236 Guidi Ubaldo e Marchionibus Montis *in duos Archimedes aequiponderantium libros paraphrasis Scholiis illustrata*, Pisauri apud Hieronymum Concordiam, MDLXXXVIII; p. 9. (T. N.: The Latin title reads, Guido Ubaldo del Monte, *A Paraphrase on Two Books of Archimedes on Equilibrium*, illustrated with scholia, Pesaro, Jerome Concordia, 1588, p. 9.)

237 Guidi Ubaldi e Marchionibus Montis *Mecanicorum liber*. Venetiis, MDCXV, p. 15. (T. N.: The Latin title reads, Guido Ubaldo del Monte, *A Book on Mechanics*, Venice, 1615, p. 15.)

238 Montucla, *Histoire des Mathématiques*, Paris, Part III, Book V, Vol. I, p. 91.

239 T. N.: The name used by English speaking peoples is Villalpand.

240 Hieronymi Pradi et Joannis-Baptistae Villalpandi e Societate Jesu *in Ezechielem explanationes et apparatus Urbis et Templi Hierosolymitani commentariis et imaginibus illustratus.* Opus tribus tomis distinctum. Romae, MDXCVI—MDCIIII. (T. N.: The Latin title reads, *Exegeses on Ezechiel with commentaries and sketches of the City and Temple of Jerusalem.* By Jeronimo Prado and J.-B. Villalpand of the Society of Jesus, a work divided into 3 volumes. Rome 1596—1604.)

241 *Tomi III, apparatus Urbis ac Templi Hierosolymitani, Partes I et II*, Joannis-Baptistae Villalpandi Cordubensis e Societate Jesu, collato studio cum H. Prado ex eadem Societate. Romae MDCIIII. (T. N.: The Latin title reads, *Volume III, the Apparatus on the City and Temple of Jerusalem, Parts I and II*, by J.-B. Villalpand of Cordova and of the Society of Jesus in collaboration with Jeronimo Prado of the same society. Rome, 1604.)

242 J. B. Villalpand, loc. cit., Prop IV, p. 321.

243 *Les Manuscrits de Léonard de Vinci*, Ms. G, fol. 75, recto.

244 *Le Traité de la Peinture de Léonard de Vinci*, Ch. CXCVII, p. 64.

245 Villalpand, loc. cit., prop. V, p. 321.

246 Id., ibid., prop. VI, p. 322.

247 *Les Manuscrits de Léonard de Vinci*, Ms. F, Fol. 82, verso.

248 Villalpand, loc. cit., prop. VII, p. 322.

249 Villalpand, loc. cit., prop. VIII, IX and X; pp. 108 and 109.

250 Id., ibid., prop. XIII, p. 324.

251 *Traité de la Peinture* de Léonard de Vinci, Ch. CXCVI, p. 64.

[252] Villalpand, loc. cit., prop. XII, p. 324.
[253] In Chapter XIII, Section 1, we already discussed the *Mathematical Synopsis* of Mersenne and the *Mechanicorum libri* which it contains. The first of these books is entitled *De gravitatis et Universi centro* (a). The first part is divided into four parts. It is defined in the following way: *Continens definitiones et ea quae spectant ad centrum Universi* (b). The second part restates the propositions from the treatise of Commandino. The third part restates the propositions from the treatise of Luca Valerio. The fourth part is entitled: *De linea directionis et reliquis ad centrum gravitatis pertinentibus* (c). It first restates the theorems of Villalpand (prop. I to prop. XIV), then six more propositions given without the name of their author. (a) T. N.: The Latin title reads, *On the center of gravity and of the Universe.* (b) T. N.: The Latin title reads, *Containing definitions and matters relating to the center of the Universe.* (c) T. N.: The Latin title reads, *On the line of direction and other matters pertaining to the center of gravity.*
[254] Mersenne, *Mechanicorum libri*, p. 4.
[255] Id., ibid., p. 7.
[256] *Les questions théologiques, physiques, morales et mathématiques*, où chacun trouvera du contentement et de l'exercice, composées par L. P. M.; à Paris, chez Henry Guenon, rue Saint-Jacques, près les Jacobins, à l'image Saint-Bernard. MDCXXXIV. Question VIII, p. 32. (T. N.: The French reads, *Theological, Physical, Moral, and Mathematical Questions*, where every reader will find pleasure and practice, a work written by Father Mersenne, Paris, Henry Guenon, Rue St. Jacques, near the Jacobins at the sign of St. Bernard, 1634, Question VIII, p. 32.)
[257] *La Vérité des Sciences contre les Sceptiques ou Pyrrhoniens*, dédié à Monsieur, frère du Roy, par F. Marin Mersenne, de l'ordre des Minimes; à Paris, chez Toussainct du Bray, rue Saint-Jacques, aux Epiesmeurs, MDCXXV; p. 871. (T. N.: The French reads, *The Truth of the Sciences against the Sceptics or Pyrrhoniens*, dedicated to the Brother of the King, by Father Marin Mersenne of the Order of Minims; in Paris; Toussainct du Bray, Rue St. Jacques, at the sign of the Spice Blenders, 1625; p. 871.)
[258] Mersenne, *Mechanicorum libri*, p. 112.
[259] id., ibid., p. 111.
[260] Cf. Chapter XVII; Section 4. *The Great Treatises on Statics from the Jesuit School.* Father Dechales (1621—1678). Father Paolo Casati (1617—1707).
[261] R. P. Claudii Milliet Dechales, Camberiensis, e Societate Jesu, *Cursus seu Mundus mathematicus.* Editio altera; Lugduni, apud Anissonios, Joan. Posuel et Claud. Rigaud. MDCXC. — tomus II, Staticae lib. VIII, prop. IV, p. 364.
[262] Mersenne, *Mechanicorum libri*, p. 114.
[263] T. N.: Cf. Chapter XV, Section 1.
[264] Bernardini Baldi Urbinatis, Guastalla Abbatis, *In mechanica Aristotelis problemata exercitationes, adjecta succincta narratione, de Autoris vita et scriptis.* Moguntiae, typis et sumptibus Viduae Joannis Albini, MDCXXI. (T. N.: The Latin title reads, Bernardino Baldi of Urbino, Abbot of Guastalla, *Exercises on the Mechanical Problems of Aristotle*, with a brief narration of the life and works of the author. Mainz, printed by the Widow of Johann Albinus, 1621.)
[265] T. N.: Passages in Baldi's preface and in the text of the book imply that the year of composition is 1589.
[266] Nicolai Leonici (*sic*) Thomaei *Opuscula nuper in lucem edita quorum nomina proxima habentur pagella . . . Conversio mechanicorum quaestionum Aristotelis, cum*

*figuris et annotationibus quibusdam.* In fine: Opusculum hoc ex impressione repraesentavit Bernardinus Vitalis Venetus, Anno Domini MDXXV Die XXIII Februarii, ex Venetiis. (T. N.: The Latin title reads, Nicolo Leonico (sic) Tomeo *Treatises recently published with titles on the next pages . . . Translation of Aristotle's Mechanical Problems*, with figures and notes. At the end: This treatise was published by the Venetian Bernardino Vitali, Feb. 23, 1525, Venice. The title has apparently confused Nicolo Leoniceno with his contemporary, Nicolo Leonico Tomeo.)

267 Cf. P. Duhem, *Léonard de Vinci et Bernardino Baldi* (*Bulletin Italien*, t. V, p. 309, Octobre 1905).

268 Bernardini Baldi *In mechanica Aristotelis problemata exercitationes*, p. 1.

269 Bernardino Baldi, loc. cit., Quaest. XXIX, p. 162.

270 Bernardino Baldi, loc. cit., Questio XXX, p. 166.

271 *Les Manuscrits de Léonard de Vinci*; Ms. A de la Bibliothèque de l'Institut, folio 28, verso.

272 Bernardini Baldi, *In mechanica Aristotelis problemata exercitationes*, p. 172.

273 Bernardino Baldi, loc. cit., p. 176.

274 Id., ibid., p. 84.

275 Id., ibid., p. 60.

276 Bernardino Baldi, loc. cit., p. 20 and 31.

277 Id., ibid., Quaestio II, pp. 18—34.

278 Bernardino Baldi, loc. cit., p. 14.

279 T. N.: The further the center of gravity from the point of support the greater the increment in elevation from the equilibrium position for small displacements of the balance.

280 Bernardino Baldi, loc. cit., p. 33.

281 *Les Manuscrits de Léonard de Vinci*: Ms. A de la Bibliothèque de l'Institut, fol. 50, verso.

282 Léonard de Vinci, loc. cit., fol. 52, recto.

283 Léonard de Vinci, loc. cit., fol. 21, verso and fol. 52, recto.

284 Cf. Chapter II, figure 8; Chapter VIII, Section 3, figure 56; Chapter XV, end of Section 6.

285 Bernardini Baldi *In mechanica Aristotelis problemata exercitationes*, pp. 62—64.

286 *Les Questions théologiques, physiques, morales et mathématiques, où chacun trouvera du contentement ou de l'exercice*, par L. P. M. (le P. Mersenne); à Paris, MDCXXXIV, chez Henri Guenon, Ruë St. Jacques, près les Jacobins, à l'image St. Bernard: p. 38. (T. N.: The French reads, *The Theological Physical, Moral and Mathematical Questions in which everyone will find satisfaction or practise*, written by L. P. M. (Father Mersenne); in Paris 1634, at Henry Guenon, Rue St. Jacques, next to the Jacobins, at the sign of St. Bernard, p. 38.)

287 Mersenne, loc. cit. Question VIII: What is the line of direction which is helpful to mechanics?

288 Bibliothèque Nationale, fonds Latin, Ms. No. 7226, fol. 85, recto fol. 207, recto.

289 T. N.: The French title reads, *Treatise on mechanics and in particular on the piping and pumping of water.*

290 Cf. P. Duhem, *Bernardino Baldi, Roberval et Descartes* (*Bulletin Italien*, vol. VI, January 1906).

291 Furthermore the influence of Guido Ubaldo, combined with that of Bernardino

Baldi, was very powerful during the time of Galileo. The works of Monantholius and of Father Mersenne are proof of this. Further evidence of this can be found in the commentaries on the *Mechanical Problems* of Aristotle by John of Guevara (see a, below). When the latter teaches (see b, below) "that the entire gravity of a heavy body is united at its center of gravity, that it concentrates there in such a way that there seems to be no gravity in the rest of the body," Guevara is borrowing most of the supporting commentary for this idea from Guido Ubaldo del Monte and from Baldi. Furthermore, he is continually quoting these two authors.

a. Joannis de Guevara, cher. reg. min., in *Aristotelis mechanicas commentarii*, una cum additionibus quibusdam ad eandem materiam pertinentibus; Romae, apud Jacobum Mascardum, MDCXXVII. (T. N.: The Latin reads, Juan de Guevara, clergyman of the minor orders: *Commentaries on the Mechanical Problems of Aristotle* together with certain additions pertaining to the same matter; Rome, published by Jacob Mascardus, 1627.)

b. Guevara, loc. cit., Additio secunda: *de centro gravitatis naturaliq.* mobilitate gravium et levium: p. 67. (T. N.: The Latin reads, Guevara, loc. cit., Second Appendix: *On the natural center of gravity* and the mobility of heavy and light bodies; p. 67.)

[292] T. N.: Galileo used the term *momento* in three ways: first, to mean statical moment, i.e., the product of weight and perpendicular distance to an axis, secondly, as the product of weight and velocity in the sense expressed in the *Mechanical Problems* and thirdly, as a measure of the positional gravity of a body on an inclined plane.

[293] *Les Méchaniques* de Galilée Mathématicien et Ingénieur du Duc de Florence, avec plusieurs additions rares et nouvelles, utiles aux Architectes, Ingénieurs, Fonteniers, Philosophes et Artisans: à Paris, chez Henry Guenon, MDCXXXIV. (T. N.: The French reads, *The Mechanics* of Galileo, Mathematician and Engineer to the Duke of Florence, with several rare and new additions, useful to Architects, Engineers, Fountainmakers, Philosophers and Craftsmen: Paris, Henry Guenon, 1634.)

[294] Vincenzio Viviani, *Vita di Galileo Galilei*, cavata da'Fasti Consolari dell'Accademia Fiorentina. (This life of Galileo, reproduced in all the editions of his works, was initially a letter by Viviani to Prince Cardinal Leopold of Tuscany. It was included by the Abbot Salvino Salvi in the *Fastes Consulaires* of the Academy of Florence).

