ROGER ARIEW AND PETER BARKER

INTRODUCTION

Pierre Duhem (1861–1916) was a French physicist who wrote extensively on the history and philosophy of science. From a contemporary perspective, the attractive and unusual feature of Duhem's thought is its combination of original historical research and philosophical analysis. His most important works in history and philosophy of science are: *La théorie physique, son objet et sa structure* (Paris, 1906); *Sozein ta phainomena: Essai sur la notion de théorie physique* (Paris, 1908); *Etudes sur Léonard de Vinci*, 3 vols., (Paris, 1906–13); and *Le Système du Monde*, 10 vols., (Paris, 1913–59).

Duhem's philosophical works had an immediate influence. They were discussed by the founders of twentieth-century philosophy of science, including Ernst Mach, Henri Poincaré, the members of the Vienna Circle, and Karl Popper. A second wave of interest in Duhem's philosophy began with W. V. O. Quine's reference (Quine 1953) to Duhem's thesis that experimental evidence alone cannot conclusively falsify hypotheses. (This and related theses are referred to in the literature as the Duhem–Quine thesis.) As a result, Duhem's predominantly philosophical works were translated into English – as *The Aim and Structure of Physical Theory* and *To Save the Phenomena* (Duhem 1954 and 1969). Moreover, the Librairie Philosophique J. Vrin, a leading French publisher, reissued both volumes in 1981–82.

By contrast, few of Duhem's far more extensive historical works have been translated, with five volumes of the *Système du Monde* actually remaining in manuscript form until 1954–59. Lately, two volumes of Duhem's predominantly historical work have appeared in translation – as *The Evolution of Mechanics* and *Medieval Cosmology* (Duhem 1980 and 1985).

There has been a recent resurgence of interest in Duhem's history and philosophy of science, as evidenced by the publication of numerous articles and several books dealing with Duhem, for example, those of Stanley Jaki (1984), Roberto Maiocchi (1985), and Niall Martin (forthcoming).

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The current interest in Duhem's work can be attributed to a change of climate in the history and philosophy of science. While the dominant methodology in philosophy of science was logical analysis, discussion of the Duhem–Quine thesis strayed further and further from any real contact with Duhem. The decline of logic-based philosophy of science, and the emergence of new historical approaches, has reopened many issues addressed by Duhem at the turn of the century: the relation between history of science and philosophy of science, the nature of conceptual change, the historical structure of scientific knowledge, and the relation between science and religion.

In his historical studies, Duhem argued that there were no abrupt discontinuities between medieval and early modern science (the so-called continuity thesis); that religion played a positive role in the development of science in the Latin west; and that the history of physics could be seen as a cumulative whole, defining the direction in which progress could be expected. Although Duhem's coverage of primary sources in the medieval and early modern periods is rivaled perhaps only by Thorndike's *History of Magic and Experimental Science*, his work has not been effectively incorporated into the continuing dialogue. There are several reasons for this. Unlike his philosophical work, Duhem's historical work was not sympathetically received by influential contemporaries, notably George Sarton. His supposed main conclusions were rejected by the next generation of historians of science who presented modern science as discontinuous with the science of the middle ages. This view was echoed by historically-oriented philosophers of science who, from the early 1960s, emphasized discontinuities as a recurrent feature of historical change in science (Thomas Kuhn in *The Structure of Scientific Revolutions*, for example). However, the rejection of Duhem's conclusions occurred before the majority of his historical works were fully published or translated.

We feel the time is ripe for a reevaluation of Duhem's positions in the history and philosophy of science. Recently philosophers have begun to show a genuine interest in historical work. Duhem's historical corpus is now available in its entirely, and significant portions of it have been translated. New commentaries are being written on Duhem's thought, but that work is still isolated and uncoordinated. Historians and philosophers alike are beginning to reject the picture of science as an activity lurching from one scientific revolution to another, especially for the period of the Copernican revolution, the chief focus of Duhem's work.

The relations between science and religion are again a matter of active scholarly interest. In all of the areas Duhem may be seen as a potential contributor to current debates.

In March 1989, we held a conference entitled "Pierre Duhem: Historian and Philosopher of Science" at Virginia Polytechnic Institute and State University, as a way of bringing together historians, philosophers, and others with an active interest in the range of issues sketched above. Before the conference we circulated our translations of two essays: 'Logical Examination of Physical Theory', and 'Research on the History of Physical Theories', corresponding to the second and third parts of Duhem's summary of his own work supporting his candidacy for the Académie des Sciences. These are reproduced at the beginning of the present issue. The balance of the present work consists of an edited selection from the papers presented at the conference. We regret that limitations of space have made it impossible to present all the contributions to the conference. In several cases the papers have been substantially revised by their authors. We have followed a topical arrangement, grouping together papers on related subjects, and those that comment on each other.

We would like to express our thanks to the following individuals, who led discussions, chaired sessions, or generally facilitated intellectual exchange: Brian Baigrie, Ezra Brown, Mordechai Feingold, Daniel Fouke, Steve Fuller, Allen Gabbey, Daniel Garber, James Garrison, Marjorie Grene, Bernard R. Goldstein, David Lux, Deborah Mayo, Albert Moyer, Robert Paterson, and Joseph Pitt. We gratefully acknowledge the support of the National Endowment for the Humanities, an independent Federal Agency, and of the College of Arts and Sciences at Virginia Polytechnic Institute and State University, through the Center for the Study of Science in Society, the Center for Programs in the Humanities, the Department of Philosophy and the Department of History.

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LOGICAL EXAMINATION OF PHYSICAL THEORY*

Theoretical physics may be treated in the fashion of Cartesians and Atomists. They resolve the bodies perceived by the senses and instruments into immensely numerous and much smaller bodies of which reason alone has knowledge. Observable motions are regarded as the combined effects of the imperceptible motions of these little bodies. These little bodies are assigned shapes which are few in number and well defined. Their motions are given by very simple and entirely general laws. These bodies and these motions are, strictly speaking, the only real bodies and the only real motions. When they have been suitably combined, and recognized as together capable of producing effects equivalent to the phenomena we observe, it is claimed that the explanation of these phenomena has been discovered.

Our own view, Energetics, does not proceed in this manner. The principles it embodies and from which it derives conclusions do not aspire at all to resolve the bodies we perceive or the motions we report into imperceptible bodies or hidden motions. Energetics presents no revelations on the true nature of matter. Energetics claims to explain nothing. Energetics simply gives general rules of which the laws observed by the experimentalist are particular cases.

Alternatively, theoretical physics may be conceived in the [737b] manner of Newtonians. They reject all hypotheses about imperceptible bodies and hidden motions, of which the bodies and motions accessible to the senses and instruments may be composed. The [152] only principles admitted are very general laws known through induction, based on the observation of facts.

Energetics does not follow the method of the Newtonians. Energetics recognizes without doubt an experimental origin to the principles it admits, in the sense that observation has suggested them, and that experiment has many times counselled their modification. But Energetics does not regard these experiments, which explain the possible genesis of the principles that Energetics embodies, as capable of conferring any certainty whatever on these principles. Energetics regards

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these principles as pure postulates, or arbitrary decrees of reason. When they produce numerous consequences conforming to experimental laws, Energetics regards them as playing their assigned roles well. Agreement with the teaching of observation is not, therefore, as the Newtonian method would require, the beginning of physical theory; it has its place at the end.

Is Energetics being wise when it refuses equally to follow the method of Cartesians and Atomists, and the method of the Newtonians? Does careful examination of the epistemological methods of physics justify the attitude that Energetics adopts? To this question we have replied: Yes.

We have criticized the method of the Cartesians and Atomists for not being autonomous (Duhem 1892, 1906a). The physicist who wishes to follow it cannot use [738a] exclusively the methods proper to physics, since, behind perceptible bodies and motions which he regards as appearances, he aspires to get hold of other bodies and other appearances, which are the only true ones. Here he enters the domain of cosmology. He no longer has the right to shut his ears to what metaphysics wishes to tell him about the real nature of matter; hence, as a consequence, through dependence on metaphysical cosmology, his physics suffers from all the uncertainties and from all the vicissitudes of that doctrine. Theories constructed by the method of Cartesians and Atomists are also condemned to infinite multiplication and to perpetual reformulation. They do not appear to be in any state to assure consensus and continual progress to science.

