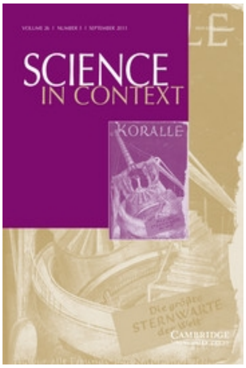

An Account of the Scientific Titles and Works of Pierre Duhem

Pierre Duhem

Science in Context / Volume 1 / Issue 02 / September 1987, pp 333 - 348
DOI: 10.1017/S0269889700000405, Published online: 26 September 2008

Link to this article: http://journals.cambridge.org/abstract_S0269889700000405

How to cite this article:
Pierre Duhem (1987). An Account of the Scientific Titles and Works of Pierre Duhem. Science in Context, 1, pp 333-348 doi:10.1017/S0269889700000405

Science in Context 1, 2 (1987), pp. 333–348

PIERRE DUHEM

An Account of the Scientific Titles and Works of Pierre Duhem

Written by the Author Himself at the Time of His Candidacy for the Academy of Sciences (May 1913)

> Certain authors, in speaking of their works, say: My book, my commentary, my history, etc. They smack of these bourgeois homeowners, with "my house" always on their lips. They should rather speak of: our book, our commentary, our history, etc., since, generally speaking, there is far more in them of others than of their own. [108]
>
> Pascal, *Pensées*

We shall not attempt to summarize, however succinctly, the all too numerous publications that have just been enumerated.* We shall content ourselves with arranging them under several main heads; we shall then run a conducting wire from one group to another. The wire will catch against all significant projections, while clearing all areas of lesser relief without touching them. Perhaps in this way we shall be able to put some order and unity into this confused multiplicity. In pruning all these fragments one by one, we have never lost sight of the idea of a simple and harmonious doctrine, which was what our clumsy mosaic was attempting to achieve. We would like to give at least some outline of that idea which has guided our efforts over the past thirty years.

Translated from "Notice sur les titres et travaux scientifiques de Pierre Duhem," *Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux*, 7^{e} Série, tome I (Paris et Bordeaux 1917), pp. 71–169. Translated by Y. Murciano and L. Schramm, revised by Pierre Kerszberg, The University of Sydney, Australia. The first part, "Recherches de Physique théorique" (pp. 72–150) is omitted here. Numbers in the margins refer to the pagination of the French original. Duhem's quotations from his own oeuvre correspond (with minor changes) to passages of the second edition (1914) of his *La théorie physique, son objet, sa structure*, which was translated into English by Philip P. Wiener as *The Aim and Structure of Physical Theory*. The references to the second French edition and to the English edition of 1962 are given in the footnotes. (We have not always adopted Wieners translation.)

*In the original a full bibliography precedes Duhem's text. We have quoted at the end of the paper only those publications to which Duhem refers in the parts of the text published here.

[151] Part II. Logical Examination of Physical Theory

One could treat theoretical Physics in the manner of the Cartesians or the Atomists. Bodies perceived by the senses and by instruments are resolved into an enormous number of much smaller bodies conceived by reason alone; observed motions are seen as resulting from the effects of imperceptible motions of these small bodies; these bodies are assigned a small number of well-defined shapes; very simple and entirely general rules are formulated about their motions. These bodies, these motions, are, properly speaking , the only real bodies and the only true motions. When, through combining them in a suitable manner, we see that they are capable of producing a group of effects similar to the observed phenomena, we say that we have discovered the explanation of these phenomena.

[152] Our Energetics does not proceed in this way. The principles it formulates, and their attendant consequences, in no way attempt to resolve the bodies that we perceive, the motions that we observe, into imperceptible bodies or into hidden motions. They in no way claim to be revelations concerning the true nature of matter. They do not presume to explain anything. They simply profess to be general rules of which the laws observed by the experimenter are particular cases.

One could view theoretical Physics as the Newtonians do. One could reject all hypotheses about imperceptible bodies and hidden motions that could form the bodies and the motions perceptible by the senses and by instruments. The only admissible principles would be very general laws, arrived at by induction from the observation of facts.

Our Energetics does not follow the Newtonian method. True, it admits an experimental origin to the principles it formulates, inasmuch as they are suggested by observation and seek several times the advice of experience for the modification of their statements. However, these experiences, although explaining the historical origin of the principles, do not bestow any certitude on these principles. The principles are laid down as pure postulates, arbitrary decrees of human reason; they are considered to have successfully fulfilled their role when they yield numerous consequences that conform to experimental laws. Compliance with the teachings of observation is therefore not required, as the Newtonian method would have it, at the outset of physical theory, but rather at its end.