[295] Viviani was not the only one to have noticed this lacuna in the deduction of Galileo. On October 11, 1638 Descartes wrote to Mersenne:[a] "Reverend Father, I shall begin this letter with my observations on the book of Galileo. In general I find him to philosophize much better than the common man in as far as he avoids as much as possible the errors of the School and tries to examine physical matters by means of mathematical reasoning. In this, I am in complete agreement with him and I maintian that there is no other method for finding the truth . . . . He also assumes that the velocity of the same body on different planes is equal when the elevations of these planes are equal. But he does not prove this and it is not absolutely true. And as far as everything else which follows depends on these two suppositions, one can say that he built entirely on a foundation of sand . . . ."

a. *Oeuvres de Descartes*, édition Ch. Adam and Paul Tannery, *Correspondance*, vol. II (March 1638 to December 1639), p. 379ff.

[296] Vincenzio Viviani, *Vita di Galileo Galilei.*

[297] *Lettera di Galileo Galilei al P. Ab. D. Benedetto Castelli, contenente una dimostra-*

*zione d'un principio gia supposto dall'Autore nel suo Trattato del Moto accelerato ne'Dialoghi de' movimenti locali.* (The letter is reproduced in the various editions of the Works of Galileo.)

[298] Vincenzio Viviani, *Vita di Galileo Galilei*: Cf. also *Opere di Galileo Galilei*, divise in quattro tomi, in questa nova edizione accresciute di molte cose inedite: tomo primo. In Padova MDCCXLIV, nella stamparia del Seminario, appreso Gio. Manfrè. *Prefazione universale*, p. XXX. (T. N.: The Italian title reads, Vincenzio Viviani, *The Life of Galileo Galilei*: Cf. also: *The Works of Galileo Galilei*, divided into four volumes, a new edition augmented by many new unpublished items; 1st Vol. Padua, 1744.)

[299] Montucla, *Histoire des Mathématiques,* nouvelle édition. Paris, An VII, tome II, p. 201.

[300] Cf. Gassendi, *Opera*, vol. VI, pp. 53 and 54.

[301] Petri Gassendi *Epistolae tres de proportione qua gravia descendentia accelerantur, quibus ad totidem epistolas R. P. Petri Cazraei, Societatis Jesu, respondetur*: Epistola prima, Art. XIV; Parisiis, eid. martis MDCLV (Petri Gassendi *Opera*, vol. III, p. 570; Lugduni, 1638.) (T. N.: The Latin reads, Pierre Gassendi; *Three letters on the ratio by which falling bodies accelerate, in response to three letters by Father Peter Cazrée of the Society of Jesus*; First Letter, article 14, Paris, March 1655. (Pierre Gassendi, *Opera*, Vol. III, p. 570, Lyon, 1658.)

[302] The date of this treatise of Pascal is unknown. It was published by Etienne Périer in 1663, one year after the death of his brother-in-law.

[303] Blaise Pascal, *Oeuvres Complètes*, vol. III, pp. 86 and 87; Paris, Hachette and Co., 1880.

[304] *Letter of Pascal to M. de Ribeyre*, first president of the Cour des Aides at Clermont-Ferrand, concerning what was stated in the prologue to the philosophical theses defended in his presence in the Jesuit Collège de Montferrand, on June 25, 1651. (Blaise Pascal, *Oeuvres Complètes*, vol. III, pp. 76 and 77; Paris, Hachette, 1880.)

[305] F. Marini Mersenni Minimi, *Cogitata physico-mathematica in quibus tam naturae quam artis effectus admirandi certissimis demonstrationibus explicantur*; Parisiis, sumptibus Antonii Bertier, via Jacobea, MDCXLIV. *Ars navigandi.* Hydrostaticae liber primus, p. 239. (T. N.: The Latin reads, F. Marin Mersenne of the Minims, *Physical and Mathematical Reflections in which the marvelous effects of both nature and art are explained by clear demonstrations*; Paris, at the expense of Antoine Bertier, Rue de Jacobins, 1694. The Art of Navigation, Book I on Hydrostatics, p. 239.)

[306] Cf. on this subject: P. Duhem, "Le Principe de Pascal, Essai historique" (*Revue Générale des Sciences*), 16th Year, p. 599, 15 July 1905.

## FOOTNOTES TO CHAPTER XVI

[1] T.N.: A finite change in the system's configuration will raise the center of gravity but for an infinitesimal change the center of gravity remains stationary.

[2] Evangelistae Torricellii *de dimensione parabolae solidique hyperbolici problemata duo* ad lectorem prooemium, p. 9. (T.N.: The Latin reads, *Two Problems on the Measurement of the Hyperbolic and Solid Parabola*: Preface to the reader.)

[3] Evangelista Torricelli, loc. cit., p. 11.
[4] Jo. Kepleri *littera ad Herwartum*, 28 mars 1605 (*Joannis Kepleri astronomi Opera omni* edidit Ch. Frisch; vol. II, p. 87).
[5] Joannis Kepleri *De motibus stellae Martis commentarii*, Pragae, 1609 (*Kepleri Opera omnia*, vol. III, p. 151).
[6] Cf. P. Duhem, *La théorie physique, son objet et sa structure*; 2^e^ partie, ch. VII, paragraph 2, p. 364, paris, 1905.
[7] *Harmonie universelle*, par F. Marin Mersenne. *Seconde partie de l'Harmonie universelle*. Livre VIII, De l'utilité de l'harmonie et des autres parties des mathématiques. Proposition XVIII, p. 61. Paris, MDCXXXVII. (T.N.: The French reads, On the utility of harmony and the other fields of mathematics.)
[8] T.N.: Geostatics is that part of mechanics which deals with balanced forces in rigid bodies.
[9] Mersenne, loc. cit., p. 63.
[10] Fermat, *Oeuvres*, published through the efforts of Paul Tannery and Ch. Henry, vol. II, Correspondance, p. 4.
[11] Joannis de Beaugrand, Regis Francae domui regnoque ae aerario sanctiori a consiliis secretisque. *Geostatice, seu de vario pondere gravium secundum varia a Terrae centro intervalla dissertatio mathematica*; Parisiis, apud Tussanum Du Bray. MDCXXXVI. (T.N.: The Latin title reads, *Geostatics or a mathematical dissertion on the ratio of the weight of heavy bodies to their distance from the center of the earth*; Paris, with Tussamus du Bray, 1936.)
[12] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery, vol. II, *Correspondance*, p. 174: Lettre de Descartes à Mersenne du 29 juin 1638.
[13] The anonymous pamphlets which Beaugrand wrote against Descartes were discovered by Paul Tannery (Paul Tannery, *La Correspondence de Descartes dan les inédits du fonds Libri*; Paris, 1896).
[14] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery, vol. II, *Correspondance*, p. 253.
[15] Fermat, *Oeuvres*, publiées par les soins de Paul Tannery et Ch. Henry. Tome II, *Correspondance*, p. 14.
[16] Id., ibid., p. 6: Propositio geostatica Domini de Fermat. (T.N.: The Latin reads, The proposition on geostatics by M. de Fermat.)
[17] T.N.: The "disturbance" must be infinitesimally small.
[18] Alberti de Saxonia *Quaestiones in octo libros Physicorum*; in librum IV quaestio V.
[19] Alberti de Saxonia *Quaestiones in libros de Caelo et Mundo*; in librum II quaestio XXIII.
[20] Johannis Marcillii Inguen *Quaestiones super octo libros Physicorum*; circa librum IV quaestio V. (T.N.: The Latin title reads, Marsilius of Inghen, *Questions on the eight books of the Physics*, concerning Book IV, question V.)
[21] *Harmonie universelle*, par F. Marin Mersenne. *Seconde Partie de l'Harmonie universelle*. Livre VIII, De l'utilité de l'harmonie et des autres parties des mathématiques. Proposition XVIII, p. 63. Paris, MDCXXXVII.
[22] Descartes, *Oeuvres*, publiées par Ch Adam et Paul Tannery. Tome II, *Correspondance*, p. 190; Lettres de Descartes à Mersenne du 20 juin 1638.
[23] Fermat, *Oeuvres*, publiées par les soins de MM. Paul Tannery et Ch. Henry; Tome

II, *Correspondance*, p. 25; Nova in mechanicis theoremata Domini de Fermat. (T.N.: The Latin reads, New Theorems on Mechanics by M. Fermat.)
[24] Fermat, loc. cit., p. 17.
[25] Fermat, loc. cit., p. 23.
[26] Ibid.
[27] T.N.: "indifferent equilibrium" would be more accurate.
[28] Fermat, loc. cit., p. 18.
[29] Ibid., p. 26.
[30] Fermat, loc. cit., p. 25.
[31] Ibid., p. 25.
[32] Fermat, *Oeuvres*, published through the care of Messrs. Paul Tannery and Ch. Henry, vol. II, Correspondance, p. 28.
[33] Fermat, *Oeuvres*, published through the care of Messrs. Paul Tannery and Ch. Henry, Vol. II, Correspondance, p. 28.
[34] Id., ibid., p. 31.
[35] Fermat, *Oeuvres*, published through the care of Messrs. Paul Tannery and Ch. Henry, vol. II, Correspondance, p. 35.
[36] T.N.: The Latin title reads, *On the System of the World* by Aristarchus of Samos.
[37] Etienne Pascal and Roberval, loc. cit., p. 43.
[38] *Harmonie universelle*, par F. Marin Mersenne. *Seconde Partie de l'Harmonie universelle.* Livre VIII, De l'utilité de l'harmonie et des parties des mathématiques. Proposition XVIII, p. 63. Paris, MDCXXXVII.
[39] The one asserted by Beaugrand.
[40] Cf. Chapter VII, Section 4.
[41] Cf. Chapter XV, Section 5.
[42] Alberti de Saxonia, *Quaestiones in libros de Caelo et Mundo*; in librum I quaestio X. (T.N.: The Latin title reads, *Questions on Book of On the Heavens*, Question X on Book I.)
[43] Fermat, *Oeuvres*, published by the care of Messrs. Paul Tannery and Ch. Henry; vol. II, Correspondance, p. 58.
[44] Fermat, op. cit., p. 59. *Letter of Fermat to Roberval*, Sept. 16, 1636.
[45] Fermat, op. cit., p. 75.
[46] *Harmonie universelle*, par F. Marin Mersenne; Seconde partie de l'Harmonie universelle; Nouvelles observationes physiques et mathématiques. V^e observation, p. 17. Paris, MDCXXXVII.
[47] This rule was proposed by Fermat.
[48] Fermat, *Oeuvres*, published through the care of Messrs. Paul Tannery and Ch. Henry; Vol. II, Correspondance, p.87.
[49] Fermat, *Oeuvres*, published through the care of Messrs. Paul Tannery and Ch. Henry, vol. II, Correspondance: Letters by Fermat to Roberval on December 6, 1636 (p. 89) and on December 16, 1636 (p. 92).
[50] Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; vol. II, Correspondance (March 1638—December 1639), p. 222. (T.N.: The French reads, *Examination of the question whether a body weighs more or less, in proportion to its proximity to the center of the earth.*)
[51] Descartes, loc. cit., p. 238.

[52] Id., ibid., p. 242.
[53] In a letter probably addressed to Boswell and perhaps written in 1646, Descartes declares that he "shares the opinion of those who say that two weights are in equilibrium when they are in inverse ratio to the perpendiculars drawn from the center of the balance to the lines which join the extremities of the arms and the center of the earth." He adds that "not only is the reason for this obvious, but it can also be proven." We have to admit that we have been unable to find any trace of a conclusive reasoning in the considerations of Descartes.

a. Descartes, *Oeuvres*, published by Ch. Adam and Paul Tannery; vol. IV, Correspondance, Additions, p. 696.

[54] Descartes, loc. cit., p. 244.
[55] Descartes, loc. cit., p. 245.
[56] Fermat, *Oeuvres*, published through the care of Messrs. Paul Tannery and Ch. Henry, vol. II, Correspondance, p. 26.

## FOOTNOTES TO CHAPTER XVII

[1] Cf. Chapter XIII, Section 1 and Chapter XV, Section 2.
[2] *Universae Geometriae mixtaeque Mathematicae synopsis, et bini refractionum demonstratarum tractatus*; studio et opera F. M. Mersenni M.; Parisiis, apud Antonium Bertier, via Jacobaea, sub signo Fortunae, MDCXLIV. (T. N.: The Latin reads, *A Synopsis of both Universal Geometry and Mathematics and two treatises on the Demonstration of Refraction*, published through the zeal and effort of F. M. Mersenne of the Minims in Paris by Antoine Bertier, Rue Jacobins under the Sign of Fortuna, 1644.)
[3] Mersenne, *Les Méchaniques de Galilée*, p. 25. Cf. the dedicatory letter addressed to M. de Ruffuge.
[4] F. Marini Mersenni, ordinis minimorum, *Harmonicorum libri*: Lutetiae Parisiorum, sumptibus Guglielmi Baudry, MDCXXXVI; Liber secundus, de causis sonorum, Propositio XXIV, Corollarium IV, p. 22. (T. N.: The Latin title reads, *Books on Harmony* by Father Marin Mersenne of the Order of the Minims, Paris at the expense of Guillaume Baudry 1636: Book Two, On the Causes of Sound. Proposition XXIV, Corollary IV, p. 22.)
[5] Mersenne, loc. cit., Proposition VII, Corollaire VIII, p. 121.
[6] Id., ibid., Proposition X, Corrollaire I, p. 124.
[7] Id., ibid., Proposition X, Corrollaire II, p. 124.
[8] Id., *Harmonie universelle. A. Treatise on the nature of sound and motion in all kinds of bodies. Third Book: On the motion, tension, force, weight and other properties of harmonic chords and other bodies.* Proposition XIX, p. 207.
[9] Mersenne, Harmonie universelle, *Nouvelles observations physiques et mathématiques*, V[e] observation, pp. 16—17.
[10] Id., ibid., Book VIII. De l'utilité de l'harmonie et des autres parties des mathématiques, Proposition XVIII, pp. 61 et seq. (T. N.: The French reads, On the usefulness of harmony and on other parts of mathematics. Proposition XVIII, pp. 61 and following.)