We have criticized the Newtonian method for being impractical (Duhem 1894, 1906a).

A science may progress following the Newtonian method [153] while its epistemological methods remain those of common sense (*sens commun*). When science no longer observes facts directly, but substitutes for them measurements, given by instruments, of magnitudes that mathematical theory alone defines, induction can no longer be practiced in the manner that the Newtonian method requires.

> An experiment in physics is not simply the observation of a phenomenon...An experiment in physics is the precise observation of a group of phenomena accompanied by the interpretation of these phenomena. For concrete sense-impressions [*données*] really collected by observation, this interpretation substitutes abstract and symbolic representations, which correspond to them in virtue of physical theories admitted by the observer. (Duhem 1894, 1906a)

From this truism follow numerous consequences strongly opposed to the idea of a science in which each principle may be supplied by induction:

The physicist can never submit an isolated hypothesis to the control of experiment, but only a whole group of hypotheses. When experiment is in disagreement with his predictions, it teaches him that one at least of the hypotheses that constitute this group is wrong and must be modified. But experiment does not show him the one that must be changed.

Here we are a long way from the mechanism of experiment such as people who are strangers to its functioning readily imagine it. One commonly thinks [738b] that each of the hypotheses used by physics may be taken in isolation, submitted to the control of experience, and then, when varied and repeated proofs have established its value, placed into the totality of science, in an almost definitive fashion. In reality, it is not so; physics is not a machine that lets itself be disassembled. We cannot address each piece in isolation, and wait to adjust it until its soundness has been minutely controlled. Physical science is an organism one must take hold of in one piece. It is an organism in which one part cannot be made to function without the parts most distant from it coming into play, some more, some less, all to some degree. If some difficulty, some *malaise* reveals itself in its functioning, the physicist will be obliged to discover the organ that needs to be adjusted or modified without it being possible for him to isolate that organ and [154] to examine it on its own. The clockmaker to whom one gives a clock that does not work takes all the wheels out of it and examines them one by one until he finds the bent or broken one. But the doctor to whom one brings a sick person cannot dissect the patient to establish his diagnosis; he must discover the seat of the illness only through the inspection of effects produced on the whole body. The physicist responsible for repairing a rickety theory resembles the latter, not the former. (Duhem 1894,1906a)

Physical theory is not an explanation of the inorganic world; still less is it an inductive generalization of the teachings of experience. So what is it? (Duhem 1893b,1906a, 1908a, 1908d). Is theory simply, as the Pragmatists would like it, a tool [device] that gives us truths of empirical knowledge in the easiest manner, permits us to make faster and more profitable use of it in our action on the external world, but does not teach us anything about this world that we would not already have been taught by experience alone?

Or, on the contrary, does theory teach us about what is real – something that experience has not taught us and would not be able to teach us, something that would be transcendent to purely empirical knowledge?

If we were to respond affirmatively to this last question, we would be saying that physical theory is true, that it has value as knowledge. If, on the contrary it is the first question that constrains us to say "Yes", we would have to say also that physical theory is not true, but simply convenient; that it has no value as knowledge, but solely practical value. [739a]

When the physicist, turning his attention to the science he is constructing, submits the procedures that he has used to a rigorous examination, he discovers nothing able to introduce into the edifice the least particle of truth, except experimental observation. Of propositions attempting to state the facts of experience and of these alone we may say: *It is true* or: *It is false*. Of these alone we may assert that they will not permit illogicality, and that of two contradictory propositions one at least must be rejected. As for propositions introduced by theory, they are neither *true* nor *false*. They are simply *convenient* or *inconvenient*. If the physicist finds it convenient to construct two chapters [155] of physics with the aid of hypotheses that contradict each other, he is free to do so. The principle of contradiction is able to judge truth and falsity decisively. It has no ability to decide what is useful and what is not. Therefore, to require physical theory to observe a rigorous logical unity in its development would be to exert an unjust and insupportable tyranny on the intellect of the physicist.

When, after having submitted the science that concerns him to this minute examination, the physicist returns to his own concerns, when he takes notice of the tendencies that direct the steps of his reasoning, he recognizes at the same time that all his most profound and most powerful aspirations are crushed by the heartbreaking conclusions of his analysis. No, he cannot bring himself to see in physical theory only a collection of practical procedures, a bag full of tools. No, he cannot believe that physical theory only catalogs knowledge accumulated through empirical science, without changing the nature of this knowledge in the least, and without imprinting it with a character that experience alone would not be able to engrave at all. If there were no more in physical theory than critical examination had shown him in it, he would stop devoting his time and his efforts to a work of so little importance. *The study of the method of physical science is powerless to show the physicist the reason that leads him to construct physical theory.*

No physicist, however positivistic we imagine him to be, would be able to deny this declaration. But his positivism must be sufficiently rigorous that he would not go beyond this declaration, and say that his efforts towards a physical theory, which is always more unitary and always more general, are reasonable, although critical examination of the method of physical science has not been able to discover a reasonable basis for it. Such a basis, might be [739b] expressed precisely in the following propositions:

Physical theory gives us a type of knowledge of the external world not reducible to purely empirical knowledge. This knowledge comes neither from experience nor from the mathematical procedures the theory employs. Purely logical dissection of the theory would not discover the crack by which this knowledge introduces itself into the edifice of physics, through a route which the physicist can no more deny is real, any more than he can describe its course. This knowledge derives from a truth [156] other than the truths which our instruments are appropriate to grasp. The order into which theory places the results of observation does not find its full and complete justification in its practical or aesthetic aspects. We come to see, on the other hand, that this order is, or tends to become, a *natural classification*. Through an analogy the nature of which escapes the grasp of physics, but the existence of which imposes itself on the mind of the physicist as certain, we come to know that this order corresponds better and better to a certain overarching order.

In a word, the physicist is forced to recognize that *it would be irrational to work towards the progress of physical theory if that theory were not the more and more clear, and more and more precise reflection of a metaphysics. The belief in an order transcending physics is the sole reason for the existence of physical theory.*

The attitude, hostile or favorable by turns, which all physicists take towards this declaration is captured in this saying of Pascal: "Our powerlessness to prove anything is invulnerable to Dogmatism; our idea of truth is invulnerable to Skepticism [*Pyrrhonisme*]." (Duhem 1908a)

Separated from the various schools of Pragmatists on the subject of the value of physical theory, we do not take our stand, in any circumstances, among the number of their followers. The analysis we have given of experiments in physics shows fact to be completely interpenetrated by theoretical interpretation, to the point where it becomes impossible to express fact in isolation from theory, in such experiments. This analysis has found great favor on the side of many Pragmatists. They have applied it to the most diverse fields: to history, to exegesis, to theology. We do not deny that this extension is legitimate to some extent. However different the problems may be, it is always the same human intellect that exerts itself to resolve them. In the same way, there is always something common in the several procedures reason employs. But if it is good to notice the analogies between our diverse scientific methods [740a], it is on condition that we do not forget the differences separating them. And, when we compare the method of physics, so strangely specialized in the application of mathematical theory and by the use of instruments of measurement, to other methods, there are surely more differences to describe than analogies to discover.

[157] We accept that physical theory is able to obtain a certain type of knowledge of the nature of things; but this knowledge, which is purely analogical, appears to us as the terminus of theoretical progress, as the limit which theory endlessly approaches without ever reaching it. On the contrary, the schools of the Cartesians and Atomists place hypothetical knowledge of the nature of things at the origin of physical theory. If, therefore, we separate ourselves from the Pragmatists, it is not to take a place among the Cartesians or the Atomists.

The school of the neo-Atomists, the doctrines of which center on the concept of the electron, have taken up again with superb confidence the method we refuse to follow. This school thinks its hypotheses attain at last the inner structure of matter: that they make us see the elements as if some extraordinary ultra-microscope were to enlarge them until they are made perceptible to us.