When Energetics equally refuses to follow the method of the Cartesians, of the Atomists, and of the Newtonians, is it acting wisely? Does a close examination of the means of knowledge available to Physics justify the course it takes? To this question we have replied: yes.

Our objection to the Cartesians and the Atomists is that their method is not autonomous (1892, 1906b); the physicist wishing to follow their method cannot rely exclusively on methods characteristic of Physics. By claiming that beyond sensible bodies and observable motions, which according to him are mere appearances, are other bodies and other motions that alone are real, he is led to the domain of Cosmology; he no longer has the right to turn a deaf ear to the teachings of

Metaphysics regarding the true nature of matter. Thus his Physics comes under the domination of metaphysical Cosmology and all is subject to the uncertainties and fluctuations of this doctrine. That is why the theories constructed in accordance with the Cartesian and atomistic method suffer from infinite multiplicity as well as perpetual reformulation; they are incapable of offering Science a general consensus or continuous progress.

As to the Newtonian method, our objection is that it is unworkable (1894, 1906b).

Any science may progress according to the Newtonian method as long as its means [153] of knowledge are those of common sense. Induction can no longer be practiced in the way this method supposes when science no longer observes facts directly, but rather through measurements (provided by instruments) of magnitudes defined only by mathematical theory.

> An experiment in Physics is not merely observing a phenomenon.... An experiment in Physics is the precise observation of a group of phenomena, accompanied by the *interpretation* of these phenomena; this interpretation replaces concrete data, actually gleaned through observation, with abstract and symbolic representations that correspond to the data by virtue of the physical theories accepted by the observer.[1]

From this truth we can deduce numerous consequences strongly opposite to the idea of a science where each principle is furnished by induction:

> The physicist can never submit an isolated hypothesis to the control of experimentation, but rather a whole group of hypotheses; when the experiment does not agree with its predictions, at least one of the hypotheses of this group is erroneous and must be modified; but the experiment does not tell which hypothesis requires modification. We are hence far removed from the mechanism of experimentation readily imagined by people ignorant of its workings. It is commonly believed that each of the hypotheses used by the Physicist can be taken in isolation, submitted to the control of experimentation, and then, backed up by various and numerous proofs, it can take up a more or less definitive place within the totality of science. This is not the case, however. Physics is not a machine that can be taken apart; one cannot test out each part in isolation and wait until its solidity has been checked in minute detail before reassembling it. Physical science is an organism that must be taken as a whole. When one part is put to work, the most remote parts are also affected, to a greater or lesser degree. If there is some failure, some operational fault, the physicist is obliged to guess which organ must be rectified or modified, without being able to isolate this organ and examine it by itself. The watchmaker, [154] when repairing a watch that does not run, takes apart the entire works and examines the wheels one by one, until he finds the faulty or broken part. But the physician trying to cure a patient cannot dissect him in order to make a diagnosis;

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[1] Cf. Duhem 1914, 221–22; 1962, 147.

> he must guess at the source of the illness solely by examining the effects of the illness on the body as a whole. The physicist, in attempting to rectify a lame theory, resembles the physician and not the watchmaker.[2]

Physical theory is not an explanation of the inorganic world; nor is it an inductive generalization of what may be learned empirically. What then is it? (1893b, 1906b, 1908d, 1908a.)

Is a theory, as the Pragmatist School would have it, simply a device that makes it easier to manipulate the truths of empirical knowledge, that allows us to apply these truths more promptly and more efficiently in our action on the external world, but does not teach us anything about the world that experience itself has not already taught us?

Or, on the contrary, does a theory teach us something about reality that experience has not taught us and cannot teach us, something that transcends purely empirical knowledge?

If the answer to the latter question is yes, we could say that a physical theory is true, that it has value with regard to *knowledge*. If, on the contrary, the answer to the former question is yes, we would be forced to admit that a physical theory is not *true*, but simply *convenient*, that it has no value, with regard to knowledge but simply a *practical value*.