[11] Here is some information on this bizarre work. It is entitled: F. Marini Mersenni Minimi *Cogitata physico-mathematica, in quibus tam naturae quam artis effectus admirandi certissimis demonstrationibus explicantur.* Parisiis, sumptibus Antonii Bertier: via Jacobea, MDCXLIV. (T. N.: The Latin title reads, *Physical and Mathematical Reflections in which the miraculous effects of both nature and art are explained by means of the most absolutely certain demonstrations.* Paris, at the expense of Antoine Bertier, Rue Jacobin, 1644.) A *Praefatio praefationum* unites the different treatises which make up this volume. (T. N.: The Latin reads, *A Preface to the Prefaces.*) It is followed by a Table of Contents: Tractatus isto volumine contenti: I. De mensuris, ponderibus et nummis Hebraicis, Graecis et Romanis ad Gallica redactis. — II. De hydraulicopneumaticis phaenomenis. — III. De Arte nautica, seu Histiodroma et Hydrostatica. — IV. De Musica theorica et practica. — V. De mechanicis phaenomenis. — VI. De Ballisticis, seu Acontismologicis phaenomenis. (T. N.: The Latin reads, Treatises contained in this volume: I. Hebrew, Greek and Roman measures, weights, and coins with French equivalents. — II. Hydraulic and Pneumatic Phenomena. — III. On the Art of Sailing or "Histiodroma" and Hydrostatics. — IV. On the Theory and Practices of Music. — V. On Mechanical Phenomena. — VI. On Ballistics or "Acontismology.") A *Prefatio generalis* without pagination precedes a text of 40 pages: *De Gallicis, Romanis, Hebracis et aliis mensuris, ponderibus et nummis.* (T. N.: The Latin reads, *A General Preface and On the Measures, Weights and Coins of the French, Romans, Hebrews, et al.*) This treatise is a revised draft, more correct than the one which we shall encounter further on.

A variant title which reads, *Hydraulica, pneumatica, arsque navigandi, Harmonia theorica, practica et mechanica phaenomena,* autore M. Mersenno M., Parisiis, sumptibus Antonii Bertier, via Jacobea, MDCXLIV. (T. N.: The Latin title reads, *Hydraulics, Pneumatics, and the Art of Navigation, the Theory and Practice of Harmony and Mechanical Phenomena,* written by M. Mersenne of the Minims, Paris, at the expense of Antoine Bertier, Rue Jacobins, 1644.) This variant title is followed by: 1. A dedicatory letter to the Marquis d'Estampes Valençay. 2. the *Tractatus de mensuris, ponderibus atque nummis tam Hebraicis quam Graecis et Romanis ad Parisiensia expensis* (pp. 1—40) and 3. the *De hydraulicis et pneumaticis phaenomenis* (pp. 41—214). (T. N.: The Latin titles read, *Treatise on Hebrew, Greek and Roman Measures, Weights and Coins with Parisian* (sic) *equivalents; On Hydraulics and Pneumatic Phenomena.*) A second variant title is: *Ars navigandi super et sub aquis, cum Tractatu de Magnete et Harmoniae theoreticae, practicae et instrumentalis. Libri quatuor.* Parisiis, sumptibus Antonii Bertier, via Jacobaea, sub signo Fortunae, MDXLIV. (T. N.: The Latin reads, *The Art of Navigation on and below water with a treatise on the Magnet and on theoretical, practical and instrumental harmony. Four Books.* Paris, at the expense of Antoine Bertier, Rue Jacobins, under the Sign of Fortuna, 1634.) This second variant title gives the contents of pages 225 to 370, in particular, hydrostatics occupies pages 225—233. We find still another variant title which this time contains a change in pagination and reads F. Marini Mersenni Minimi *Tractatus mechanicus, theoricus et practicus.* Parisiis, sumptibus Antonii Bertier, via Jacobaea, sub signo Fortunae, MDCXLIV. T. N.: The Latin reads, Marin Mersenne of the Minims; *A Treatise on the Theory and Practice of Mechanics,* Paris, at the expense of Antoine Bertier, Rue Jacobins, under the Sign of Fortuna, 1644).

This treatise is comprised of ninety-six pages. A final variant title containing a third change in pagination reads as follows: F. Marini Mersenni Minimi *Ballistica et Acontismologia, in qua sagittarum, jaculorum et aliorum missilium jactus, et robur arcuum explicantur.* Parisiis, sumptibus Antonii Bertier, via Jacobaea, MDCXLIV. (T. N.: The Latin reads, Marin Mersenne of the Minims: *Ballistics and "Acontismology" in which the hurling of arrows, javelins, and other missiles as well as the power of the bow are explained.* Paris, at the expense of Antoine Bertier, Rue Jacobins, 1644. This last part contains 140 pages.) An *Index amplissiumus omnium rerum quas hoc primum volumen complectitur* ends the work. (T. N.: The Latin reads, An extensive index of all the subjects contained in this first volume.)

[12] T. N.: The Latin title reads, *Treatise on Mechanics.*

[13] T. N.: The Latin reads, Introduction.

[14] Mersenne, *Tractatus mechanicus*, p. 2.

[15] Id., ibid., p. 10 and p. 18.

[16] Mersenne, *Tractatus mechanicus*, p. 23.

[17] Id., ibid., p. 36.

[18] T. N.: The Latin reads, Most Illustrious Man.

[19] He, to be sure, was duly authorized to do so because of a letter by Descartes sent on February 2nd, 1643 (*Oeuvres de Descartes*, published by Ch. Adam and Paul Tannery, Correspondance, Vol. III, p. 611. In this letter, Descartes informed Mersenne that several persons in Holland were already in possession of a copy of his statics. These copies derived from the version sent to Constantin Huygens; some were in French and others translated into Latin. It is one of those Latin translations which the Abbot Nicolas Poisson, a Priest of the Oratoire Congregation, translated back into French and had published in 1668.

[20] *Traité de la Mécanique* written by M. Descartes, and *Abrégé de la Musique*, by the same author, put into French with the necessary explanations by N. P. P. D. L.: Paris, Angot, 1668. This translation of the *Traité de la Mécanique* was reprinted in 1724 in Paris, together with the *Method*, the *Dioptrics* and the *Meteors*. Victor Cousin included it in Vol. V of his edition of the *Oeuvres de Descartes* (Paris, 1825). (T.N. The French titles read, *Treatise on Mechanics* and a *Short Treatise on Music.*) On the other hand, Johan Daniel Mayor, discovered a French copy of the *Explanation of engines*, translated it into Latin and had it published in Kiel in 1672.

[21] Mersenne, *Tractatus mechanicus*, Propositio III, p. 12.

[22] Id., ibid., Propositio IX, p. 34.

[23] Id., ibid., Propositio VII, p. 25.

[24] Id., ibid., pp. 47—56.

[25] Mersenne, *Ballistica et Acontismologia*, pp. 10—18.

[26] T. N.: This reference can be found in Stevin's *Mathematicorum Hypomnematum de Statica*, Liber primus Staticae.

[27] Id., *De hydraulicis et pneumaticis phaenomenis*, p. 141.

[28] *Nova de Machinis Philosophia in qua, Paralogismis Antiquae detectis, explicantur Machinarum vires unico principio, singulis immediato*, authore Nicolao Zucchio Parmensi, Societatis Jesu, olim professore Mathematicae in Collegio Romano. Accessit exclusio vacui contra nova experimenta, contra vires Machinarum. Promotio Philosophiae Magneticae; ex ea novum argumentum contra systema Pythagoricum, Romae.

typis haeredum Manelphii, MDCXLIX. — Une première édition de cet ouvrage avait été donnée à Paris en 1646; les maitières mentionnées dans le titre de la seconde édition à partir du mot *accessit* ne figuraient pas dans la première édition. (T. N.: The Latin and French read, *The New Philosophy of Machines, in which, by uncovering the faulty Arguments of Antiquity, the capabilities of machines are explained by a unique, singular and immediately evident principle.* Nicholas Zucchi of Parma of the Society of Jesus, formerly Professor of Mathematics in the Collegium Romanum. A first edition of this work has been printed in Paris in 1646. The subject matter mentioned in the title of the second edition beginning with the word "accessit" is not included in the first edition.)

[29] Zucchi, loc. cit., pars secunda, sectio V, 2, p. 45.

[30] Zucchi, loc. cit., pars tertia, sectio III, p. 86.

[31] *Tractatus physicus de motu locali, in quo effectus omnes, qui ad impetum, motum naturalem, violentum et mixtum pertinent, explicantur et ex principiis physicis demonstrantur*; auctore Petro Mousnerio, Doctore medico; cuncta excerpta ex praelectionibus R. P. Honorati Fabry Societatis Jesu, Lugduni, apud Joannem Champion, in foro Cambii, MDCXLVI. (T. N.: The Latin reads, *A Treatise in Physics on Local Motion, in which all effects pertaining to impetus, natural, violent and mixed motion are explained and demonstrated by physical principles*, by Pierre Mousnier, Doctor of Medicine, the entire work taken from the lectures of Honoré Fabri of the Society of Jesus, Lyon, Jean Champion, Place Cambi, 1646.)

[32] T. N.: The Latin title reads, *On motion on diverse planes.*

[33] Id., ibid., p. 195, Axioma I.

[34] Id., ibid., p. 196, Theorema V.

[35] T. N.: The Latin title reads, A heavy body is heavier in descent the more direct its motion is towards the center.

[36] Pierre Mousnier, loc. cit., p. 219.

[37] Id., ibid., Appendix secunda: De principio physico-statico ad movenda ingentia pondera, p. 458. (T. N.: The Latin reads, Second Appendix: On the physical principle of statics for the motion of very heavy weights.)

[38] Mersenni *Cogitata physico-mathematica. Tractatus mechanicus*, p. 47.

[39] *Huygens et Roberval; Documents inédits* par C. Henry. Leyde, 1880. (T. N.: The French reads, *Huygens and Roberval; Unpublished Documents.*)

[40] T. N.: The virtual center is the center of action of the forces.

[41] T. N.: The French title reads, *Project for a book on mechanics dealing with compound motion.*

[42] *Divers ouvrages de mathematique et de physique par messieurs de l'Académie Royale des Sciences.* A Paris, MDCXCIII. (T. N.: The French title reads: *Diverse works on Mathematics and Physics* by the Honorable Members of the Royal Academy of Sciences; Paris, 1693.)

[43] T. N.: The French title reads, *Observations on the compisition of motion.*

[44] Bibliothèque Nationale, Latin Collection, Ms. No. 7226 — Here is the exact content of that manuscript: fol. 1: blank; fol. 2 (recto) to fol. 30 (verso): contains the *Treatise on Mechanics* of D. D. Roberval, 1645; fol. 31 (recto) to fol. 33 (verso): A mechanical demonstration — fol. 34 (recto) to fol. 54 (recto): A letter from M. de Roberval to M. de Fermat, Counselor in Toulouse, containing several propositions on mechanics; fol. 54 (verso) to fol. 56 (verso): A proposition by M. de Roberval for determining centers

of gravity sent to M. Fermat on April 1, 1645 — fol. 57 and 58 (blank), fol. 59 (recto) to fol. 82 (recto): A Lemma Marvelously Suited for the Determination of Centers of Gravity by M. de Roberval, 1645. (This fragment not only contains the lemma mentioned here, but also its application to the determination of centers of gravity of semi-circles, semi-circumferences, trochoids, of the curve associated with the trochoid and finally the centers of gravity of the triangle.) fol. 82 (verso), 83 and 84 (blank); fol. 85 (recto) to fol. 207 (recto): *Treatise on Mechanics* and more specifically on the piping and pumping of water, by M. de Roberval; fol. 207 (verso) to fol. 210 (recto): A fundamental proposition on bodies floating on water. The remaining pages of the notebook are blank.