We do not share this confidence. We are not able to recognize in these hypotheses a clairvoyant vision of what there is beyond sensible things; we regard them only as models. We have never denied the usefulness of these models, dear to physicists of the English school (Duhem 1893a, 1906a). We believe they lend an indispensable aid to minds more broad than deep, more able to imagine the concrete than to conceive the abstract. But the time will undoubtedly come when, through their increasing complications, these representations or models will cease to be aids for the physicist. He will regard them instead as embarrassments and impediments. Putting aside these hypothetical mechanisms, he will carefully release from them the experimental laws they have helped to discover. Without pretending to explain these laws, he will seek to classify them according to the method we have just analyzed and to understand them within a modified and a broader Energetics.

NOTES

* Part II of Duhem 1917, pp. 151–57, translated by Peter Barker and Roger Ariew; published also by Duhem in Duhem 1913a, pp. 737–40.

PIERRE DUHEM

RESEARCH ON THE HISTORY OF PHYSICAL THEORIES*

All abstract thought requires the control of facts; all scientific theories call for comparison with experience. Our logical considerations about the proper method of physics cannot be judged rationally unless they are confronted with the teachings of history. We must now apply ourselves toward gathering these teachings.

During antiquity, the Middle Ages, and the Renaissance, there has hardly been more than one part of physical theory in which mathematical theory had sufficient development and observation had sufficient precision for us to discuss their mutual relations; this part is astronomy.

With regard to the nature and value of astronomical theory, one might say that the Greek mind, so admirably supple, penetrating, and varied, conceived all the systems that our time has seen flourish again (Duhem 1908b). But among these systems, there is one that wins over the approbation of the most profound thinkers. It can be summarized in the following principle that Plato taught to those who wanted to work in astronomy: "When taking certain assumptions as our point of departure, one must attempt to save what appears to the senses – *Tinon upotethenton,...sozein ta phainomena.*" And this principle spans the Arabic, Jewish, and Christian Middle Ages, is repeated at the time of the Renaissance, is explained, specified, or contested, up to the day when Andreas Osiander formulates it thus, in the preface that he placed at the head of Copernicus' book: "*Neque enim necesse est eas hypotheses esse veras, imo, ne verisimiles quidem, sed sufficit hoc unum* [159] *si calculum observationibus congruentem exhibeant.* (It is neither necessary that these hypotheses be true nor even that they be likely, but only one thing suffices, namely, that the calculation to which they lead agrees with the result of observation.)" For two thousand years, therefore, the majority of those who reflected on the nature and value of the mathematical theory used by the physicists agreed to proclaim the axiom that Energetics came to take as its own: the first postulates of physical theory are not given as affirmations of certain suprasensible realities; they are general rules which would have played their role

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admirably if the particular consequences deduced from them agreed with the observed phenomena.

The method followed by Energetics is not an innovation; it can call forth the most ancient, most continuous, and most noble tradition for itself. But, what should we say about the essential notions and fundamental principles of that science? Logic does not require any justification of Energetics when it defines these notions and posits these principles; Logic leaves it free to posit its foundations as it wishes, as long as, having reached its zenith, the edifice is capable of accommodating without constraint or disorder the laws ascertained by the experimenter. Is that to say that Energetics defines these notions haphazardly and posits these principles without reason? Not at all. Although Logic does not impose any constraint upon Energetics, the teachings of history are an extremely sure and meticulous guide for it; the remembrance of past attempts, and of their happy or unhappy fate, prevents Energetics from receiving hypotheses which have led older theories to their ruin, or persuades it to adopt ideas which have already been shown to be fruitful. Energetics would not be able to prove its postulates, and does not have to prove them; but by retracing the vicissitudes they have gone through before they came to have their present form, it can gain our confidence for them – that is, it can obtain some credit for them at the moment when their consequences would be receiving the experimental confirmation we have anticipated.

We undertook to write the history of the great laws of statics and dynamics in order for Energetics to be in the position to understand and exhibit the evolution experienced by each of its fundamental principles.

It was known that important reflections on statics were sketched in the manuscript notes of Leonardo da Vinci. Our reading of Leonardo da Vinci and Cardano drew our attention to the unexplored statics of the Middle Ages; and soon, the act of [160] laying bare all the manuscripts on statics at the public libraries of Paris yielded unexpected discoveries in abundance (Duhem 1905–1906, vol. 1). The Christian Middle Ages had known the writings on statics composed by the Greeks; some of these writings came to it directly and others through the intermediary of Arabic commentaries. But the Latins who read those works were not at all the slavish commentators, devoid of any invention, that people were pleased to depict to us. The remains of Greek thought that they received from Byzantium or from Islamic science did not remain in their minds as in a sterile depository; these relics were sufficient to awaken their attention, to fertilize their intellect. And, from the thirteenth century on, perhaps even before that time, the school of Jordanus opened to students of mechanics some paths that antiquity had not known.

At first, the intuitions of Jordanus de Nemore were extremely vague and extremely uncertain; some grave errors were intermixed with some great truths. But soon, the disciples of the inventor refined the master's thought. The errors were eclipsed and began to disappear; the truths became more precise and firmer, and several of the most important laws of statics were finally established with complete certainty.

Specifically, we owe to the school of Jordanus a principle whose importance was demonstrated, with ever-growing clarity, during the development of statics. Without analogy to the postulates specific to the lever, of which Archimedes' deductions made use, this principle has only a distant affinity to the inexact axiom invoked by Aristotle's *Mechanical Questions*. It affirms that the same motive force can lift different weights to different heights, as long as the heights are inversely proportional to the weights. Applied by Jordanus only to the straight lever, this principle allowed one of his disciples to ascertain the law of the equilibrium of weights on an inclined plane and, by an admirable geometric device, the law of the equilibrium of the bent lever.

Descartes took up almost without change what this anonymous mathematician of the thirteenth century had written; and henceforth, from Descartes to Wallis, from Wallis to Bernoulli, and from the former to Lagrange, then to Gibbs, the principle of virtual displacements continued to be extended.

[161] Toward the year 1360, Albert of Saxony, a master of arts of the University of Paris, wrote:

> It is not true that every part of a weight tends toward its center becoming the center of the world – which would be impossible. It is the whole that descends in such a way that its center becomes the center of the world, and all the parts tend toward the goal that the center of the whole becomes the center of the world; therefore, they do not impede one another....

This center, this point which, in every weight, tends to place itself at the center of the world, is, as Albert repeated on several occasions, the center of gravity.

Therefore, every weight moves as if its center of gravity sought the center of the world – a false idea that, during the seventeenth century, engendered many errors, engaged the greatest geometers, and yielded only after a fierce discussion (Duhem 1905–1906, vol.1); but, in the meanwhile, it was a fertile idea that imparted new truths to statics. In fact, it immediately gave statics the following proposition: a system of weights is in equilibrium when the center of gravity is as low as possible. Torricelli and Pascal one day accepted that proposition as the foundation of all statics, and it gave rise to the theorem of Lagrange and Lejeune-Dirichlet on the stability of equilibrium.

Leonardo da Vinci, that indefatigable reader, leafed through and meditated endlessly upon 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 the equilibrium of the bent lever, led him to the following memorable law, which governs the composition of concurrent forces: with respect to a point taken on one of the composing forces or on the resulting force, the two other forces have equal moments (Duhem 1904, 1905–1906, vol.2, 1906–1913, vol. 1, pp. 257–319). Moreover, Albert of Saxony's ideas on the role of the center of gravity allowed him to discover the rule of the polygon of support (Duhem 1905–1906, vol. 2, 1906–1913, vol. 1, pp. 257–319), which Villalpand plagiarized (Duhem 1905–1906, vol. 2, 1906–1913, vol. 1, pp. 53–89). Thus, we find the origins of several principles essential to statics in the writings composed during the thirteenth and fourteenth centuries.

Was it the same for dynamics?

The dynamics begun by Galileo – and by those who emulated him and his disciples, such as Baliani, Torricelli, Descartes, Beeckmann, and Gassendi – is not an innovation; the modern intellect did not produce it, suddenly and completely, as soon as the reading [162] of Archimedes revealed the art of applying geometry to natural effects.