> When the physicist, turning his attention to the science he is constructing, submits the various procedures he has set in motion in order to construct it to a rigorous examination, he discovers nothing that can introduce the least bit of truth into the structure of his edifice other than empirical observation. The statements *this is true* and *this is false* can be applied only to propositions that claim to state empirical facts, and to no other. Only of such propositions can one say with any certainty that they do not allow illogicality, and that of two contradictory propositions at least one must be discarded. As to propositions introduced by the theory, they are neither *true* nor *false* but simply *convenient* or *inconvenient*. If the [155] physicist considers it convenient to build a pair of chapters of Physics by means of contradictory hypotheses, he is free to do so. The principle of contradiction can categorically adjudicate between truth and falsehood, but it has no power to decide what is *useful* or *useless*. Therefore, to require that physical theory preserve a rigorous logical unity in its development would be to exercise an unjust and insupportable tyranny over the physicist's intelligence.
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> When, after having submitted the science he is working on to this careful examination, the physicist reverts to himself and becomes aware of the tendencies that guide the processes of his reason, he immediately recognizes that his most powerful and deepest aspirations are frustrated by the hopeless conclusions of his analysis. No, he cannot resign himself to viewing physical theory as merely a group

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[2] Cf. Duhem 1914, 284–85; 1962, 187–88

> of practical procedures, a rack full of tools. No, he cannot believe that it merely classifies the knowledge accumulated by empirical science, without in any way transforming the nature of this knowledge, without imprinting on it a character that experience alone cannot provide. If physical theory contained only findings that its own self-critique revealed within it, he would cease devoting his time and effort to a work of such puny importance. *The study of the physical method cannot reveal to the physicist the reason that induces him to construct the physical theory.*
>
> No physicist, however much of a positivist, could deny this statement. However, he would have to be an extreme positivist not to go beyond that statement, not to affirm that his strivings towards an increasingly unified and comprehensive physical theory are reasonable, even though the critique of physical method has not been able to discover the reason. He will find it very difficult not to posit this reason in the correctness of the following propositions:
>
> Physical theory provides us with a certain knowledge of the external world, which cannot be reduced to purely empirical knowledge; this knowledge does not follow from experience, nor from the mathematical procedures used by the theory, so that a purely logical analysis of the theory would not reveal the gap through which it has entered into the body of Physics. This knowledge derives [156 from a truth other than those truths available to our instruments, through a path the reality of which the physicist could no more deny than he could describe its course. The order in which theory classifies the results of observation cannot be entirely or fully justified by their practical or aesthetic properties. We suppose, moreover, that it is, or tends to be, a *natural classification*. Through an analogy the nature of which lies beyond the grasp of Physics, but the existence of which imposes itself as certain on the physicist's mind, we suppose that it corresponds ever better to a certain transcendent order.
>
> In a word, the physicist must admit that *it would be unreasonable to work towards the advancement of physical theory unless that theory were the increasingly clear and precise reflection of metaphysics. Belief in an order that transcends Physics is the only raison d'être of physical theory.*
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> The attitude, whether hostile or favorable, that each physicist adopts with regard to this statement, can be summed up in Pascal's words: "We have an impotence to prove, that no Dogmatism can overcome. We have an idea of truth, which no Scepticism can overcome" (1908a).[3]

Differing from the various Pragmatist schools with regard to the value of physical theory, we do not under any circumstances count ourselves among their disciples. The analysis we have given of experiments in Physics, through which we showed how any fact is theory-laden to the point that it cannot be stated in isolation from the theory, this analysis, we repeat, has found great favor with several Pragmatists. They have applied it to the most diverse areas, to History, Exegesis, Theology. We do not deny that this extension is legitimate *up to a certain point*. However diverse the

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[3] Cf. Duhem 1914, 507–9; 1962, 333–35.

problems, it is always the same human reason that attempts to resolve them, so that there is always something common in all the various procedures it uses. But while it is good to pay attention to analogies among the various scientific methods it must not lead us to disregard the differences that separate them. And when we compare the method of Physics – so strangely specialized by its recourse to mathematical theory and its use of measuring instruments – to other methods, we shall surely discover more differences than analogies.

[157] We admit that physical theory may attain a certain knowledge of the nature of things. But we consider this knowledge – of a purely analogic nature – to be the goal of the theory's progress, the limit it constantly approaches without ever reaching it. The Cartesian and Atomist schools, on the other hand, place this hypothetical knowledge of the nature of things at the starting point of physical theory. If we have parted company with the Pragmatists, it is surely not to join ranks with the Cartesians or Atomists.