Only one of these various texts has been published, it is the letter addressed to Fermat, written on October 11, 1636 and deals with the dispute over the proposition on geostatics. The beginning of this letter was published in Toulouse in 1679 in the *Various Works on Mathematics* of M. Pierre de Fermat, pp. 138—141. The letter was published in its entirety by Paul Tannery and Charles Henry in their edition of the *Oeuvres* of Fermat, vol. II, Correspondance, Art. XIV, p. 75. All of the other fragments remain unpublished, but are certainly worthy of publication.

[45] T. N.: The Latin reads, A Lemma. Cf. footnote No. 44, above.

[46] This is the name by which Roberval designates the curve called the roulette by Pascal and which we commonly call today the cycloid, according to the proposition of Beaugrand.

[47] T. N.: The Latin title reads, *Book Four, which relates an extraordinary case of plagiarism.*

[48] T. N.: The French title reads, *History of the Roulette.*

[49] *Oeuvres Complètes* of Blaise Pascal, vol. III, p. 338, Paris, Hachette, 1880.

[50] T. N.: The French title reads, *Fundamental Proposition on bodies floating in water.*

[51] T. N.: The French title reads, *Project for a book on mechanics dealing with the composition of velocities.*

[52] This treatise was also sold separately in Paris by Richard Charlemagne, Rue des Amandiers, à la Vérité Royalle, 1636.

[53] T. N.: The French title reads, *Treatise on Mechanics and especially on the piping and pumping of water.*

[54] Roberval, loc. cit., folio 176, verso.

[55] Pascal, *Nouvelles experiences touchant le vide; au lecteur* (*Oeuvres complètes de Blaise Pascal*, Ed. Hachette 1880; p. 1) (T. N.: The French reads, *New Experiments on the Vacuum.*)

[56] Cf. P. Duhem, "Bernardino Baldi, Roberval and Descartes" (*Bulletin Italien*, vol. VI, January 1906).

[57] Cf. Chapter XV, Second Period, p. 346.

[58] Bibliothèque Nationale (Latin collection), Ms. 7226, fol. 89, recto.

[59] Bibliothèque Nationale (Latin collection), Ms. 7226, fol. 99, verso.

[60] Because of an obvious error of the copyist, the text does not contain the bracketed words, but merely reads, "than it."

[61] Johannis Wallis *Mechanica, sive de Motu. Tractatus geometricus.* Pars prima, in qua De motu generalia, De gravium descensu et motuum declivitate, De libra. Londini MDCLXIX. — Pars secunda, quae est de centro gravitatis ejusque calculo. Londini,

MDXLXX. — Pars tertia, in qua De vecte, . . . , De cuneo, De elatere et resilitione seu reflexione, De hydrostaticis et aeris aequipondio, variisque quaestionibus mechanicis. Londini, MDCLXXI. — Reprinted in: Johannis Wallis *Opera mathematica*. Volumen primum. Oxoniae, e Theatro Sheldoniano, MDCXCV. (T. N.: The Latin title reads, John Wallis, *Mechanics or On motion. A Treatise on Geometry*. Part I, which treats of motion in general, of the descent of heavy bodies and of the declivity of motion, and of the balance. London, 1669. Part II, the determination of the center of gravity, London, 1670. Part III, which treats of the lever, . . . , the wedge, of elasticity and resilience or flexibility, of hydrostatics and equilibrium in the atmosphere and various qeustions of mechanics. London, 1671 (Reprinted in John Wallis, *Mathematical Works*, Vol. I, Oxford, at the Sheldon Theater, 1695.)

[62] Johannis Wallis, *Mechanica*. Pars prima, Cap. 1; De motu generalia.

[63] It is an obvious error when Wallis says in the text: *tempus* (time) instead of *celeritatem* (velocity).

[64] T. N.: The Latin reads, I call *momentum* that which is conducive to the production of motion, *impedimentum* that which opposes motion or impedes it. *Momentum* derives from the verb to move and *impedimentum* from the verb to impede. By *momentum* I am referring to the [product of] motion force and velocity. The larger these two are, the more motion will be effected. By *impedimentum*, I am referring to the [product of] resistance and distance (displacement). The larger these two are, the more motion will be impeded.

[65] Johannis Wallis, *Mechanica*, Pars prima, Cap. II. De gravium descensu et motuum declivitate. (T. N.: The Latin title reads, John Wallis, *Mechanics*, Part I, Chapter II, On the descent of heavy bodies and the declivity of motion.)

[66] Johannis Wallis, *Mechanica*, Pars prima, Cap. I, Art. XII.

[67] Johannis Wallis, *Mechanica*, Pars prima, Cap. II, Art. XII.

[68] Id., ibid., Pars prima, Cap. II, Prop. V.

[69] Id., ibid., Pars prima, Prop. VI and VIII.

[70] T. N.: The Latin reads, Making the necessary adjustments, the same is true for any motor force whatsoever.

[71] Cf. id., ibid., Cap. III, On the balance, where the influence of Torricelli is obvious.

[72] Cf. id., ibid., Cap. III, in particular, cf. especially Prop. XIV and the two scholia.

[73] Johannis Wallis, *Mechanica*, Pars prima, Cap. II, Prop. XV.

[74] Id., ibid., Prop. XVII, Scholium.

[75] *La Statique ou la science des forces mouvantes*, by Father Ignatius Gaston Pardies of the Society of Jesus, Paris with Sebastien Mabre-Cramoisy, Royal Printer, Rue St. Jacques, at the sign of the Storks, MDCLXXIII, Preface. (T. N.: The French title reads, *Statics or the Science of moving forces*, 1673.)

[76] *Histoire de l'Académie Royale des Sciences*, vol. I, from its founding in 1666 to 1686. Paris, MDCCXXXIII, p. 199.

[77] R. P. Claudii Francisci Milliet Dechales, Camberiensis, e Societate Jesu, *Cursus seu Mundus mathematicus*. Tomus secundus, complectens Geometriam practicam, Staticam, Geographiam, Tractat, de Magnete, Architectionicam civilem, Artem tignariam, et Tractat. de Lapidum sectione. — Editio altera, ex manuscriptis Authoris aucta et amendata, opera et studio R. P. Amati Varcin, ejusdem Societatis. — Lugduni, apud Anissonios, Joan Posuel et Claud. Rigaud. MDCXC. The first edition, in two volumes,

of the *Cursus seu Mundus Mathematicus* was published in Lyon in 1674; I was unable to consult it. (T. N.: The Latin title reads, Father Claude François Milliet Dechales, Chambéry, of the Society of Jesus, *The Course or the Mathematical Universe*, vol. II, containing practical geometry, statics, geography, a treatise on the magnet, civil architecture, carpentry, a treatise on stone cutting. Second Edition, enlarged and revised from the manuscripts of the author through the efforts of Father Amatus Varcin of the same society. Lyon, published by Anissonios, Jean Posuel, and Claude Rigaud, 1690.)

[78] Ibid., Tractatus nonus: Statica, seu de Gravitate Terrae. Liber octavus: Proprietates centri gravitatis et lineae directionis. (T. N.: The Latin title reads, Ibid., Ninth Treatise: Statics or on the gravity of the earth. The Properties of the Center of Gravity and its line of direction.)

[79] Ibid., Tractatus octavus: Mechanica. Liber Primus: De vera cause et principio augmenti potentiae per machinam. (T. N.: The Latin title reads, Ibid., Eighth Treatise: Mechanics. Book I, On the True Cause and Principles for the increase of power by machines.)

[80] *Cursus seu Mundus mathematicus.* Tractatus nonus: Statica, seu de Gravitate Terrae. Liber tertius De descensu gravium in planis inclinatis et funependulis: Definitiones — Liber quartus: De aequiponderantibus. Propositio IV. (T. N.: The Latin title reads, *The Course or Mathematical Universe.* Ninth Treatise: Statics, or on the gravity of the earth. Book 3, On the descent of heavy bodies on inclined planes, and when suspended by ropes. Definitions — Book 4, On equilibrium, Proposition IV.)

[81] Ibid. tractatus nonus: Statica, seu de Gravitate Terrae. Liber quartus: De aequiponderantibus. Prop. XV. (T. N.: The Latin reads, Ibid. Ninth Treatise: Statics or On the Gravity of the Earth. Book 4: On equilibrium, Proposition XV.)

[82] Ibid. Tractatus octavus: Mechanica. Liber primus: De vera cause et principio augmenti potentiae per machinam, p. 168. (T. N.: The Latin reads, Ibid. Eighth Treatise. Mechanics. Book I, On the True Cause and Principles for the increase of power by machines.)

[83] Ibid. Tractatus nonus: Statica seu Gravitate Terrae. Liber tertius: De descensu gravium in planis inclinatis et funependulis. Propositio VIII. (T. N.: The Latin reads, Ibid. Ninth Treatise: Statics, or On the Gravity of the Earth. Book 3: On the descent of heavy bodies on inclined planes and when suspended by ropes. Proposition VIII.)

[84] Ibid. Tractatus octavus: Mechanica. Liber primus: De vera causa et principio augmenti potentiae per machinam. Prop. XVII. (T. N.: The Latin reads, Ibid. Eighth Treatise. Mechanics. Book I, On the True Cause and Principles for the increase of power by machines.)

[85] *Cursus seu Mundus mathematicus,* loc. cit., Prop. XIX.

[86] Ibid., loc. cit., Prop. XVIII.

[87] Ibid., loc. cit., Prop. XVII.

[88] Ibid., loc. cit., Prop. XIV.

[89] Ibid. *Tractatus octavus: Mechanica.* Liber secundus: De vecte. Propositio X. (T. N.: The Latin reads, Ibid. *Eighth Treatise. Mechanics.* Book 2: On the lever. Proposition X.

[90] Ibid. *Tractatus nonus: Statica seu de Gravitate Terrae.* Liber tertius: De descensu gravium in planis inclinatis et funependulis. Definitiones.

[91] *Cursus seu Mundus mathematicus.* Tractatus nonus: Statica. Liber quartus: De aequiponderantibus. Propositio IV.

[92] Ibid. Liber tertius: De descensu gravium in planis inclinatis. Prop. II.

93 Ibid., loc. cit., Prop. I.
94 Ibid., loc. cit., Prop. X et XI.
95 Ibid. Liber octavus: Proprietates centri gravitatis et lineae directionis. Prop. I. (T. N.: The Latin reads, Book Eight: Properties of the center of gravity and the line of direction.)
96 *Cursus seu Mundus mathematicus.* Tractatus nonus: Statica. Liber quartus: De aequiponderantibus. Petitio IV.
97 Ibid. Liber primus: Digressiones physicae. Digressio X. (T. N.: The Latin reads, Ibid., Book I: Digressions on Physics. Digression X.)
98 *Terra machinis mota ejusque gravitas et dimensio.* Dissertationes duoa quas . . . publice exposuit . . . Antonius Comes de Montfort. Authore Paulo Casato e Societate Jesu. Romae, typis haeredum Corbeletti, MDCLV. (T. N.: The Latin title reads, *The Earth Moved by Machines and its Gravity and Size.* Two dissertations which . . . were publicly expounded . . . by Anthony, Count of Montfort. By Paolo Casati of the Society of Jesus, Rome, in the Press of the Corbeletti heirs, 1655.)
99 *Terra machinis mota.* Dissertationes geometricae, mechanicae, physicae, hydrostaticae, in quibus machinarum conjugatarum vires inter se comparantur; multiplici nova methodo Terrae magnitudo et gravitas investigatur; *Archimedes Terrae motionem spondens ab arrogantiae suspicione vindicatur.* Authore Paulo Casato, e Societate Jesu. Romae, ex typographia Ignatii de Lazaris, MDCLVIII. (T. N.: The Latin title reads, *The Earth Moved by Machines.* Dissertations on geometry, mechanics, physics, and hydrostatics, in which the capacities of integrated machines are compared. The magnitude and gravity of the earth are investigated by a complex new method. Archimedes' vow to move the earth is vindicated of the suspicion of arrogance. Paolo Casati of the Society of Jesus, Rome. In the Press of Ignatius de Lazaris, 1658.)
100 T. N.: Duhem translates Father Casati to read arrogantly: Give me a point of support and I shall shake the earth.
101 R. P. Pauli Casati Placentini, Societ. Jesu, *Mechanicorum libri octo, in quibus uno eodemque principio vectis vires physice explicantur et geometrice demonstrantur, atque machinarum omnis generis componendarum methodus proponitur.* Lugduni, apud Anissonios, Joan. Posuel et Claudium Rigaud, MDCLXXXIV. (T. N.: The Latin title reads, Father Paolo Casati of Plaisance, Society of Jesus, *Eight Books on Mechanics in which in accordance with a single principle, the forces of a lever are explained physically and demonstrated geometrically. A method for constructing machines of every sort is proposed.* Lyon, published by Anissonios, Jean Posuel and Claude Rigaud, 1684.)
102 Id., ibid. Liber primus: De centro gravitatis. (T. N.: The Latin reads, Id., ibid. First Book, The Center of Gravity.)
103 *Les Manuscrits de Léonard de Vinci*, Ms. I de la Bibliothèque de l'Institut, fol. 57[9], verso.
104 P. Casati, *Mecanicorum libri octo*, lib. II, Cap. I, p. 130. (T. N.: The Latin title reads, *Eight Books on Mechanics*, Book II, Chapter I, p. 130.
105 P. Casati, *Mecanicorum libri octo*: liber primus: De centro gravitatis; Cap. XI: Quomodo animalium motus ordinentur ex centro gravitatis. (T. N.: The Latin reads, Chapter 11, How the motion of animals is governed by the center of gravity.)
106 Id., ibid. Cap. XIII: Qua ratione minuatur gravitatio in plano inclinato. (T. N.: The Latin reads, Id., ibid. Chapter 13, How gravity diminishes on an inclined plane.)
107 Id., ibid. Cap. XIV: qua ratione corpus gravitet in planum inclinatum; p. 88. (T. N.:

The Latin reads, Id., ibid. Chapter 14, How a body exerts weight on an inclined plane; p. 88.)