Galileo and his contemporaries made use of the mathematical skill, acquired in antiquity by the geometers while they practiced their trade, in order to render more precise and to develop a science of mechanics, a science whose principles and most essential propositions had been posited by the Christian Middle Ages. The physicists who taught this mechanics during the fourteenth century at the University of Paris had conceived it by taking observation as their guide; they substituted it for Aristotle's dynamics, convinced of its inability to 'save the phenomena'. At the time of the Renaissance, the superstitious archaism, which delighted equally in the wit of the humanists and in the Averroist habit of retrograde scholasticism, rejected this doctrine of the 'Moderns'. The reaction against the dynamics of the 'Parisians' and the inadmissible dynamics of the Stagirite was powerful, particularly in Italy (Duhem 1906–1913, vol. 3, pp. 113–261). But, in spite of this hardheaded resistance, the Parisian tradition found some masters and savants to maintain it and develop it outside the schools, as well as in the universities. Galileo and his followers were the heirs of this Parisian tradition. When we see the science of Galileo triumph over the stubborn Peripatetism of Cremonini, we believe, since we are ill-informed about the history of human thought, that we are witness to the victory of modern, young science over medieval philosophy, so stubborn in its mechanical repetition. In truth, we are contemplating the well-paved triumph of the science born at Paris during the fourteenth century over the doctrines of Aristotle and Averroes, restored into repute by the Italian Renaissance.

No motion can last unless it is maintained by the continuous action of a motive power directly and immediately applied to the mobile. That is the axiom upon which all of Aristotle's dynamics rests.

In conformity with this principle, the Stagirite wanted to apply a motive power for transporting the arrow, which continues to fly after having left the bow. He believed he had found this power in the perturbation of air; it is air, struck by a hand or by a ballistic machine, which supports and carries forth the projectile.

This hypothesis, which seems to push verisimilitude to the brink of ridicule, appears to have been accepted almost unanimously [163] by the physicists of Antiquity (Duhem 1906–1913, vol. 2, pp. 97–281). Only one of them spoke clearly against it, and he, living during the final years of Greek philosophy, is almost separated from that philosophy by his Christian faith; we are referring to John of Alexandria, surnamed Philoponus. After having demonstrated what was inadmissible about the Peripatetic doctrine of projectile motion, John Philoponus declared that the arrow continues to move without any motor applied to it, because the string has given it an energy that plays the role of motive virtue.

The last Greek thinkers and Arabic philosophers did not even mention the doctrine of John the Christian, for whom Simplicius and Averroes had only sarcastic comments. The Christian Middle Ages, in the grip of a naive admiration for the newly discovered Peripatetic science, at first shared the Greek and Arabic commentators' disdain for Philoponus' hypotheses; Saint Thomas Aquinas mentions the hypothesis only to warn those who might be seduced by it.

But, following the condemnations brought forth in 1277 by Etienne Tempier, the Bishop of Paris, against a set of theses upheld by 'Aristotle and his followers', there appeared a large movement that liberated Christian thought from the shackles of Peripatetic and Neoplatonic philosophy and produced what the Renaissance archaically called the science of the 'Moderns'.

William of Ockham attacked Aristotle's theory of projectile motion with his customary zeal (Duhem 1906–1913, vol. 2, pp. 97–281). He was content, however, in destroying without building, but his critiques restored into repute the doctrine of John Philoponus for some of Duns Scotus' disciples. The *energy*, the motive virtue of which Philoponus spoke, reappeared under the name *impetus*. The hypothesis of impetus – what was impressed into the projectile by the hand or the machine that launches it – was taken over by a secular master of the Faculty of Arts of Paris, a physicist of great genius (Duhem 1906–1913, vol. 3, pp. 1–112). Toward the middle of the fourteenth century, John Buridan took impetus as the foundation of a dynamics that 'accords with all the phenomena'.

The role that impetus played in Buridan's dynamics is exactly the one that Galileo attributed to *impeto* or *momento*, Descartes to *quantity of motion*, and Leibniz finally to *vis viva*. So exact is this correspondence that, in order to exhibit [164] Galileo's dynamics, Torricelli, in his *Lezioni accademiche*, often took up Buridan's reasons and almost his exact words.

Buridan took this impetus, which remains without change within the projectile unless constantly destroyed by the resistance of the medium and by the action of weight contrary to the motion, to be proportional to the quantity of primary matter within the body; he conceived and described that quantity in terms almost identical to those Newton used to define mass. With equal masses, the impetus increases as the speed increases; Buridan prudently abstained from further specifying the relation between the magnitude of the impetus and that of the speed. More daring, Galileo and Descartes affirmed that this relation is reduced to proportionality; thus they obtained an erroneous estimation for impeto and for quantity of motion, which Leibniz needed to rectify.

Gravity increases indefinitely, as does the resistance of the medium, and it ends up annihilating the impetus of a mobile thrown upward, since such a motion is contrary to the natural tendency of that gravity. But with a falling mobile, motion conforms to the tendency of gravity.

Thus, the impetus must be augmented indefinitely and speed must increase constantly during the motion. Such is, according to Buridan, the explanation for the acceleration observed in the fall of a weight, an acceleration that Aristotle's science already understood, but for which the Greek, Arabic, or Christian commentators of the Stagirite had given unacceptable reasons.

This dynamics exposited by Buridan presents in a purely qualitative, but always exact fashion the truths that the notions of *vis viva* and work allow us to formulate in quantitative language.

The philosopher of Béthune was not alone in professing this dynamics; his most brilliant disciples, Albert of Saxony and Nicole Oresme, adopted it and taught it. The French writings of Oresme allowed it to be understood even by those who were not clerics (Duhem 1906–1913, vol. 3, pp. 261–583).

When no resistant medium, when no natural tendency analogous to gravity is opposed to motion, the impetus maintains a constant intensity. The mobile, to which a motion of translation or of rotation has been communicated, continues [165] to move indefinitely in the same manner, with a constant speed. That is the form under which the law of inertia presented itself to the mind of Buridan; it is the form under which it was received by Galileo.

From this law of inertia, Buridan derived a corollary whose novelty we should admire (Duhem 1906–1913, vol. 2, pp. 97–281). The celestial orbs move eternally with a constant speed, because, according to the axiom of Aristotle's dynamics, each one of them is subject to an eternal motor of immutable power. The Stagirite's philosophy required that such a motor be an intelligence separated from matter. The study of the motive intelligences of the celestial orbs was not only the crowning glory of Peripatetic metaphysics, it was the doctrine about which revolved all the Neoplatonic metaphysics of the Greeks and Arabs; the Scholastics of the thirteenth century did not hesitate to receive this heritage of the pagan theologies into their Christian systems.

Now, Buridan had the boldness to write these lines:

> Since the creation of the world, God has moved the heavens by movements identical to those by which they are actually moved. Hence, he has impressed upon them some *impetus* by which they continue to be moved uniformly. In effect, these *impetus*, encountering no contrary resistance, are never destroyed or weakened . . . . According to this imagination, it is not necessary to posit the existence of intelligences moving the celestial bodies in an appropriate manner.

Buridan expressed this thought in various places; Albert of Saxony formulated it also (Duhem 1906–1913, vol. 2, pp. 97–281); and Nicole Oresme, in order to formulate it, made use of this comparison: "Violence excepted, the situation is similar to a man making a clock and letting it go and move by itself."

If we wanted to draw a precise line separating the period of ancient science from the period of modern science, we would have to draw it at the instant when John Buridan conceived this theory, at the instant when the stars stopped being perceived as moved by divine beings, when celestial motions and sublunar motions were admitted as dependent upon a single mechanics.

This mechanics, both celestial and terrestrial, to which Newton gave the form we admire today, [166] was attempting to constitute itself ever since the fourteenth century. The writings of Francis of Mayronnes (Duhem 1913b) and of Albert of Saxony (Duhem 1909) during the whole of that century teach us that there were physicists who maintained that one could construct a more satisfactory astronomical system than the one in which the earth is deprived of motion, by assuming the earth mobile, and heaven and the fixed stars immobile. Of these physicists, Nicole Oresme developed the reasons for this doctrine (Duhem 1909) with a fullness, clarity, and precision that Copernicus was far from achieving. He attributed to the earth a natural impetus similar to the one Buridan attributed to the celestial orbs. In order to account for the vertical fall of weights, he allowed that one must compose this impetus by which the mobile rotates around the earth with the impetus engendered by weight. The principle he distinctly formulated was only obscurely indicated by Copernicus and merely repeated by Giordano Bruno (Duhem 1906–1913, vol. 3, pp. 113–261). Galileo used geometry to derive the consequences of that principle but without correcting the incorrect form of the law of inertia implied in it.