The neo-Atomist school, whose doctrines revolve around the concept of the electron, has taken up, with superb confidence, the method we refuse to adopt. This school believes that its hypotheses have at last penetrated to the intimate structure of matter, enabling us to see the elements as if some extraordinary super-microscope had enlarged them so as to become perceivable.

We cannot share this confidence. We cannot recognize in these hypotheses an oracular view of what is beyond sensible things; we simply regard them as models. We have never denied the utility of these models, dear to the physicists of the English School (1893a, 1906b). They are, we believe, an indispensable aid to these minds that are more broad than deep, more suited to imagining what is concrete than conceiving the abstract. But the time will probably come when, because of their increasing complexity, these representations, these models will cease serving a useful function to physicists, but will be considered rather as hindrances and encumbrances. Abandoning then these hypothetical mechanisms, the physicist will carefully bring out the experimental laws they have helped discover. Without claiming to explain these laws, he will seek to classify them according to the method we have just analyzed, to understand them within a modified and more broadly based Energetics.

[158] Part III. Studies in the History of Physical Theories

Any abstract thought must be checked by facts; any scientific theory calls for comparison against experience. Our logical appraisals of the method of Physics can be soundly assessed only when measured against the lessons of History. We will now apply ourselves to these lessons.

In Antiquity, the Middle Ages, and the Renaissance, there was only one branch of Physics where mathematical theory was sufficiently developed and observation sufficiently accurate for a debate over their mutual relationship to be relevant. We are referring to Astronomy.

Concerning the nature and value of astronomical theory (1908b), the admirably flexible, penetrating, and varied Hellenic genius conceived, one might say, of all those systems that have resurfaced in our times. Among these systems, there is one that meets with general acceptance on the part of the most profound thinkers. It can be summed up in the principle that Plato taught to would-be students of Astronomy. "One must, by starting from certain assumptions, be able to save what appears to the senses" (*Tinon hypotethenton, ... sozein ta phainomena*). This principle came through the Arab, Jewish, and Christian Middle Ages, was repeated during the Renaissance, and was explained, defined, or challenged, until the day Andreas Osiander formulated it as follows in his preface to Copernicus's book: "*Neque enim necesse est eas hypotheses esse veras, imo, ne verisimiles quidem, sed sufficit hoc unum si calculum observationibus congruentem exhibeant.*" (For these hypotheses need not [159] be true, nor even probable. On the contrary, if they provide a calculus consistent with the observations, that alone is enough).[4] Thus, for two thousand years most thinkers who reflected on the nature and value of mathematical theory used by physicists, came to adopt this axiom, which was subsequently taken over by Energetics, namely, the first postulates of physical theory are not intended to be statements about certain suprasensible realities; they are general rules, which will have properly played their part if the specific consequences deduced from them agree with observed phenomena.

The method followed by Energetics is not an innovation. It can be traced back to the most ancient, continuous, and noble tradition. But what about the essential concepts and fundamental principles of this science? In defining these concepts, or laying down these principles, Logic demands of it no justification. Logic allows it to lay its foundations as it sees fit, provided that, when brought to completion, the structure can easily and neatly accommodate the laws ascertained by the experimenter. Does this mean that Energetics will define its concepts at random and lay down its principles without reason? Not at all. If Logic imposes no constraints, the lessons of History provide a reliable and rigorous guide. The memory of past attempts, with their more or less felicitous outcome, excludes hypotheses that have undermined more ancient theories, in favor of ideas that have already proved fruitful. Energetics cannot prove its postulates, nor is it required to do so. However, by retracing the vicissitudes through which they have passed before assuming their current form, it can win our confidence in them, bring us to trust them until such time as their consequences receive the empirical confirmation we have counted on.

Our aim in undertaking to write the history of the major laws of Statics and Dynamics was to provide Energetics with the knowledge of the evolution of each of its own fundamental principles and with the ability to present it.

We knew that important reflections on Statics were scribbled in Leonardo da Vinci's manuscript notes. A perusal of Leonardo da Vinci and Cardan drew our attention to the unexplored Statics of the Middle Ages. Soon after, an analysis of all [160]

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[4] Nicholas Copernicus, *On the Revolutions*, trans. Edward Rosen, London: Macmillan, 1978, p. xvi.

the manuscripts on Statics to be found in the public libraries of Paris provided us with a wealth of unexpected finds (1905/6, vol. I). The Christian Middle Ages were acquainted with Greek works on Statics; some of these had reached them directly, and others through Arab commentaries. The Latins who read these works were not at all uncreative and servile commentators as we are led to believe. The remnants of Greek thought, which reached them via Byzantium or Muslim Science, far from stagnating in their minds, aroused their attention and fertilized their intelligence. And since the thirteenth century, perhaps even before then, the School of Jordanus opened up to the Mechanists new paths unknown to the ancients.