[108] P. Casati, *Mecanicorum libri octo*; liber primus: De centro gravitatis: Cap. XV: Inquiruntur rationes gravitationis corporum suspensorum; p. 95. (T. N.: The Latin reads, The reasons for the gravity of suspended bodies are examined.)

[109] Id., ibid., p. 100.

[110] Id., ibid., liber secundus: De causis motus machinalis. (T. N.: The Latin reads, Book Two, On the causes of motion in machines.)

[111] Id., ibid., Cap. II: Impetus motum proxime efficientis natura explicatur; p. 142. (T. N.: The Latin reads, Chapter II, The Nature of the Impetus causing motion is explained in detail.)

[112] P. Casati, *Mecanicorum libri octo*; liber secundus: De causis motus machinalis; Cap. V; In quo machinarum vires sitae sint; pp. 171—172. (T. N.: The Latin title reads, *Eight Books on Mechanics*. Book 2, On the cause of the motion in machines. Chapter V. How the forces of machines are to be positioned.)

[113] Sous forme de deux fragments que l'on trouvera dans: Cyrano de Bergerac, *Histoire comique des états et empires de la lune et du soleil ou Voyage dans la lune*. Nouvelle édition par P. L. Jacob, Bibliophile, Paris, 1858. Ces deux fragments furent publiés pour la première fois, en 1662, dans les *Nouvelles oeuvres* de Cyrano. Rohault était certainement l'auteur de cette publication et de la préface qui y fut mise. (T. N.: The French reads, In the form of two fragments which can be found in: Cyrano de Bergerac, *A Comical History of the States and Empires of the Moon and the Sun* or *A Voyage to the Moon*, New Edition by P. L. Jacob, Bibliophile, Paris, 1858. These two fragments had originally been published in 1662 in the *New Works* of Cyrano. Rohault was most certainly its publisher and the author of its preface.)

[114] *Traité de Physique*, par Jacques Rohault. A Paris, chez la veuve de Charles Savreux, libraire juré, au pied de la Tour de Notre Dame, à l'Enseigne des Trois Vertus, MDCLXXI. (T. N.: The French reads, *Treatise on Physics* by Jacques Rohault. In Paris at the widow of Charles Savreux, certified bookseller, at the foot of the Tower of Notre Dame, under the Shop-sign of the Three Virtues, 1671.)

[115] *Preface* written by Clerselier for the *Oeuvres posthumes* of his son-in-law Jacques Rohault.

[116] *Oeuvres posthumes* de M. Rohault. A Paris, chez Guillaume Desprez, rue St. Jacques à S. Prosper, et aux Trois Vertus, au dessus des Mathurins. MDCLXXXII. *Traité des Méchaniques*, pp. 479—594. (T. N.: The French reads, *Posthumous Works* by M. Rohault. Paris, published by William Desprez, Rue St. Jacques, at S. Prosper and at the Sign of the Three Virtues, above the Mathurins, 1682. *Treatise on Mechanics*, pp. 479—594.)

[117] Rohault, *Traité de physique*. First part, Chapter X: On motion and rest.

[118] This discourse, as well as the other works written by Father Pardies which we must discuss later was reprinted in the *Oeuvres* of Father Ignatius Gaston Pardies, of the Society of Jesus and contains: 1. *The Elements of Geometry*; 2. *A discourse on local motion*; 3. *Statics or the science of moving forces*; 4. *Two machines capable of making quadrants*; 5. *A discourse on the understanding of animals, with an appendix in this new edition of a table for understanding the Elements of Geometry according to Euclid*. Lyon, at the Brothers Bruyset, rue Mercière, at the Sign of the Sun, 1725.

[119] T. N.: The French title reads, *Discourse on local motion.*
[120] *La Statique ou la Science des forces mouvantes*, by Father Ignatius Gaston Pardies of the Society of Jesus. Paris, printed by Sebastian Mabre-Bramoisy, Royal Printer, Rue St. Jacques, at the Sign of the Storks, 1673. Second edition, 1674. (T. N.: The French title reads, *Statics or the Science of Moving Forces.*)
[121] Id., ibid., p. 40.
[122] Id., ibid., p. 40.
[123] Pardies, loc. cit., pp. 110 and seqq.
[124] Id., ibid., p. 99.
[125] Id., ibid., p. 101.
[126] Id., ibid., p. 102.
[127] *Traitez de Méchanique, de l'équilibre des solides et des liqueurs.* (T. N.: The French title reads, *Treatise on Mechanics, on the equilibrium of solids and liquids.*) In which one will find the causes of the effects of all machines and can measure their forces in a particular way; other new machines are also proposed in this treatise. By Father Lamy, a priest of the Order of Orators. Paris, with André Pralard. Rue Saint Jacques, à l'Occasion, 1679. *Treatise on Mechanics, on the equilibrium of solids and liquids.* New Edition, including a new way of demonstrating the principal theorems of this science. By Father Lamy, a priest of the Order of Orators. Paris, with André Pralard. Rue Saint Jacques, à l'Occasion, 1687. This second edition is, in reality, the same as the first, but the subtitle has been changed and an addition made which we will discuss in the following article. A third edition bears the same title as the first, but is followed by these words: Reviewed and corrected by the Reverend Father Bernard Lamy, a priest of the Order of Orators. Paris, with Denys Mariette, rue Saint Jacques, à Saint Augustin, 1701.
[128] Lamy, loc. cit., p. 74.
[129] Id., ibid., p. 76.
[130] Lamy, loc. cit., p. 117.
[131] Id., ibid., p. 79.
[132] Lamy, loc. cit., p. 121.
[133] Id., ibid., p. 125.
[134] T. N.: In this passage, Lamy shows his ignorance of the Law of the Composition of Forces.
[135] Id., ibid., p. 121.
[136] Lamy, loc. cit., p. 122.
[137] Id., ibid., p. 131.
[138] T. N.: Father Lamy means by "inclination" the length of the plane.
[139] Id., ibid., p. 135.
[140] T. N.: Father Lamy is referring to a device discussed earlier by Leonardo da Vinci. (Cf. Vol. I, pp. 120—122.)
[141] Id., ibid., p. 137.
[142] Lamy, loc. cit., p. 139.
[143] Johannis Alphonsi Borelli, Neapolitani Matheseos professoris, *De motu animalium.* Pars prima. Romae, MDCLXXX. Pars secunda, Romae, MDCLXXXI. Editio altera. Lugduni in Batavi, MDCLXXXV. (T. N.: The Latin title reads, *On the motion of animals*, Giovanni Alphonso Borelli, Professor of Mathematics at Naples, Part I, Rome

1680. Part II, Rome 1681. Second Edition, Leyden, 1685.) In 1710 Leyden an edition was published to which was appended a dissertation, *On the motion of muscles*, from the pen of Jean Bernoulli. In this supplemented form the work of Borelli was reprinted several times, for example, in Naples in 1734. The last edition appeared in the The Hague in 1743.

[144] Id., ibid., Pars prima, Cap. XIII: Lemmata pro musculis squorum fibrae non sunt parallelae et oblique trahunt. (T. N.: The Latin reads, Part I, Chapter 13, Lemmas on muscles whose sinews are not parallel and pull obliquely.)

[145] Id., ibid., Pars prima, Cap. XIII: Digressio (following proposition 69).

[146] T. N.: The Latin reads, an illustrious modern Geometer.

[147] Varignon, *Project d'une nouvelle Méchanique. Avec un Examen de l'opinion de M. Borelli sur les proprietez des Poids suspendus par des Cordes.* In Paris, with the widow of Edme Martin, Jean Boudot, and Estienne Martin, rue St. Jacques, at the sign of the Golden Sun, 1687.

[148] Cf. Chapter VI, Section 2.

[149] *Traité de Méchanique et spécialement de la conduitte et élévation des eaux*, by Roberval (Bibliothèque Nationale, Latin collection, Ms. No. 7226, folio 145, recto.) (T. N.: The French title reads, *Treatise on Mechanics with emphasis on the piping and pumping of water.*)

[150] Cf. Chapter XIII, Section 2.

[151] *Divers ouvrages* de M. Personier (sic) de Roberval. *Observations sur la Composition des Mouvemens et sur let moyen de trouver les Touchantes des lignes courbes.* First printed in a collection, entitled: *Divers ouvrages de mathématiques et de Physique par Messieurs de l'Académie Royale des Sciences*, Paris, 1693 and reprinted in the Transactions of the Academy of Sciences from 1666 to 1699; Vol. VI, 1732; p. 1. (T. N.: The French titles read respectively, *Diverse Works* by M. Personier (sic) de Roberval, *Observations on the Composition of Velocities and on the means of finding the tangents of curved lines.*)

[152] Roberval, loc. cit., p. 2.

[153] Id., ibid., p. 9.

[154] Roberval, loc. cit., p. 10.

[155] Id., ibid., p. 2.

[156] Id., ibid., p. 3.

[157] Roberval, loc. cit., p. 4.

[158] Id., ibid., p. 6.

[159] T. N.: The French title reads: *Observations on the Composition of Velocities.*

[160] Id., ibid., p. 90.

[161] Foreword to the *New Mechanics* of Varignon.

[162] T. N.: The French reads, *History of the Republic of Letters.*

[163] Pierre Varignon, *A General Demonstration of the Use of the Block and Tackle*, which appeared in the *Histoire de la République des Lettres*, May 1687, p. 487. I was unable to obtain this work. I am transcribing here what Lagrange says about it (*Mécanique analytique*, First Part, Section 1, art. 13): "In this work, the author analyzes the equilibrium of a weight supported by a rope which runs over a pulley and extends out obliquely. He neither uses nor mentions the principle of the Composition of Forces, but he uses known theorems on weights supported by ropes and he quotes the statics of

Pardies and Dechales. In a second demonstration, he reduces the problem of the pulley to that of the lever by considering the line which joins the two points at which the ropes leave the pulley as a lever loaded with the weight attached to the pulley and with its extremities being pulled by the two segments of the rope which is supported by the pulley." One can see, as Lagrange remarks, that the foreword to the *New Mechanics* "lacks in accuracy" by claiming that Varignon "made use of composite motion" in his work on the block and tackle.

[164] *Project for a New Mechanics* with an analysis of the view of Borelli on the properties of weights suspended by ropes. (anonymous). Paris, with the widow of Edme Martin, Jean de Baudot and Estienne Martin. Rue St. Jacques, at the sign of the Golden Sun, 1687.

[165] *The New Mechanics or Statics* followed the *Projet* which was published in 1687. Posthumous work of M. Varignon of the Royal Academies of France, England and Prussia, Royal Lecturer in Philosophy at the Royal Collège and Professor of Mathematics at the Collège Mazarin. Paris, with Claude Jombert, Rue St. Jacques, at the corner of the Rue des Mathurins, at the Statue of Notre Dame, 1725.

[166] Varignon, *Project for a New Mechanics*, Preface.

[167] Varignon, *Project for a New Mechanics*, p. 1, Axiom.

[168] Varignon, *New Mechanics or Statics*, Vol. 1, p. 3.

[169] Varignon, *Project for a New Mechanics*, p. 6 — *New Mechanics or Statics*, Vol. 1, p. 14.

[170] Varignon, *New Mechanics*, First Section, Lemma XVI, vol. 1, p. 84.

[171] *Nouvelle manière de démontrer les principaux théorèmes des élémens des Méchaniques* (a). Intended as a supplement to the *Treatise on Mechanics* by the Reverend Father Lamy, of the Order of Orators. Paris with André Pralard, Rue St. Jacques, à l'Occasion, 1687. The several pages which make up this small work were indeed appended to the earlier *Traitez de Méchanique*, of Father Lamy and the subtitle was changed to read: *Traitez des Méchanique, de l'équilibre des solides et des liqueurs*. New Edition. To which is added a new method of demonstrating the principle theorems of this science. By Father Lamy, of the Order of Orators: Paris, with André Pralard, Rue St. Jacques, à l'Occasion, 1687.