While dynamics was being established, the laws of falling weights were being discovered a few at a time.

In 1368, Albert of Saxony proposed these two hypotheses: the speed of the fall is proportional to the time elapsed from the start; the speed of the fall is proportional to the path travelled (Duhem 1908c, 1906–1913, vol. 3, 261–568). He did not choose between these two laws. The theologian, Peter Tataret, who taught at Paris toward the end of the fifteenth century, reproduced textually what Albert of

Saxony had said. The great reader of Albert of Saxony, Leonardo da Vinci, after having accepted the second of these two hypotheses, rallied to the first. But he was not able to discover the law of spaces traversed by a falling weight; by a reasoning that Baliani took up, he concluded that the spaces traversed in laps of equal and successive times are as the series of whole numbers, while, in truth, they are as the series of odd numbers.

However, the rule that allowed the evaluation of the space traversed, in a certain time, by a mobile moving in a uniformly varied motion was known for a long time. Whether this rule was discovered at Paris, during the time of John Buridan, or at Oxford, during the time of Swineshead, it was formulated clearly in the work in which Nicole Oresme posited the essential principles of analytic geometry (Duhem 1906–1913, vol. 3, 261–568). [167] Moreover, the demonstration that serves to justify it is identical to the one Galileo gave for it.

This rule was not forgotten from the time of Nicole Oresme to the time of Leonardo da Vinci; formulated in most of the treatises produced by the thorny dialectics of Oxford, it was discussed in the various commentaries of which these treatises were the object, during the fifteenth century, in Italy, and then in the various works of physics written at the start of the sixteenth century by Parisian Scholasticism.

None of the treatises of which we have just spoken, however, contains the thought of applying this rule to the fall of weights. We encounter that thought for the first time in the *Questions on Aristotle's Physics* published in 1545 by Domingo de Soto (Duhem 1906–1913, vol. 3, 261–568). A student of the Parisian Scholastics, most of whose physical theories he received and adopted, the Spanish Dominican de Soto admitted that the fall of a weight is uniformly accelerated, that the vertical rise of a projectile is uniformly retarded, and, in order to calculate the path traversed in each of these two movements, correctly used the rule formulated by Oresme. That is to say, he knew the law of falling weights, whose discovery is attributed to Galileo. Moreover, he did not claim the discovery of these laws; rather, he seemed to be giving them as commonly received truths. No doubt they were accepted at the time by the Paris masters whose lessons de Soto followed. Thus, from William of Ockham to Domingo de Soto, we see the physicists of the Parisian school posit the foundations of the mechanics Galileo, his contemporaries, and his disciples developed.

Among those who, before Galileo, received the tradition of Parisian Scholasticism, there was none who deserved more attention than Leonardo da Vinci. During the time he lived, Italy firmly resisted the penetration of the mechanics of the '*moderni*', of the '*juniores*'. Among the university masters, even those who leaned in the direction of the terminalist doctrines of Paris, merely reproduced, under an abridged and often hesitant form, the essential assertions of that mechanics; they were far from being capable of having it produce any of the fruits of which it was the flower.

Leonardo da Vinci, on the contrary, was not satisfied in admitting the general principles of the dynamics of impetus. [168] He meditated endlessly upon these principles, and turned them every which way, pressing them in some fashion to deliver the consequences they enclosed (Duhem 1906–1913, vol. 3, 113–261). The essential hypothesis of that dynamics was similar to the first form of the law of *vis viva*; da Vinci perceived in it the idea of the conservation of energy, and he found some terms of almost prophetic clarity to express that idea (Duhem 1906–1913, vol. 2, pp. 97–281). Albert of Saxony had left his reader in suspense between the two laws of falling weights, the one correct and the other inadmissible. After some tentative steps that Galileo also went through, da Vinci came upon the choice of the correct law. He extended it happily to the fall of a weight along an inclined plane (Duhem 1906–1913, vol. 3, 261–568). Through a study of composite *impeto*, he attempted the first explanation of the curvilinear trajectory of projectiles, an explanation that was completed by Galileo and Torricelli (Duhem 1906–1913, vol. 2, pp. 97–281). He glimpsed the correction that needed to be brought to the law of inertia announced by Buridan, and he prepared for the work that Benedetti and Descartes accomplished (Duhem 1906–1913, vol. 3, pp. 113–261).

No doubt, da Vinci did not always recognize the richness of the treasures accumulated by Parisian Scholasticism. He set aside some of them, which would have been complementary to his doctrine of mechanics. He misunderstood the role that impetus must play in the explanation of the accelerated fall of weights (Duhem 1906–1913, vol. 3, pp. 113–261). He was unaware of the rule which allows the calculation of the path traversed by a body moving of uniformly accelerated motion. It is no less true that the whole of his physics placed him among those the Italians of his time called the Parisians.

Moreover, this title was properly given to him. In fact, his principles of physics were derived from an assiduous reading of Albert of Saxony, and probably also from a meditation upon the writings of Nicholas of Cusa (Duhem 1906–1913, vol. 2, pp. 97–281); and Nicholas of Cusa was also an initiate of the Parisian mechanics. Da Vinci is therefore given his proper place among the Parisian precursors of Galileo.

We have just retraced, in broad strokes, the essential laws of equilibrium and motion at their infancy. On occasion, we have described some portions of physics at the time when that science had reached adolescence. Thus, we have inquired into the sources of the hydrostatic theories of Pascal (Duhem 1905), detailed the role that Mersenne played in the discovery of the weight of air (Duhem 1906b), and sketched the genesis of [169] the doctrine of universal attraction (Duhem 1906a). Now, we did not see any essential principles proceed from the desire to resolve the bodies we perceive and touch into imperceptible, but simpler bodies; we saw none that had as aim to explain sensible motions by means of hidden motions. Atomism did not contribute to their formation in any way. All of them were born from the desire to formulate some very general rules whose consequences 'saved the phenomena'. Thus, the history of the development of physics has come to confirm what the logical analysis of the methods used by that science had taught us. From the former and from the latter, we have gained a renewal of faith in the future fruitfulness of the method of Energetics.

NOTE

*Part III of Duhem 1917, pp. 158–69, translated by Roger Ariew and Peter Barker.

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DUHEM, THE ARABS, AND THE HISTORY OF COSMOLOGY

ABSTRACT. Duhem has generally been understood to have maintained that the major Greek astronomers were instrumentalists. This view has emerged mainly from a reading of his 1908 publication *To Save the Phenomena*. In it he sharply contrasted a sophisticated Greek interpretation of astronomical models (for Duhem this was that they were mathematical contrivances) with a naive insistence of the Arabs on their concrete reality. But in *Le Système du monde*, which began to appear in 1913, Duhem modified his views on Greek astronomy considerably; his more subtle understanding included the recognition that many Greeks subordinated mathematical astronomy to physical theory. But he could not completely repudiate his earlier views about Greek astronomy in part because his extreme nineteenth century prejudices led him to continue to insist on a clear-cut demarcation between Greek and Arabic astronomy. The inevitable result is a certain unevenness in the *Système* and some glaring inconsistencies.