The intuitions of Jordanus of Nemore are at first rather vague and uncertain; grave errors keep company with great truths. But the disciples of the great inventor soon distilled the master's thought. Errors were weeded out and disappeared, truths defined and reinforced, and several of the most important laws of Statics were finally established with total certainty.

In particular, we owe the school of Jordanus a principle whose importance would become increasingly evident during the development of Statics. Without analogy to the postulates concerning the lever which were vindicated by Archimedes' deductions, this principle has no more than a remote affinity with the inexact axiom put forward in Aristotle's Mechanical Questions. It states that the same motive power can raise different weights to different heights, provided that the heights are in inverse proportion to the weights. Applied by Jordanus exclusively to the straight lever, this principle was extended by one of his disciples, who formulated the law of the equilibrium of weights on an inclined plane, and, through an admirable geometric device, the law of equilibrium of a bent lever.

The writings of this anonymous thirteenth-century mathematician were taken up by Descartes almost unchanged. From that point on, from Descartes to Wallis, from Wallis to Jean Bernoulli, from Jean Bernoulli to Lagrange, and then to Gibbs, the principle of virtual displacement spread incessantly.

[161] Round about 1360, a Master of Arts of the University of Paris, Albert of Saxony, wrote: "Each part of a heavy body does not strive to make its center the center of the world, which would be impossible. It is the body as a whole which descends in such a manner that its center becomes the center of the world, and all its parts strive towards this end – that the center of the body as a whole become the center of the world. They do not, thus, hinder one another. . . ." This center, this point in any heavy body which tends to seek the center of the world, is, as Albert repeats several times, the center of gravity.

Thus each heavy body moves as if its center of gravity were seeking out the center of the world. This erroneous idea, which, during the seventeenth century, gave rise to many an error, held the greatest geometricians in its grasp and gave way only after a fierce debate (1905/6, vol. II). In the interim, however, it was a fruitful idea, which was to provide Statics with new truths. In effect, it immediately provided it with this proposition: A heavy system is in equilibrium when its center of gravity is as low as possible. This proposition was taken up by Torricelli and Pascal as the foundation of all Statics, until it gave birth to the theorem of Lagrange and Lejeune-Dirichlet on the stability of equilibrium.

Leonardo da Vinci, an indefatigable reader, used to peruse and ponder incessantly the writings of the School of Jordanus, on the one hand, and the Scholastic questions of Albert of Saxony on the other. The former, by acquainting him with the law of equilibrium of the bent lever, led him to the memorable law governing the composition of concurrent forces: Through a point taken on one of the components, or on the resultant, the other two forces have equal moments (1904; 1905/6, vol. I; 1906a). On the other hand, Albert of Saxony's ideas on the role of the center of gravity led to da Vinci's discovery of the law of the subtended polygon (1905/6, vol. II; 1906a) which was plagiarized by Villalpand (1905a; 1905/6, vol. II). Thus we find in the writings of the thirteenth and fourteenth centuries the origins of several basic principles of Statics.

Can the same be said for Dynamics?

The Dynamics inaugurated by Galileo, his rivals, his disciples, Baliani, Torricelli, Descartes, Beeckmann, and Gassendi, is not a creation ex nihilo. Modern intelligence did not produce it at first attempt and out of nothing, as soon as its reading of Archimedes had taught it the art of applying geometry to natural effects. [162]

The mathematical skill acquired from reading the ancient geometers was used by Galileo and his contemporaries to refine and develop a mechanical science whose most essential principles and propositions had been formulated during the Christian Middle Ages. The physicists who taught at the University of Paris in the fourteenth century had conceived this mechanics with observation as their guide. They had substituted it for Aristotle's Dynamics, convinced of the latter's failure to "save the phenomena." During the Renaissance, this doctrine of the "Moderns" was rejected by a superstitious archaism, in which the wit of the Humanists and the Averroist routine of retrograde Scholasticism rubbed shoulders. The reaction against the Dynamics of the "Parisians" and in favor of the inadmissible Dynamics of the Stagirite was strong, particularly in Italy (1909/10). But despite this obstinate resistance, the Parisian tradition found, both outside the schools as well as within the universities, masters and scholars to uphold and develop it. This Parisian tradition became the heritage of Galileo and his emulators. When we see the science of a Galileo triumph over the stubborn Peripateticism of a Cremonini, we think, uninformed as we are of the history of human thought, that we witness the victory of the young modern Science over medieval philosophy, obstinate in its thoughtless parroting; whereas in fact we are contemplating the long-prepared triumph of the science born in fourteenth-century Paris over the doctrines of Aristotle and Averroes, restored to honor by the Italian Renaissance.