[172] T. N.: The French title reads, *History of Scholarly Works*.

[173] The *Nouvelle édition* of the *Traitez de Méchanique* by Father Lamy ends with an *Extrait du Journal des Sçavans*, from Monday, September 13, 1688. A paper intended as a response to what the author of the *Histoire des ouvrages des Sçavans* stated in April 1688, Art. 3 concerning a letter in which Father Lamy had proposed in the preceding year a new method for demonstrating the Principal Theorems of the elements of mechanics.

[174] *Philosophiae naturalis principia mathematica*, auctore Isaaco Newtono, Londini, MDCLXXXVII. (T. N.: The Latin title reads, *Mathematical Principles of Natural Philosophy* by Isaac Newton, London, 1687.)

[175] Newton, loc. cit., Definitiones. Definitio IV.

[176] T. N.: The Latin reads, This force exists because of action alone and does not remain in the body after the action.

[177] Newton, loc. cit., Axiomata, sive leges motus. Corollarium I. (T. N.: The Latin reads, Axioms, or the laws of motion. Corollary I.)

[178] *Euclides a omni naevo vindicatus sive conatus geometricus* quo stabiliuntur prima universae geometriae principia, auctore Hieronymo Saccherio, Societatis Jesu, in Ticinensi Universitate Matheseos professore. Opusculum ex$^{mo}$ Senatui Mediolanensi ab auctore dicatum. Mediolani, MDCCXXXIII. Ex typographia Pauli Antonii Montani. (T. N.: The Latin title reads, *Euclid Freed of Every Flaw or a Geometrical Enterprize*, in which the first principles of universal geometry are established by Girolamo Saccheri of the Society of Jesus, Professor of Mathematics at the University of Pavia. A treatise dedicated by the author to the illustrious Senate of Milan. Milan, at the Press of Paolo Antonio Montane, 1733.)

[179] E. Beltrami, "Un precursore italiano di Legendre e di Lobatchewski," *Rendiconti della R. Accademia dei Lincei*, t. V, p. 441; 17 mars 1889. (T. N.: The Italian title reads, *An Italian Precursor to Legendre and Lobatchewski.*)

[180] P. Mansion, *Analyse des recherches du P. Saccheri, S. J., sur le Postulatum d'Euclide*, Annales de la Société Scientifique de Bruxelles, XIV$^{e}$ année, 1889 — 1890, seconde partie, p. 46. (T. N.: The French title reads, *Analysis of the Research of Father Saccheri, Society of Jesus, on the Postulate of Euclid.*)

[181] *Neo-Statica* auctore Hieronymo Saccherio, e Societate Jesu, in Ticinensi Universitate matheseos professore, excellentissimo Senatui Mediolanensi: MDCCVIII. Ex typographia Josephi Pandulphi Malatestae. (T. N.: The Latin title reads, *Neo-statics*, by Girolamo Saccheri, of the Society of Jesus, Professor of Mathematics at the University of Pavia. For the illustrious Senate of Milan: 1708. At the Press of Joseph Pandulphus Malatesta.) I owe to the Reverend Father Thirion the information about this rare work. I take the opportunity here to express my sincerest appreciation to him.

[182] Cf. Saccheri, *Neo-Statica*, liber IV, Introductio, p. 125.

[183] Saccheri, *Neo-Statica*, Liber I, Definitiones, p. 2.

[184] Id., ibid., lib. I, Definitio 7, p. 2.

[185] Id., ibid., lib. I, Definitio 9, p. 2.

[186] Id., ibid., lib. I, Propp. IX, X, XI.

[187] Id., ibid., lib. I, Propp. XXVII et XXVIII.

[188] Saccheri, *Neo-Statica*, Liber II, Definitio 5, p. 55.

[189] Id., ibid., Liber III, Propositio I.

[190] Id., ibid., Liber III, Admonitio, p. 84.

[191] *Mechanica sive Motus Scientia, analytice exposita*, auctore Leonhardo Eulero, Academiae Imper. Scientiarum membro et matheseos sublimioris professore. Instar supplementi ad Commentar. Acad. Scient. Imper. Petropoli, ex typographia Academiae Scientiarum. An. 1736. (T. N.: The Latin title reads, *Mechanics or the Science of Motion, an analytical exposition* by Leonhard Euler, Member of the Royal Academy of Science and Professor of Higher Mathematics. A supplement to the Proceedings of the Royal Academy of Science, St. Petersburg, at the Press of the Academy of Science, 1736.)

[192] T. N.: Lagrange's very clear demonstration of this principle in the *Mécanique analytique* attests to Duhem's assertion. His demonstration is based on what he calls the Principle of Pulleys and avoids any connection with Aristotelian dynamics.

[193] Pierre Varignon, *Nouvelle Mécanique ou Statique*; section IX, Corollaire général de la Théorie précédente. Tome II, p. 174. (T. N.: The French title reads, *New Mechanics*

*or Statics*, section IX, General Corollary to the preceding Theory. vol. II, p. 174.) (T.N. 2: The communication was made on February 26, 1715. A printing error in Varignon's *Nouvelle Mécanique* resulted in the incorrect date being assigned.)

[194] One can see that Jean Bernoulli has given the name of *virtual velocities* to distances and not to velocities. This unfortunate term still exists in mechanics and many authors still use the name *Principle of Virtual Velocities* for a principle in which velocities play no role and which should be called the *Principle of Virtual Displacements*. (T. N.: Bernoulli (1667—1748) attempts to reconcile Ancient and Modern developments in his formulation. Since a compatible but arbitrary system of displacements must be used to apply the Principle of Virtual Displacements, all displacements can be divided by a constant. If this constant is the time interval in which the displacement pattern takes place, the resulting quantities have units of velocity. Thus, rather than force times displacement, one has force times velocity. Therefore, the Principle of Virtual Displacements can be called the Principle of Virtual Velocities. This development ignores the fundamental conceptual differences between the two principles.)

[195] The reader will note that Jean Bernoulli introduces several inaccurate assertions and useless restrictions in his statement. We shall not dwell any further on this trifling matter.

[196] Varignon, *Nouvelle Mécanique ou Statique*, Vol. II, p. 174.

[197] T. N.: The fourth order differential equation, which defines the shape of the deflected surface of a plate, requires four boundary conditions to be satisfied. Prior to Kirchhoff's reformulation of the problem, there were, it was thought, six physical boundary conditions.

[198] T. N.: Larzac is the name of a large region in the Massif Central of France.

[199] T. N.: The Vissec is a dry river; the Foux is the name of the source of the Vis River.

[200] T. N.: The Cévennes is a mountain range in the southeast of that region.

[201] T. N.: Claude Bernard (1813—1878), French physiologist, who showed that living processes are physical phenomena and can be studied using the experimental method.

## FOOTNOTES TO NOTE A

[1] T. N.: Cf. the Loeb Classical Library translation. Aristotle, *The Physics*, Vol. II, 249b, p. 257. *If then A is the moving agent, B the mobile body, C the distance traversed and D the time taken, then A will move one half B over the distance 2C in time D and A will move one half B over the distance C in time one half D; for so the proportion will be observed.*

[2] *Bolletino di Bibliografia e Storia della Scienze Matematiche*, pubblicato per cura di Gino Loria. Anno IX, p. 13, 1906.

[3] T. N.: Cf. the Loeb Classical Library translation. Aristotle, *Minor Works. Mechanical Problems*, 3.850b 5, p. 353 ... *so that by the use of the same force, when the motive force is farther from the [fulcrum of the] lever, it will use a greater movement.*

[4] *Simplicii in Aristotelis Physicorum libros quatuor posteriores commentaria* edidit Hermanus Diels: Berolini, 1895. Commentaria in Physicorum VII, 5, p. 1110. (T. N.:

The Latin title reads, *The Commentaries of Simplicus on the Last Four Books of Aristotle's Physics*, edited by Hermann Diels; Berlin 1895. Commentaries on the *Physics*, Book VII, 5, p. 1110.)

[5] T. N.: Duhem has accurately translated the original Greek of Simplicius.

[6] T. N.: Cf. Loeb Classical Library, Aristotle, *The Physics*, 250a7, p. 257.

[7] T. N.: Duhem's sentence is an accurate translation of the Latin sentence which follows it.

[8] T. N.: Cf. the Loeb Classical Library. *On the Heavens*, I. vi, 273b 32, pp. 49—51. W. K. C. Guthrie rather awkwardly, if not inaccurately translates: *And the proportion which the weights bear to one another, the times too (sic!) will bear to one another, e.g. if the half weight covers the distance in x, the whole weight will cover in x/2.* Duhem's translation is a more accurate rendering of the Greek.

[9] Bernardini Baldi Urbinatis, Guastallae abbatis, *In mechanica Aristotelis problemata exercitationes*; adjecta succincta narratione de autoris vita et scriptis; Moguntiae, typis et sumptibus viduae Joannis Albini, MDCXXI; p. 36. (T. N.: The Latin title reads, Bernardino Baldi of Urbino, Abbot of Guastalla *Exercises on the Mechanical Problems of Aristotle*, with a brief narration of the life and works of the author; Mainz, printed at the expense of the widow of Johan Albin, 1621, p. 36.)

[10] Joannis de Guevara, cler. reg. min., *In Aristotelis mechanicas commentarii, una cum additionibus quibusdam ad eandem materiam pertinentibus*; Romae, apud Jacobum Mascardum, MDCXXVII; p. 89. (T. N.: The Latin reads, John of Guevara, a member of the minor clergy, *Commentaries on Aristotle's Mechanical Problems, with Appendices Pertaining to the Same Subject-Matter*; Rome, Jacob Mascardus, 1627, p. 89.)

[11] T. N.: The Latin reads, "in this position."

[12] Cf. Vol. I, pp. 173—183.

[13] Cf. Vol. I, p. 173.

[14] Cf. Vol. I, p. 238.

[15] Cf. Vol. I, pp. 236—238.

[16] Cf. Vol. II, pp. 386—387.

[17] T. N.: Cf. the Loeb Classical Library translation. *Aristotle. The Physics*, Vols. I, IV, VIII, 215a25, p. 351. *We see that the velocity of a moving weight or mass depends on two conditions* (1) *The distinctive nature of the medium-water, earth, or air — through which the motion occurs, and* (2) *the comparative gravity or levity of the moving body itself, other conditions being equal.*

[18] G. Milhaud, *Études sur la pensée scientifique chez les Grecs et chez les Modernes*; Paris, 1906, pp. 112—117. (T. N.: The French reads, *Studies on the Scientific Thought of the Greeks and the Moderns.*)

[19] T. N.: The Latin term means "mathematicians."

[20] Cf. the Loeb Classical Library, *Aristotle, The Physics*, Vol. IV, viii, 215b 1, pp. 351—353.

[21] Cf. Vol. I, pp. 81—83.

[22] T. N.: Cf. the Loeb Classical Library, *Aristotle, Minor Works, Mechanical Problems*, 1, 849a, 6, p. 345—347.

[23] T. N.: Cf. the Loeb Classical Library, *Aristotle, Minor Works, Mechanical Problems*, 1, 849a, 16 p. 343. *The lesser radius always moves in its unnatural direction.*

[24] T. N.: Cf. the Loeb Classical Library, *Aristotle, Minor Works, Mechanical Problems*,

1, 849b, 16, pp. 345—347. *Nor will the proportion between the natural and unnatural movements be the same in the two circles. From what has already been said, the reason why the point more distant from the center travels more quickly than the nearer point, through impelled by the same force, and why the greater radius describes the greater arc, is quite obvious.*

## FOOTNOTES TO NOTE B

[25] Cf. Vol. I, Chapter IV, Section 2, pp. 61—71.
[26] Cf. Vol. I, note A.
[27] *Le Livre des appareils pneumatiques et des machines hydrauliques* by Philo of Byzantium. Edited and translated by Baron Carra de Vaux; Paris, 1902. Introduction, pp. 6 and 9. (T. N.: The French title reads, *Book on Pneumatic Apparati and Hydraulic Machines.*)
[28] Ibid., p. 9.
[29] Cf. Vol. I, p. 65 and Vol. II, p. 449.
[30] Pappi Alexandrini *Collectiones quae supersunt* edidit Fridericus Hultsch. Volume III, p. 1068.
[31] *Le Livre des appareils pneumatiques et des machines hydrauliques* by Philo of Byzantium, edited and translated by Baron Carra de Vaux; Paris, 1902. Introduction, pp. 5 and 14.
[32] *Les Mécaniques ou l'Élévateur* of Hero of Alexandria, published for the first time from the Arabic version of Qusta ibn Luqa and translated into French by Baron Carra de Vaux. Excerpt from the *Journal Asiatique*. Paris, 1894. Cf. in particular, pp. 25—29 of the splendid introduction by Baron Carra de Vaux. (T. N.: The French title reads, *The Mechanics or the Elevator.*)
[33] *Les Mécaniques ou l'Élévateur* of Hero of Alexandria, pp. 87—90.
[34] Id., p. 106.
[35] *Les Mécaniques ou l'Élévateur* of Hero of Alexandria, p. 28.
[36] T. N.: The Greek title reads, *On the Equilibrium of Planes* or *The Center of Gravity of Planes*. Cf. *The Works of Archimedes*, ed. by T. L. Heath, Dover Publishers, New York, 1912, pp. 188—220.
[37] T. N.: The Greek term means "of equal inclination."
[38] Id., p. 74.
[39] The name "Poseidonios" as well as his status as a Stoic philosopher are doubtful. All the more so, because the person mentioned here seems to be considered by Hero as preceding Archimedes, and because Posidonius is posterior to Archimedes. (T. N.: Posidonius' dates are ca. 135 B.C. to ca. 51 B.C., Archimedes' ca. 287 B.C. to 212 B.C.)
[40] Archimedis *Opera omnia*, ed. Heiberg, vol. II, p. 306.
[41] Pappi Alexandrini *Collectiones quae supersunt* edidit Fridericus Hultsch; volumen III; Berolini, 1878. Lib. VIII, prop. 2; pp. 1034—1035.
[42] Ch. Thurot, *Recherches historiques sur le principe d'Archimède*. Deuxième article (*Revue Archéologique*, Nouvelle Série, t. XIX, p. 47, 1869). (T. N.: The French title reads, *Historical Investigations on the Principle of Archimedes.*)