Given the totality of Duhem's enormous output, one would be hard put to claim that Arabic science played more than a peripheral role in his historical and philosophical writings. And because Duhem, who did not know Arabic, was often grossly mistaken in his views and interpretations of Arabic science, leaving it little more than a grotesque caricature, it is appropriate to ask: why bother? There are several reasons, which may serve both as an introduction and an apologia. It is a far from worthless exercise to try to discover why a great thinker goes so far off the track. It is rather facile to claim that Duhem's views were a result of his anti-Semiticism; but this takes us only so far and fails to put his views into historical perspective. Yes, Duhem was rather extreme in his notions of Arabic science, but I think he only took to a logical conclusion views that were until recently fairly prevalent. And the continuing influence of Duhem's historical works makes an evaluation of his attitude toward Arabic science even more imperative than it would otherwise be. Another reason to look at Duhem's understanding of Arabic astronomy is because of his use of it as a foil to put in bolder relief the genius of Greek science. As we know from recent debates, Duhem left himself open to interpretation in the use of his cherished 'saving the phenomena'. Because in Duhem's view the Arabs could never rise to the Greeks' elevated understanding of astronomical

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theory, a study of what he said about their failure – indeed ignorance – can provide, I believe, some useful insight into what he intended by 'saving the phenomena'. My own contention will be that there was a marked evolution and refinement in his thinking about Greek astronomy between *To Save the Phenomena* (herein abbreviated STP), which appeared in 1908, and the second volume of *Le Système du monde*, which was published in 1914. But there was no similar change in thinking about Arabic astronomy, a situation that led to a major inconsistency in the *Système*; ironically this inconsistency could have been dealt with, though perhaps not completely resolved, if Duhem had been willing to be less hostile toward the Arabs. But he was unable, or perhaps unwilling, to do this.

In order to illustrate my point, I shall be concentrating on the question of Duhem's understanding of the relation of ancient physics to mathematical astronomy. I believe that by examining this issue (rather than, say, the much more intractable question of the reality of astronomical models)[1] one can most clearly understand Duhem's evolution as well as the importance, and limitation, of the criticism of G. E. R. Lloyd (1978), Duhem's most severe critic on this point. Without going into a detailed exposition or analysis of STP, I should like to give a brief overview of some of its main points.[2]

According to Duhem, there were basically two methods for dealing with the celestial realm: the method of the astronomer and the method of the physicist. Plato had set forth the method of the astronomer, namely to save the appearances of the planets using only uniform, circular motions. This was the method of Eudoxus and Callippus, who sought to save the appearances with homocentric spheres. On the other hand, Aristotle, representing the physicists, went beyond the merely mathematical saving of the appearances and sought to impose other restrictions that had to do with the nature of the heavenly bodies. Thus the fictitious models of Eudoxus were turned by Aristotle into combinations of real spheres whose nature it was to move with uniform, circular motion. Because they were real and not simply mathematical, Aristotle needed to add extra counterspheres in order to keep the system of spheres for each planet from interfering with the others.

At this point, Duhem was illustrating the relation of physics to astronomy by using the example of a physicist, in this case Aristotle, attempting to make real the mathematical models of the astronomers. But the relationship became more complicated when the Greek astronomers found that different hypotheses or models could equally well save the phenomena. This was emphasized by Hipparchus who was struck by the fact that an epicycle on a concentric deferent could be made mathematically equivalent to an eccentric model. The relationship was further complicated by the fact that an astronomy using epicycles and eccentrics was no longer compatible with an Aristotelian physics that demanded that all celestial motion be uniform about the center of the Universe. Among others Adrastus of Aphrodisias and Theon of Smyrna sought to accommodate the new astronomy by proposing a modified Aristotelian physics that exempted celestial motion from being about the center of the world but still held that the orbs, whether eccentric, epicyclic, or concentric, had to be solid bodies.

As we have seen, Duhem claimed that Eudoxus and Callippus had followed the method of the astronomers as laid out by Plato; he also put Hipparchus in this category. But it was with Ptolemy that a new, much more sophisticated understanding of the relation of mathematical astronomy to physics was reached. Since "no craftsman could have constructed a wooden or metal representation of [his hypotheses in the *Syntaxis*] . . . Ptolemy's followers were bound – on pain of abandoning their own doctrines – to liberate astronomical hypotheses from the conditions to which physicists had generally subjected them".[3] Duhem quoted a number of passages to support his position but the most important by far was from the *Almagest*, Bk. 13, Chap. 2,[4] in which Ptolemy, according to Duhem, "means to indicate...that the many motions he compounds in the *Syntaxis* to determine the trajectory of a planet have no physical reality; only the resultant motion is actually produced in the heavens".[5] Duhem related this interpretation of Ptolemy to the earlier doctrine of the Stoic Cleanthes (d.ca. 230 B.C.), who held that the planet was self-propelled and described whatever curves were observed, and to the later view of the neo-Platonist Proclus (d. 485 A.D.), who, in Duhem's reading, took the essence of the heavenly motions to be irregularity. The final freeing of the astronomers from the restrictions of the physicists was complete. And what would be the role of the physicist?

> But only the physicist would be authorized to say whether or not [the mathematical models] conform to reality. Generally speaking, the principles he is able to affirm are too general, too remote from particulars, to empower him to pronounce that kind of judgment.[6]

In other words, the method of the astronomer was eventually victorious in ancient Greece.

Lloyd has severely criticized most of Duhem's contentions in a detailed way. There is no need to rehearse the entire list; it is only important for our purposes to identify three of his main objections. First he makes the case that Duhem has ignored the strong evidence that the astronomers, far from being independent, took their starting-points or principles from the physicists. This, for example, was stated explicitly by Geminus, whom Duhem took as a witness for the opposite point of view.[7]

A second criticism by Lloyd concerns Ptolemy. Whatever one may wish to say about his position in the *Almagest*, there is the matter of the *Planetary Hypotheses* in which there is no doubt that he is taking a realist stance. How else is one to understand the transforming of the circles of the *Almagest* into physical bodies, a metamorphosis effected in Book II of the *Planetary Hypotheses*? And even if one were to excuse Duhem in 1908 for not having gotten hold of L. Nix's German translation from the Arabic of the nonextant Greek text of Book II, a work published in 1907,[8] one could still argue, as does Lloyd, that the physical arguments in Book I of the *Almagest* should have alerted Duhem that Ptolemy was ostensibly basing his mathematical models, indeed subordinating them, to a revised Aristotelian physics.[9]

Lloyd's third criticism has to do with Proclus. Duhem did not hesitate to compare him to the positivists, indeed to John Stuart Mill;[10] but Lloyd systematically shows that whatever Proclus's neo-Platonist position may have been, and if anything it is ambiguous, he was fairly consistent in arguing from realist assumptions whether he was criticizing realist or instrumentalist alternatives.[11]

One cannot but admire the incisive way Lloyd has gone about refuting Duhem's contentions in *To Save the Phenomena*. It is a masterful piece of writing, but it has not gone without criticism. Niall Martin (1987) has criticized Lloyd and others for viewing Duhem too simplistically; Martin claims that Duhem was more than aware that the ancients were not instrumentalists per se but rather tied their instrumentalism, if we can call it that, to cosmological and epistemological stances. Thus Ptolemy's instrumentalism was not to be separated from his stoicism just as Maimonides' should not be understood as separable from his religious beliefs; in both cases their skepticism about the possibility of a complete understanding of celestial phenomena, as distinct from sublunar phenomena, led them to their quasi-instrumentalism.[12] Now both Lloyd and Martin are basing their positions on STP; from that context alone, Martin may have a point but one must admit that Duhem certainly seems to be attributing instrumentalist views to the Greeks. After all it was Duhem, not Lloyd, who evoked the name of John Stuart Mill in his discussion of Proclus. But to judge Duhem solely with reference to STP, as Lloyd and Martin both do, is to do him a grave disservice. For this was not Duhem's final word; in Volume II of *Le Système du monde* (1914) he took up many of the issues of Greek and Arabic astronomy that he had dealt with before in STP but in a much more careful way. This is Duhem's mature work, and I think it only fair that we take it into account when we discuss his views on Greek astronomy. And on the crucial issue of the relation of physics to astronomy, Duhem has made a number of significant shifts.