No motion can persist if it is not maintained by the continuous action of a motive force, directly and immediately applied to the moving object. This is the axiom on which all of Aristotelian Dynamics rests.

According to this principle, the Stagirite wanted to attribute a motive force to an arrow that continues to fly after having left the bow. He thought he had found this force to be the air that has been disturbed. It is the air, struck by the hand or by the ballistic machine, that supports and carries the projectile.

This hypothesis, which to us seems to be stretching improbability to the point of [163] ridicule, seems to have been almost unanimously accepted by the physicists of Antiquity (1907/8). It was unequivocally denied by only one of them, who, although he lived during the last years of Greek philosophy, was practically separated from it by his Christian faith. We are referring to John of Alexandria, known as Philoponus. After having shown the inadmissible character of the Peripatetic theory of the motion of projectiles, John Philoponus declared that the arrow continues to move with no motor being applied to it, because the string of the bow supplied an *energy* that acts as a moving power.

The last thinkers of Greece and the Arab philosophers did not so much as mention John the Christian's doctrine – a doctrine that earned the sarcasm of a Simplicius or an Averroes. The Christian Middle Ages, in their naive admiration of Peripatetic science once it was revealed to them, initially shared the disdain of the Greek and Arab commentators towards Philoponus's hypothesis. Saint Thomas Aquinas mentions it only to warn off those who might be seduced by it.

However, following the condemnation in 1277 by the Bishop of Paris, Etienne Tempier, of a host of theories held by "Aristotle and his retinue," a large movement grew up which was to liberate Christian thought from the yoke of Peripateticism and Neoplatonism, and produce what the archaism of the Renaissance was to call the science of the "Moderns."

William of Ockham (1907/8), with his customary verve, attacked the theory of projectile motion proposed by Aristotle, but offered no constructive alternative. However, his criticisms restored John Philoponus's doctrine to favor among certain disciples of Duns Scotus. *Energy,* the motive power of which Philoponus had spoken, reappears under the name of *impetus.* This hypothesis of an *impetus,* impressed on the projectile by the hand or the machine that has launched it, was seized upon by a secular master in the Faculty of Arts of Paris, a physicist of genius (1909a); Jean Buridan adopted this hypothesis, towards the middle of the fourteenth century, as the foundation of a Dynamics with which "all phenomena accord."

The role played by *impetus* in Buridan's Dynamics is exactly the same as Galileo was later to attribute to *impeto* or *momento,* Descartes to the *quantity of motion,* and finally Leibniz to the *vis viva.* This correspondence is so exact that Torricelli, expounding Galileo's Dynamics in his *Lezioni accademiche,* often adopts the reasoning and almost the very words of Buridan. This *impetus* would remain unchanged within the projectile, were it not constantly destroyed by the resistance of the medium and by the opposing action of gravity. Buridan takes this *impetus* to be, at constant velocity, proportional to the *quantity of primary matter* that the body contains. He conceives of and describes this quantity in terms almost identical to those used by Newton to define mass. For a given mass, the greater the velocity, the greater is the *impetus.* Buridan carefully refrained from determining more precisely the relationship between the magnitude of the *impetus* and that of the velocity. More daring, Galileo and Descartes admitted that this relationship could be reduced to proportionality, thereby obtaining an erroneous measure for *impeto* or *quantity of motion,* which Leibniz later had to correct.

Like resistance exerted by the medium, gravity constantly attenuates and finally cancels out the *impetus* of a moving body thrown upwards, since such motion is contrary to the natural tendency of gravity. However, when a moving body falls, the motion accords with the tendency of gravity; in such a case, the *impetus* and the velocity must constantly increase. This, according to Buridan, explains the acceleration we observe when a heavy body falls. This acceleration, although already known to Aristotelian science, had been accounted for in unacceptable terms by Greek, Arab, and Christian commentators of the Stagirite.

The Dynamics discovered by Jean Buridan expresses, in purely qualitative albeit always correct terms, what our concepts of *vis viva* and work formulate in quantitative terms.