[43] Pappi Alexandrini *Collectiones quae supersunt* edidit Fridericus Hultsch, volumen III, p. 1025; Berolini, 1878.
[44] T. N.: The Greek title means, *On Floating Bodies.*
[45] Cf. Vol. I, p. 61.
[46] *Bulletino de Bibliografia e di Storia delle Scienze Matematiche e Fisiche* pubblicato da B. Boncompagni, Tomo IV, 1874, p. 472, en note.
[47] T. N.: The Latin reads, *The Book of Caraston on Euclid's On Weight.*
[48] Bibliothèque Nationale, Ms. 7377 B (fonds Latin).
[49] The Latin title reads: *On the Sector* (of a circle).
[50] *Bulletino de Bibliografia e di Storia delle Scienze Matematische e Fisiche* pubblicato da B. boncompagni. Tomo IV, 1871, p. 474, en note.
[51] Maximilian Curtze, *Über die Handschrift R. 4° 2, Problematum Euclidis explicatio* der Königl. Gymnasialbibliothek zu Thorn (*Zeitschrift für Mathematik und Physik*, XIII[ter] Jahrg., 1868; Supplement, p. 64).

## FOOTNOTES TO NOTE C

[52] *Vide infra*: Note D, *Sur les Mécaniques de Héron d'Alexandrie*, et note F, *sur le Précurseur de Léonard de Vinci.*
[53] *Les dix livres de l'Architecture* de Vitruve, corrigez et traduits nouvellement en François, avec des notes et des figures. Seconde édition reveuë, corrigée et augmentée Par M. Perrault de l'Académie Royalle des Sciences, Docteur en médecine de la Faculté de Paris. A Paris, chez Jean Bapiste Coignard. Imprimeur ordinaire du Roy, ruë S. Jacques, à la Bible d'Or., MDCLXXXIV. (T. N.: The French reads, the *Ten Books on Architecture* of Vitruvius, recently corrected and translated into French, with notes and drawings. Second revised, corrected and enlarged edition by M. Perrault of l'Academie Royale des Sciences, Doctor of Medicine of the Faculty of Paris. Paris at Jean Baptiste Coignard, Printer to the King, rue St. Jacques at the sign of the Golden Bible, 1684.)
[54] Chapter VIII, *On the force which the straight line and circular curve possess in machines designed to carry loads.*
[55] Vitruvius, ibid., p. 309.
[56] T. N.: The Latin reads, in a geometrical fashion, geometrically.
[57] Vitruvius, loc. cit., p. 310.
[58] Id., ibid., p. 312.

## FOOTNOTES TO NOTE D

[59] Pappi Alexandrini *Collectiones quae supersunt* e libris manuscriptis edidit, latina interpretatione et commentariis instruxit Fridericus Hultsch; Volumen III; Beroline 1878; pp. 1115—1135. (T. N.: The Latin title reads, *The Extant Collected Works* of Pappus of Alexandria, edited from the Manuscripts by Friedrich Hultsch; Vol. III; Berlin 1878; pp. 1115—1135.)
[60] *Les Mécaniques ou l'Élévateur* de Héron d'Alexandrie, publiées pour la première

fois sur la version arabe de Qostà ibn Lûqâ et traduites en français par M. le Baron Carra de Vaux; *Extrait du Journal Asiatique*; Paris, 1894. (T. N.: The French reads, *The Mechanics or the Elevator* of Hero of Alexandria; published for the first time based on the Arabic version of Qusta ibn Luqa and translated into French by Baron Carra de Vaux, *Extract from the Journal Asiatique*; Paris, 1894.)

[61] *Vide supra*, note B.

[62] Héron d'Alexandrie, *Les Mécaniques ou l'Élévateur*, p. 108 et p. 112.

[63] *Vide supra*, note B.

[64] Héron d'Alexandrie, *Les Mécaniques ou l'Élévateur*, Introduction, pp. 22—27.

[65] Héron d'Alexandrie, *Les Mécaniques ou l'Élévateur*, Livre II, Section IV.

[66] Héron d'Alexandrie, *Les Mécaniques ou l'Élévateur*, p. 107.

[67] *Vide supra*, Vol. I, pp. 54—55.

[68] Héron d'Alexandrie, *Les Mécaniques ou l'Élévateur*, p. 127.

[69] Héron d'Alexandrie, loc. cit., pp. 131—132.

[70] Héron d'Alexandrie, loc. cit., pp. 134—135.

[71] Héron d'Alexandrie, loc. cit., pp. 136—137.

[72] Héron d'Alexandrie, loc. cit., pp. 106—109.

[73] Héron d'Alexandrie, loc. cit., p. 137.

[74] Héron d'Alexandrie, loc. cit., pp. 149—151.

[75] T. N.: The vertical line is to one side of the hanging weight. If the cord is grasped closer to the point of support, the described arc is longer and the ascent of the weight greater than if the cord is grasped further from the point of support. Hero appears to have in mind the ancient saying commonly used to explain mechanical advantage in machines, to wit, "what is gained in force is lost in velocity."

[76] T. N.: Hero used a concept close to our modern "real energy." His contribution to mechanics is small because his works were rediscovered only recently and did not play a role in the development of modern mechanics.

## FOOTNOTES TO NOTE E

[77] Bernardino Baldi, *Cronica de'Matematici, overo epitome dell'istoria delle vite loro*, Urbino, per A. Monticelli, 1707. Art: Giordano.

[78] T. N.: The Italian reads, Giordano, from a place called Hemore, is called Hemorario.

## FOOTNOTES TO NOTE F

[79] T. N.: Duhem later retreated from this designation. In his *Études sur Léonard da Vinci*, vol. I, p. 316, Duhem concludes that Leonardo did not have access to Book I of the *De ratione ponderis*. Consequently, he suggests the title, "the Precursor of Simon Stevin."

[80] Cf. Vol. I, Chapt. VII, Section 3; pp. 98—107.

[81] *Études sur Léonard de Vinci — VII.* La Scientia de ponderibus et Léonard de Vinci.

[82] T. N.: Duhem is referring to the *Liber Jordani de ratione ponderis.*

[83] T. N.: Duhem has not presented convincing evidence that a more mature Jordanus

had not revised his earlier work. Cf. *Science in the Middle Ages*, edited by David C. Lindberg, The Science of Weights, p. 197.

[84] *Les Mécaniques ou l'Élévateur* de Héron d'Alexandrie, publiées et traduites par le Baron Carra de Vaux. *Extrait du Journal Asiatique*. Paris, 1894, Livre II, section IV.

[85] Axel Anthon Bjornbö, *Studien über Menelaos' Sphärik. Beiträge zur Geschichte der Sphärik und Trigonometrie der Griechen* (Abhandlungen zur Geschichte der Mathematischen Wissenschaften mit Einschluss ihrer Anwendungen, begründet von Moritz Cantor, XIVtes Heft, S. 147; 1902).

[86] *Les Mécaniques ou l'Élévateur* de Héron d'Alexandrie, pp. 87ff.

## FOOTNOTES TO NOTE G

[87] Cf. Vol. II, p. 286.

[88] Cf. Vol. II, p. 297.

[89] Maximilian Curtze, *Uber die Handschrift R. No. 4, 2, Problematum Euclidis explicatio der Königl. Gymnasialbibliothek zu Thorn* (*Zeitschrift für Mathematik und Physik*, XIIIter Jahrg., 1868; Supplement, p. 85).

## FOOTNOTES TO NOTE H

[90] *Vide supra*, Vol. II, pp. 317—319.

[91] *Questio de modalibus* Bassani Politi. *Tractatus proportionum introductorius ad calculationes* Suisset. *Tractatus proportionum* Thome Braduardini. *Tractatus proportionum* Nicholai Oren. *Tractatus de latitudinibus formarum* ejusdem Nicholai, *Tractatus de latitudinibus formarum* Blasii de Parma, Auctor *sex inconvenientium*, Colophon: Venetiis, mandato et sumptibus heredum quondam Nobilis Viri D. Octaviani Scoti Modoetiensis per Bonetum Localtellum Bergomensem presbyterum. Kalendis Septembribus 1505. *Contenta in hoc libello: Arithmetica communis ex* Severini Boetii *Arithmetica per* M. Johannem de Muris *compendiose excerpta — Tractatus brevis proportionum: abbreviatus ex libro de proportionibus* D. Thome Braguardini Anglici, *Tractatus de latitudinibus formarum secundum doctrinam* Magistri Nicolai Horem. *Algorithmus* M. Georgii Peurbachii *in integris — Algorithmus* Magistri Joannis de Gmunden *de minuciis phisicis*, Colophon: Impressum Vienne per Joannem Singrenium expensis vero Leonardi et Luce Alantse fratrum, Anno Domini MDXV. Decimonono die Maii. (T. N.: The Latin title reads, *An Inquiry into Modalities* by Bassanus Politus, *An Introductory Treatise on Proportions* by Suisset, *A Treatise on Proportions* by Thomas Bradwardine, *A Treatise on Proportions* and *A Treatise on the Extension of Forms* by Nicholas Oresme, *A Treatise on the Extension of Forms* by Blasius of Parma, the author of *Six Inconsistencies*. Colophon: Venice, printed by the Elder Bonetus Locatellus Bergomenses at the order and expense of the heirs of the late and noble Octavian Scotus, September 1505. The passage referred to in our text is not contained in the following editions: This treatise contains: *General Mathematics, largely excerpted from the Mathematics of Severinus Boetius* by Johannis de Muris, *Brief Treatise on Proportions: An Abbreviation of the Book on Proportions* by Thomas Bradwardine, *Treatise on the Extension of Forms* According to the Doctrine of Nicolas Oresme.

*Algorithm for Integers* by George Peurbach *Algorithm for Physics* by Johan of Gmunden, Colophon: Printed at Vienna by Johan Singrenius at the expense of the brothers Duchessa di Berri; ed unica Sorella dell'Invitissimo e Christianissimo Henrico secondo

[92] T. N.: Using the earth's mean radius as 6371.3 km and the moon's mean radius at 1738.3 km, the ratio of the two is about 3.7.

[93] *Thimonis Quaestiones in libros Metheororum*; in lib. I, quaest. VI: Utrum quatuor elementa sint continue proportionalia) (T. N.: The Latin title reads, *Themon's Questions on the Books on Meteors*; On Book I, Question VI; Whether the Four Elements are in a Proportional Relation?)

[94] R. P. F. Joannis Duns Scoti, Docotoris subtilis, Ordinis Minorum, *Meteorologicorum libri quatuor.* Lugduni, sumptibus Laurentii Durand, MDCXXIX. Lib. I, quaest. XIII: Utrum quatuor elementa sint proportionalia continue? Vide infra, Note I. (T. N.: The Latin title reads, John Duns Scotus, the subtile Doctor, of the Minor Order, *Four Books on Meteors*, London, at the expense of Lawrence Durand, 1639. Book I, Question XIII: Whether the Four Elements are in a Proportional Relation?)

[95] Di Nonio Marcello Saia dala Roccha Gloriosa in Lucania *Ragionamenti sopra la celeste sfera in lingua Italiana comune. Con uno breve Tractato dela compositione dela sfera materiale* alla Molto Eccellente e Magnanima Madama Margherita di Franza, Duchessa di Berri; ed unica Sorella dell'Invitissimo e Christianissimo Henrico secondo Re di Franza. Parisiis, Veneunt apud Franciscum Bartholomaeum, sub Scuto Veneto. 1552. Ragionamento primo. (T. N.: The Italian and Latin title reads, By Nonio Marcello Saia of the Roccha Gloriosa in Lucania *Reasonings on the Heavenly Sphere* in the common Italian language. With a short Tract on the composition of the Material Sphere to the very Excellent and Magnamimous Madame Margherita of France, Duchess of Berry, and the sole sister of the very Admired and Christian Henry II, King of France, Paris. Francisco Bartholomeo, under the Seal of Venice 1552. First printing.)