While one can hardly say that Duhem has repudiated the thesis of STP, one can say that he has modified it in subtle and at times decisive ways in the *Système*. On the surface Duhem seems to carry the main theme of STP into his later work. For example in the *Système*, he characterized what he calls "this war" as being between "those who want Physics [in the modern sense] to be deducible from a set philosophical system and . . . between those who require nothing more from [physics] than that it agree exactly with experience".[13] But whereas many points made in STP were repeated, sometimes verbatim, in the *Système*, the important relationship between the principles of physics and mathematical models in Greek astronomy was explored with much greater depth and precision, and Duhem reached certain conclusions that undermine, if not contradict, various assertions of STP. For example, in discussing the homocentric spheres of Eudoxus and Callippus in the *Système*, Duhem posed a question never addressed in STP, namely

> If Plato and Aristotle were only interested in obtaining mathematical rules that would permit them to predict with certainty and precision the movements of the stars, why would they impose in advance of these rules the obligation to be constructed in a certain manner? . . . Why would they constrain [astronomy] . . . with circular and uniform movements? Why would they further restrain [astronomy's] ability to choose by obliging it to configure the World with a system of homocentric spheres? Such requirements are enough to give notice that neither Plato nor Aristotle would have consented to reduce the object of astronomy to the [following] single problem: to conceive of geometrical hypotheses that would save the phenomena.[14]

There is more than one thing surprising here. After all Plato in STP had been assigned the role of initiator of the call to the astronomers 'to save the phenomena'. Duhem had mentioned there that Plato had required that this be done with uniform, circular motions, but the obvious physicalist implications had been ignored. Here, however, we see that he has not only recognized the problem but has also drawn the obvious conclusion about Plato, namely that he was hardly an instrumentalist.[15]

But there is another aspect to the problem that was new in the *Système*. Did an astronomer who accepted uniform, circular motion as a starting point for astronomy accept *ipso facto* the constraints of the physicists? And if so, what did this do to the radical separation between astronomers and physicists in STP? Again the ambiguities and silence of STP gave way to a direct assault on the problem in the *Système*. In discussing Dercyllides, Duhem noted that he was not an innovator when he "affirms the dependence, generally recognized by the philosophers, between astronomy and physics; others...have also detailed the character of this dependence". This is not very different from what one might find in STP. But then he went on to say that "those who compose astronomical treatises, respectful [*respectueux*] observers of these precepts, begin by enunciating the postulates, borrowed from Physics, which must [*devaient*] serve as points of departure for their deductions".[16] We should recall here that one of Lloyd's criticisms was that Duhem ignored the relation of the physical principles to the starting points of the astronomers. But in addressing his future critic, Duhem has made the previously clear-cut notion of 'saving the phenomena' rather ambiguous in the *Système*.

Lest one think that this is somehow unrepresentative, we should examine what Duhem said in the *Système* about Ptolemy, who was after all the Greek astronomer Duhem had claimed in STP most clearly represented the doctrine of 'saving the phenomena'. Far from denying that Ptolemy was constrained by physics, Duhem must admit that "at the beginning of his work, Ptolemy formulates his postulates as if astronomy must be entirely based upon principles of complete certitude, upon incontestable verities from Physics". But Duhem found that "at the end [of the work] . . . instructed by experience, the author does not grant his hypotheses to be anything more than contrivances that are appropriate to save, as simply as possible, the phenomena".[17] Here one would have expected the Duhem of STP to end the matter; Ptolemy had paid lip service to the physical principles, found them too constraining, and ended by rejecting them. But instead Duhem provided a real surprise. At this point he himself introduced the same weapon that Lloyd and others have used to question the Duhemian interpretation of Ptolemy, namely the *Planetary Hypotheses*. After assuring his readers, no doubt sadly, that the text is authentic, he proceeded to give a fairly detailed summary of the second book in which Ptolemy attempted to physicalize his models from the *Almagest*.[18] Duhem was fully aware of what this meant for his interpretation of Ptolemy. But he tried to salvage what he could:

> Ptolemy had rightly been able to scorn this desire to represent the movements of the celestial and imperishable bodies by means of these rough and changing bodies that make up for us the sublunar world; his criticisms had not won a definitive victory; the error that they were combatting was one of those which, apparently vanquished, overturned one moment, rise up again without ceasing, because they are the necessary consequence of an incorrigible failing of the human spirit. What Dercyllides, Adrastus and Theon had wanted was to embody abstract thoughts in concrete models that the eyes could see, that the hands could touch and move; it was to drive away reason in order to put imagination in its place. Ptolemy, after having defended reason, became, in his turn, a slave of the imagination.[19]

At this point Duhem turned to the neo-Platonists, in particular Proclus. What he found was someone less ambiguous and much more to his liking. But he had to retranslate the relevant passages from the *Hypotyposes* since he realized, long before Lloyd would make the point, that Father Halma's translation that he had depended on in STP was seriously defective. Thus Duhem could no longer have Proclus saying that the "essence of [the celestial] movements...is irregularity".[20] But Duhem still held that Proclus viewed astronomical hypotheses as mere fictions in part because he chose to concentrate on his Platonic worldview, a philosophical stance that led Proclus to be sceptical of all attempts to deal with the celestial appearances as distinct from the real heavens, which to him were, as they had been to the divine Plato, beyond perception. Lloyd, on the other hand, while recognizing this aspect of Proclus's position,[21] could counter Duhem's contentions by pointing to Proclus's realist (or perhaps it would be better to call them physicalist) suppositions under which he operated even when dealing with the appearances.

But again where did this leave Duhem? After losing both Plato and Ptolemy as pure representatives of the view that the astronomical hypotheses were mere contrivances, he was basically left with Proclus, someone that Lloyd has accurately characterized as "not exactly one of the leading lights in the history of Greek astronomical theory".[22] One would think that Duhem would be forced to back off from some of the more extreme claims of STP. But Duhem did not draw the obvious conclusion from his work, namely that the most important Greek thinkers held a basically realist position and subordinated mathematical astronomy to physics. It was not a pure position, it was not completely clear-cut, but his work could lead to no other conclusion. Though it is too much to expect that Duhem would have completely repudiated the major thesis of STP, it still comes as a shock to find the following passage from STP repeated in the *Système*:

> After some initial hesitation [the Greeks] balked at the idea that the eccentrics and epicycles are bodies, really up there on the vault of the heavens. For the Greeks they were simply geometric fictions requisite to the subjection of celestial phenomena to calculation. If these calculations are in accord with the results of observation, if the hypotheses succeed in saving the phenomena, the astronomer's problem is solved.[23]

Interestingly enough, this passage does not occur in the chapter on the Greeks but rather in the introduction to the one on the 'Semites'. All the ambiguity, the subtlety of the previous chapter on the Greeks is lost; Ptolemy once again becomes someone whose hypotheses are 'calculating devices'.[24] The *Planetary Hypotheses* are conveniently forgotten. Why Duhem chose to start his chapter on the 'Semites' in this way is apparent from the next paragraph:

> The prodigious geometric ingenuity of the Greeks did not form part of the heritage they passed on to the Arabs. Nor did the Arabs have the Greeks' remarkably sure and precise logical sense. They brought only some very minor improvements to the hypotheses whereby the Greek astronomers had managed to resolve the complex course of the planets into simple motions. Moreover, when they did at last come to examine these hypotheses in an attempt to make out their nature, their vision could not match the penetration of a Posidonius, a Ptolemy, a Proclus, or a Simplicius; slaves to their imagination, they tried to see and touch what the Greek thinkers had declared fictive and abstract.[25]

It is this stark contrast between the Greek mind and the Arab mind that Duhem wished to bring out in both STP and the *Système*. The fact that the Greeks were no longer so pure and that Ptolemy himself had been called a slave to the imagination did not prevent Duhem from repeating verbatim in the *Système* what he had written in STP. Duhem's antipathy toward the Arabs is not in doubt; indeed one can see it as part of the nineteenth century European cultural baggage.[26] But even for the nineteenth century Duhem was extreme. The person closest to his view on Arabic science was Ernest Renan, with whom Duhem shared hardly anything else.[27] Renan had denied both the Arabs and Islam a role in the history of science; nomadic Arabs were incapable of science or philosophy and Islam was too hostile to allow science to flourish. What went under the name Arabic or Islamic science was due to Christians, Jews, Persians, and others, or Muslims who, like Galileo, had heroically freed themselves of their religion.[28] But Duhem went Renan one better; while Renan had not discounted the contributions that went under the name Arabic or Islamic science, Duhem essentially denied that there was anything of importance in Arabic astronomy.[29] When he did find something worthwhile, such as the homocentric system of the Spanish Arab Biṭrūjī (fl. 1200 A.D.), which he claimed helped pave the way for Copernicus by providing an alternative to the Ptolemaic system, he rejected the possibility that any Arab could have been responsible for it.[30] The way he did this is extraordinary; Duhem maintained that the ordering of letters in the diagrams of Biṭrūjī followed the Greek alphabet and hence his system must depend on or be plagiarized from a Greek original.[31] It would be hard to exaggerate the silliness of this argument, and it is incredible to me that someone of Duhem's immense abilities could have fallen for something so ludicrous.[32] Since the Arabic alphanumeric system follows the old Semitic ordering, the one used, for example, by the Phoenicians who bequeathed it to the Greeks, it is no wonder that the Greek and Arabic ordering would be the same. Thus using Duhem's reasoning, one could hardly escape the conclusion that virtually every Arabic scientific text was plagiarized from some Greek original.[33]