The philosopher of Bethune was not the only one to uphold this Dynamics. His most brilliant disciples, Albert of Saxony and Nicole Oresme, adopted and taught it. Oresme's French works spread the theory even among non-scholars (1910–12).

When no resisting medium or natural tendency analogous to gravity opposes motion, the *impetus* retains a constant intensity. The moving body to which a motion such as translation or rotation has been imparted will continue this motion indefinitely at constant velocity. It is in this form that Buridan conceived of the law of [165]
inertia; and it is in this form that it was accepted by Galileo.

Buridan derived a corollary from this law of inertia, whose originality demands our admiration (1907/8).

If the celestial orbs move eternally with a constant velocity, this is because, according to the axiom of Aristotelian Dynamics, each of them is subject to an eternal and immutable power. The Stagirite's philosophy required that such a power be an intelligence separated from matter. The study of the moving intelligences of the celestial orbs not only constitutes the culmination of Peripatetic Metaphysics, but is also the main doctrine around which revolve all the Neoplatonic Metaphysics of the Greeks and the Arabs; and the Scholastics of the thirteenth century did not hesitate to incorporate this heritage of the pagan theologies into their Christian systems.

Buridan had the audacity to write the following lines:

> Since the creation of the world, God has moved the heavens with motions identical to those with which they currently move. He impressed upon them then the *impetuses* by means of which they continue to be moved uniformly. These *impetuses,* in effect, meet with no resistance, which would be contrary to them, and are therefore never destroyed or attenuated.... According to this construction, one can dispense with the existence of intelligences that move the celestial bodies in an appropriate manner.

Buridan stated this idea in various contexts. Albert of Saxony expounded it in his turn (1907/8); and Nicole Oresme used this comparison to formulate it: "Except for the violence, this is quite analogous to when a man has made a clock, and then leaves it to run and be moved by itself."

If one wished to draw a sharp dividing line between the era of ancient Science and that of modern Science, it should be placed, we believe, at the moment when Jean Buridan conceived this theory, at the moment when the heavenly bodies were no longer seen as being moved by divine beings, and when it was admitted that celestial and sublunary motion depended on the same Mechanics.

Here is that Mechanics, both celestial and terrestrial, which Newton was to [166] formulate in the manner we so admire today, attempting to constitute itself since the fourteenth century. Throughout that century, as the writings of François de Meyronnes (1913) and Albert of Saxony (1909b) testify, there were physicists who held that an astronomical system composed of a moving Earth and an immobile heaven of fixed stars was more satisfactory than a system in which the Earth was immobile. Among these physicists, Nicole Oresme developed the reasons (1909b) with a completeness, clarity, and precision that Copernicus himself fell far short of. Nicole Oresme attributed a natural *impetus* to the Earth, similar to that which Buridan attributed to the celestial orbs. In order to account for the vertical fall of heavy bodies, he accepted that one must compound the *impetus* by means of which the moving body turns around the Earth with the *impetus* generated by gravity. The principle so clearly formulated by him, was only vaguely indicated by Copernicus, and merely repeated by Giordano Bruno (1909/10); Galileo used Geometry to draw consequences from it, but without correcting the erroneous form of the law of inertia that it implied.

While Dynamics was being founded, the laws governing the fall of weights were being discovered.

In 1368, Albert of Saxony (1908c, 1910–12) proposed the following two hypotheses: The velocity in fall is proportional to the time elapsed since descent began; The velocity of descent is proportional to the path traversed. He does not make a choice between these two laws. The theologian Pierre Tataret, who taught at Paris toward the end of the fifteenth century, reproduced the laws formulated by Albert of Saxony word for word. Leonardo da Vinci, a great reader of Albert of Saxony, after having accepted the second of the two hypotheses, subsequently favored the first. However, he was unable to discover the law of distances traversed by a falling object. Following a line of argument later adopted by Baliani, he concluded that the distances traversed in equal and successive times are like the series of integers, whereas they are, in fact, like the series of odd numbers.

Nevertheless, the rule by means of which one could compute the distance traversed in a certain time by an object moving in a uniformly changing motion had long been known. Whether this rule was discovered in Paris, during Jean Buridan's time, or at Oxford, during Swineshead's time (1910–12, 1912/13), it was clearly formulated in Nicole Oresme's work in which he formulates the essential principles of Analytic

Geometry. Moreover, the proof he used to justify the rule is identical to that later [167] provided by Galileo.