## FOOTNOTES TO NOTE I

[96] Cf. Vol. II, Chapter XV, footnote 21.

[97] R. P. F. Joannis Duns Scoti, Doctoris subtilis, Ordinis Minorum, *Opera omnia* quae hucusque reperiri potuerunt, collecta, recognita, notis, scholiis, et commentariis illustrata, a P. P. Hibernis, collegii Romani S. Isidori professoribus, jussu et auspiciis Rmi P. F. Joannis Baptistae a Campanea, ministri generalis. Lugduni, sumptibus Laurentii Durand. MDCXXXIX; 8 vol., in-fol. (T. N.: The Latin title reads, The Reverend Father John Duns Scotus, the Subtle Doctor of the Minor Order: *The Complete Works discovered so far.* Collected, edited and provided with notes, scholia and commentaries by the Irish Professors of the Roman College, St. Isidor. Under the order and auspices of the most Reverend Father John Baptiste of Campanea, the Minister General. Lyon, at the expense of Lawrence Durand. 1639; 8 vols. in folio.)

[98] R. P. F. Joannis Duns Scoti, Doctoris subtilis, Ordinis Minorum, *Diludicissima expositio et Quaestiones in octo libros Physicorum Aristotelis.* Operum tomus II. (T. N.: The Latin title reads, The Reverend Father John Duns Scotus, the Subtle Doctor of the Minor Order: *A Clear Exposition and Questions on the Eight Books of the Physics*, vol. II of the *Works.*)

[99] Censura R. P. F. Lucae Waddingi Hiberni de sequenti opere (Joannis Duns Scoti *Opera*, tomus II). (T. N.: The Latin title reads, The Reverend Father Luke Wadding: *A Critique of the Appended Work*, in *The Complete Works* of John Duns Scotus, vol. II.)
[100] R. P. F. Joannis Duns Scoti, Doctoris subtilis, Ordinis Minorum, *Meteorologicorum libri quatuor*. Opus quod non antea lucem vidit, ex Anglia transmissum. Advertat compator librorum hunc tractatum, aequo tardius ad nos delatum, ante tomum III ponendum esse ne erret. (T. N.: The Latin title reads, The Reverend Father John Duns Scotus, the Subtle Doctor of the Minor Order: *Four Books on the Meteorology*, a previously unpublished work sent from England. Let the Binder of these Books be advised of this treatise, which has reached us so late, lest he fail to place it before vol. III.)
[101] R. P. F. Lucae Waddingi de hoc Meteororum opuscula censura. (T. N.: The Latin title reads, The Reverend Luke Wadding: *A Critique of the Treatise on Meteors*.)
[102] Lib. I. quaest. 10.
[103] Lib. I. quaest. 13.
[104] Heinrich Suter, *Eine bis jetzt unbekannte Schrift des Nic. Oresme* Zeitschrift für Mathematik und Physik, XXVII, Jahrgang; 1882. Historisch-literarische Abtheilung, p. 121. (T. N.: The German title reads, *A previously unknown work of Nic. Oresme*.)
[105] T. N.: The Latin reads, These venerable questions of Master Oresme were written on the *Books on Meteorology* of the Peripatetic Aristotle, Sept. 1459.
[106] Joannis Duns Scoti *Meteorologicorum libri quatuor*, p. 33. (T. N.: The Latin title reads, John Duns Scotus, *Four Books on Meteors*, p. 33.)
[107] T. N.: The Latin reads, Whether the ocean always flows from North to South?
[108] Joannis Duns Scoti *Meteorologicorum libri quatuor*, pp. 62—63.
[109] T. N.: The Latin reads, Let the waters be gathered together . . . Cf. Genesis I, 9.
[110] On page 297, footnote 113 of Vol. II, we mentioned two collections of astronomical treatises in which the *Tractatus de Sphaera* of Campanus can be found. These collections were printed in Venice in 1528 and in 1531. In both of these collections, the text of the *Tractatus de Sphaera* is the same.
[111] Cf. Vol. II, p. 299.
[112] Cf. Vol. II, pp. 293—294.
[113] Cf. Vol. II, p. 297, et infra, note M.
[114] Vide supra, Vol. II, pp. 293—294.
[115] Vide supra, Vol. II, pp. 305—307.
[116] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. F of the Bibliothèque de l'Institut, folio 22, verso.
[117] *Les Manuscrits de Léonard de Vinci*, published by Ch. Ravaisson-Mollien, Ms. F of the Bibliothèque de l'Institut, folio 62, verso. Cf. *Del moto e misura dell'acqua*, lib. I, capit. XIV, p. 280.
[118] P. Duhem, *Thémon le fils du Juif et Léonard de Vinci* (*Bulletin Italien*, Vol. 6, April and July, 1906.)
[119] T. N.: The Latin reads, Whether the waters of springs and rivers arise from rain water which has collected in the cavities of the earth? — Whether spring water in the cavities of the earth arises from evaporated air?
[120] T. N.: The Latin reads, Whether the waters of springs and rivers arise in the cavities of the earth?

## FOOTNOTES TO NOTE J

[121] *Treatise on the Sphere*, translated from Latin into French by Master Nicolas Oresme, the very learned and renowned philosopher. Sold in Paris in the Rue Judas by Master Simon du Bois, printer. (*In fine*: Printed in Paris by Master Simon du Bois.) — This small volume printed in Gothic type, bears neither a date of publication nor pagination.

## FOOTNOTES TO NOTE K

[122] T. N.: The Latin title reads, *XIV Very Subtle Questions on John of Sacrobosco's Treatise on the Sphere.*
[123] T. N.: The Latin title reads, It is to be asked whether the heaven and the four elements are spherical.
[124] T. N.: The Latin reads, It is to be asked whether the motion of the prime moving body from East to West around the earth is uniform.
[125] T. N.: The Latin reads, Authorities on Weight.

## FOOTNOTES TO NOTE L

[126] *Questiones subtilissime* Alberti de Saxonia *in libros de Caelo et Mundo*, Colophon. Expliciunt questiones ... Impresse autem Venetiis Arte Boneti de Locatellis Bergmonensis, impensa vero nobilis viri Octaviani Scoti Modoetiensis, Anno salutis nostre 1492, nono Kalen. novembris. ducante inclito principe Augustino Barbadico. In librum II quaestio XIIII, *in fine*.

## FOOTNOTES TO NOTE M

[127] We have seen (Vol. II, p. 293) that Marsilius of Inghen is among those sharing this view.
[128] T. N.: The Latin reads, This explanation is to be attributed to Campanus.
[129] T. N.: This title came from Peter of Abano's mediation of a dispute concerning the place of the study of medicine in a university curriculum. His book is a compilation of the disputations on this subject in Paris.
[130] T. N.: The Latin title reads, Conciliator of Philosophers, but principally of Physicians.
[131] John of Jandun was still in Paris in 1324. [Cf. Denifle and Chatelain, *Chartularium Universitatis Parisiensis*: tomus II, sectio prior, p. 303 (en note): Parisiis, 1891.]

## FOOTNOTES TO NOTE N

[132] Cf. Vol. I, pp. 25 and 135.

[133] Cf. Vol. II, p. 278.
[134] We were unable to study any edition of these *Questions* dated prior to 1516. However, in the edition which he published in Paris in the same year, George Lokert declares that they had already been published by the Venetians. Indeed, the edition printed in Venice in 1516 by Boneto Locatelli begins with a dedicatory epistle dated 1504. In it, we learn that this work must have been printed in 1504 as well. Furthermore, according to the *Repertorium bibliographicum* of Hain, it was printed in Padua as early as 1493.
[135] This treatise was printed in Bologna in 1494 by Benedictus Hectoris. Hieronymus Scotus printed it under the title: *De proportione motum quaestio* in the three editions of: *Alexandri Achillini Bononiensis Opera omnia* which he published in Venice in 1545, 1551, and 1568. The edition of the *Opera omnia* published in Venice in 1508 without the name of the printer does not include this treatise.
[136] *Alexandri Achillini Opera omnia*, ed. 1545, fol. 194, col. b.
[137] Id., ibid., loc. cit., fol. 194, col. c.
[138] *Il Codice Atlantico* di Leonardo da Vinci nella Biblioteca Ambrosiana di Milano, riprodotto e pubblicato dalla Regia Accademia dei Lincei; Ulrico Hoepli, Milano, MCDXCIV, fol. 225, recto b (34). Cf. Mario Baratta, *Leonardo da Vinci ed i Problemi della Terra*, Torino, 1903, p. 9. (T. N.: The Italian title reads, *The Codex Atlanticus* of Leonardo da Vinci in the Ambrosiana Library of Milan, reproduced and published by the Regia Accademia dei Lincei; Ulrico Hoepli, Milano, 1494, fol. 225, recto b (34). Mario Baratta, *Leonardo da Vinci and the Problems Concerning the Earth*, Turin, 1903, p. 9.)
[139] T. N.: The Italian reads, The *Proportions* of Achillini with the considerations of [Giovanni] Marliani are given by Mr. Fazio.

## FOOTNOTES TO NOTE O

[140] Vide supra, Vol. I, pp. 122—130.
[141] *La Scientia de ponderibus et Léonard de Vinci* (*Études sur Léonard de Vinci*, Première série, Paris, 1907).

## FOOTNOTES TO NOTE P

[142] Cosmotheoriae liber primus, et elementorum, et caelestium corporum magnitudines, situs, motusque universim aperiens. De omnimoda terrae et maris dispositione, cap I (Joannis Fernelii Ambianatis *Cosmotheoria*, fol. 1). (T. N.: The Latin reads, Book One of the Cosmotheory, which reveals all of the magnitudes, sites and motions of both the elements and the heavenly bodies. On the overall configuration of the earth and the sea, Chapter I (*The Cosmotheory* of Jean Fernel of Amiens, folio 1).)
[143] Joannis Fernelii Ambianatis *Cosmotheoria*, libros duos complexa. Prior, mundi totius et formam et compositionem: ejus subinde partium (quae elementa et caelestia sunt corpora) situs et magnitudines: orbium tandem motus quosvis solerter referat. Posterior ex motibus, siderum loca et passiones disquirit: interspersis documentis haud

paenitendum aditum ad astronomicas tabulas suppeditantibus. Haecque seiunctim tandem expedite praebet Planethodium. Cuique capiti, perbrevia, demonstrationum loco, adjecta sunt scholia. Parisiis, in aedibus Simonis Colini, 1528. (T. N.: The Latin title reads, *The Cosmotheory* of Jean Fernel of Amiens, comprising two books, The first pertains to the shape and composition of the entire Universe, to the sites and magnitudes of its subordinate parts (which are the elements and the hevenly bodies), and finally to the various motions of the orbs. The second investigates based upon these motions, the locations and behavior of the stars, interspersed with documents in support of this respectable approach to astronomical tables, which are presented separately and clearly. In place of demonstrations, brief scholia have been added to each chapter. Paris, at the residence of Simon Colin, 1528.)

[144] Fernel, loc. cit., De aeris ignisque situ, Chap. II. (T. N.: The Latin reads, Fernel, loc. cit., On the Locus of Air and Fire, Chapter II.)

## FOOTNOTES TO NOTE Q

[145] *Doctrinae physicae elementa, sive initia*, Philippo Melanchthone auctore; post omnes alias editiones ex postrema autoris recognitione, cum locuplete rerum et verborum in his memorabilium indice. Lugduni, apud Joan. Thornaesium et Gul. Gazium, MCLII. Quis est motus mundi, p. 60. (T. N.: The Latin title reads, Philip Melanchthon: *The Elements or Foundations of the Theory of Physics*, edited using all the most recent editions of the author and provided with a complete index of the significant subjects and terms in those. Lyons, at the press of John Torna and Wm. Gazeius, 1552. What is the motion of the Universe?, p. 60.) The first edition of this work goes back to 1549.

## FOOTNOTES TO NOTE R

[146] B. Boncompagni, *Intorno ad un testamento inedito de Nicolò Tartaglia*, p. 364 (*Collectanea Mathematica in Memoriam D. Chelini*; 1881). (T. N.: The Italian title reads, On the unpublished will of Nicolo Tartaglia.)

[147] Vincenzo Tonni-Bazza, *Frammenti di nuove ricerche intorno a Nicolò* Tartaglia [*Atti del Congresso Internationale di Scienze Storiche* (Roma, 1—9 Aprile 1903) Roma, 1904. No. XXXIII, p. 293]. (T. N.: The Italian title reads, Excerpts from Recent Research on Nicolo Tartaglia.)

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