From the time of Nicole Oresme to that of Leonardo da Vinci, this rule was never forgotten. It was formulated in most of the treatises produced by the thorny dialectic of Oxford, discussed in various commentaries on these treatises written in Italy during the fifteenth century, and in various works of Physics composed at the start of the sixteenth century by the Parisian Scholastics.

None of the treatises mentioned above, however, attempts to apply this rule to a falling body. We find this idea for the first time (1910–12) in the *Questions on Aristotelian Physics,* published in 1545 by Domingo Soto. Student of the Parisian Scholastics, whose visitor he had been and most of whose theories on Physics he adopted, the Spanish Dominican Soto granted that the fall of a heavy body is uniformly accelerated, and that the vertical rise of a projectile is uniformly retarded. To calculate the space traversed for each of these two motions, he correctly applied the rule formulated by Oresme. In other words, he was familiar with the laws of a falling body, the discovery of which is attributed to Galileo. Moreover, he did not claim to have invented these laws, but rather presented them as common truths. These must have been endorsed by the masters whose lessons Soto had followed in Paris. In conclusion, we see that from William of Ockham to Domingo Soto the physicists of the Paris school laid down all the foundations of Mechanics later to be developed by Galileo, his contemporaries, and his disciples.

Among those who received the tradition of Parisian Scholasticism prior to Galileo, no one deserves more attention than Leonardo da Vinci. During the period in which he lived, Italy firmly resisted the infiltration of the Mechanics of the *moderni,* of the *juniores.* Among the masters at universities, even those who leaned toward the terminalist doctrines of Paris confined themselves to reproducing, in an abridged and somewhat hesitant manner, the basic tenets of this Mechanics, without in any way using them creatively.

Leonardo de Vinci, on the other hand, was not content with merely accepting the general principles of the Dynamics of *impetus.* He mulled over and examined them [168] from all points of view, forcing them, as it were, to reveal the implicit consequences they concealed within them (1909/10). The basic hypothesis of this Dynamics was a rudimentary form of the *vis viva* law. Leonardo perceives in it the idea of the conservation of energy – an idea which he expressed with a prophetic clarity (1907/8). Albert of Saxony had left his readers in suspense between two laws concerning the fall of bodies, one true, and the other unacceptable. After several false starts, which was Galileo's experience too, Leonardo could choose the correct law. He extended this law succesfully to the fall of a body along an inclined plane (1912/13). Through a study of compounded *impeto,* he was the first to attempt to explain the curvilinear trajectory of projectiles, an explanation that would subsequently be completed by Galileo and Torricelli (1907/8). He had an inkling of how to correct Buridan's law of inertia, and prepared the groundwork for Benedetti and Descartes (1909/10).

No doubt, Leonardo did not always recognize the riches stored up by Parisian Scholasticism. He ignored some that would have most felicitously complemented his doctrine of Mechanics. He misunderstood (1909/10) the role of *impetus* in explaining the accelerated fall of heavy bodies. He ignores the rule for calculating the distances traversed by a body in uniformly accelerated motion. It is none the less true that the corpus of his work on Physics places him in the ranks of those whom his Italian contemporaries called the Parisians.

This title is well-deserved. The principles of his Physics in fact stem from an assiduous reading of the works of Albert of Saxony, and probably also from cogitations on the writings of Nicholas of Cusa (1907/8). Since Nicholas of Cusa was also an adept of the Parisian school of Mechanics, Leonardo can rightly be ranked among the Parisian precursors of Galileo.

We have just traced in outline the essential laws of equilibrium and motion, in their infancy. We have at times been able to clarify the history of certain aspects of Physics at the time when this science was reaching its adolescence. Thus we have researched the sources of Pascal's hydrostatic theories (1905b), discussed P. Mersenne's role in the discovery of the weight of air (1906c, 1906d), and sketched the origins of the [169] doctrine of universal attraction (1906b). Note that in no case have essential principles proceeded from a desire to resolve visible and tangible bodies into imperceptible, albeit simpler, bodies, nor has their aim been to explain sensible motion in terms of hidden motion. Atomism has played no part in the formation of these principles. They all arise from the need to formulate some very general rules, the consequences of which will "save the phenomena." Thus the history of the development of Physics confirms what we have already learned through a logical analysis of the procedures used by this science. From both we have derived confidence in the future fruitfulness of the method of Energetics.

Bordeaux, 3 May 1913

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