We have seen how Duhem's wish to draw a sharp dichotomy between the Greeks and Arabs led him to gloss over what I consider to be some of his most important work in the history of cosmology. What is sad, and indeed unfortunate for the history of science, is that Duhem did not notice, or did not wish to notice, that the ambiguous Greek attitude toward the relation of physics and astronomy that he brought to light in the *Système* was dealt with in interesting ways in Islamic astronomy. If Duhem had simply drawn the obvious conclusion from his own research that the major Greek philosophers and astronomers were committed in varying degrees to the proposition that the principles of mathematical astronomy must come from both mathematics and physics, he would have been more sensitive to the fact that the Arab thinkers that he studied were not so much naive realists as scientists interested in reconciling the inconsistencies in astronomical theory that they had inherited from the Greeks. Let me try to make this point as clear as possible. In accepting that astronomy was based on both mathematical and physical principles, Arab astronomers reached a rather simple conclusion – the mathematical models had to be consistent with the physical principles. Ptolemy had not been able to accomplish this in the *Almagest*, and indeed in Bk. 13, Chap. 2, he does seem to be taking refuge in some neo-Platonic worldview. But starting with Ibn al-Haytham in the eleventh century, this was seen not so much as a serious position on the relation of physics to mathematics but as simple inconsistency.[34]

Despite the fact that Duhem in the *Système* had understood the difference between metaphysics and physics in the Greek context and in fact had used it to distinguish between the justifications of Aristotle and Plato for uniform, circular motion, I would agree with Martin that Duhem remained committed to the view that "'astronomy' is the ancient correlate of modern physics, and 'physics' the ancient correlate of modern metaphysics".[35] This was what led him, as well as other historians of astronomy such as E. S. Kennedy and Otto Neugebauer, not to take the physical principles of ancient and medieval astronomy as seriously as the physical principles of modern physics. (This is reflected in the terminology regarding these physical principles, which are usually referred to as 'philosophical'.)[36] Thus it is easy to see why for Duhem, such figures as Ptolemy, Proclus, and Maimonides were the heroes of his story because they, as 'neo-Platonists', sharply separated astronomy from some true metaphysical reality. But what this ignores is that the physical principles were not justified solely in a metaphysical way either by the Greeks or Arabs and were regarded as having, for example, empirical justifications. Thus Aristotle, despite having what we would call metaphysical reasons for preferring uniform, circular motion in the heavens, did not shy away from giving 'empirical proofs'.[37] For reasons that we need not go into here, Islamic astronomers and physicists sought at various times to disentangle the physical principles from any metaphysical taint.[38] Uniform, circular motion could therefore be understood as justified physical premises that were established both from observation and from successful usage. In this regard it is interesting that when a physical premise did not seem to have empirical justification, a position taken by Ṭūsī concerning the Earth's state of rest, the response by his successors was to find an empirical justification, not to insist on some metaphysical principle.[39]

It may be argued that Duhem did not know enough about Arabic science to be able to form a proper judgement. Obviously he did not know of some of the more recent discoveries nor of the relation of Copernicus to Islamic astronomy.[40] He did, however, know of Naṣīr al-Dīn al-Ṭūsī's attempt to reform the Ptolemaic system since this was in a French translation that Duhem cited in the *Système*.[41] Duhem should have realized that Ṭūsī's proposed model that circumvented the equant, a device used by Ptolemy in his planetary theory that led to an irregular motion of the orb, resulted from a desire to deal with logical inconsistency in the Ptolemaic system, not from naive realism. This should have been especially clear to him in view of his understanding of the relation of physics and mathematics that he had come to in the *Système*. And it should have also been clear to him since Ṭūsī, like Duhem himself, took seriously the logical foundations of his discipline. Thus if one begins with certain principles it only seems reasonable to stick with them, something that Ptolemy did not do. Whether Duhem would have seen the Arabic contribution in this light we shall never know. I take this as a tragedy not only for Duhem but for the history of science since he was a man of great insights and intuitions. But blinded by prejudice, he did not care to delve into the problems of those who were 'slaves to the imagination'.

NOTES

1 As I shall elaborate on below, the issue that became of paramount importance in Islamic astronomy was that of the consistency between the physical and mathematical principles. I would argue that given the admitted mathematical equivalence between various astronomical models that were considered physically acceptable, the question of whether the models represented the 'real world' became rather secondary.

2 Cf. Duhem (1908), pp. 1–27; translation, 1969, pp. 3–24.

3 Ibid., p. 17; translation, p. 16.

4 Heiberg (1903), pp. 532–34; translation, Toomer (1984), pp. 600–601.

5 Duhem (1908), p. 18; translation, 1969, p. 17.

6 Ibid., p. 27; translation, p. 25.

7 Lloyd (1978), pp. 212–14.

8 Heiberg (1907), pp. 111–45.

9 Lloyd (1978), pp. 215–17.

[10] Duhem (1908), pp. 23–24; translation, 1969, p. 21. Duhem hedges a bit on the comparison with the positivists by remarking that "the line of demarcation [separating the objects accessible to human knowledge from those that are essentially unknowable to man] is not the same for Proclus as it is for John Stuart Mill".

[11] Lloyd (1978), pp. 204–11.

[12] Martin (1987), pp. 309–12.

[13] Duhem (1914), **2**, 62–63.

[14] Ibid., **2**, 69–70.

[15] The quoted passage is rather less shocking as regards Aristotle since in STP we had already been apprised of his physicalist leanings. But Duhem explored the implications of both Plato's and Aristotle's positions in much greater depth in the *Système*. He concluded that for Plato the insistence on uniform, circular motion was a theological requirement that resulted from an ontological hierarchy in which the true motions, as distinct from the observed ones, were as perfect as possible. For Aristotle this was a physical requirement that resulted from an epistemological hierarchy that made geometrical astronomy subordinate to physics (ibid., **2**, 71). Duhem's entire discussion of the relation of physics and astronomy before Ptolemy (ibid., **2**, 67–83) is extremely valuable. It is now clear to me that had I read the *Système* more carefully earlier, I would have seen that the Islamic discussion of this relation owes more to Greek precedents than I had assumed in Ragep (1982), **1**, 129–89; I was there still, at least partially, under the spell of STP, even while reacting against it, and did not take into account the extent to which Greek astronomy subordinated itself to physics.

[16] Duhem (1913), **1**, 469.

[17] Duhem (1914), **2**, 86.

[18] Ibid., **2**, 86–99.

[19] Ibid., **2**, 99.

[20] Duhem (1908), p. 20; translation, 1969, p. 19. Duhem (1914), **2**, 103 (n. 1) criticized Halma's translation (1820, p. 151) that he had depended on in 1908. In one of the few places he refers to the *Système*, Lloyd (1978), p. 205, n. 20, notes Duhem's correction of Halma.

[21] See Lloyd (1978), esp. pp. 207, 209.

[22] Ibid., p. 211.

[23] Duhem (1908), p. 27 (translation, 1969, p. 25); Duhem (1914), **2**, 117. I have not modified the translation though it does introduce minor changes to Duhem's text.

[24] Duhem (1914), **2**, 118.

[25] Duhem (1908), pp. 27–28 (translation, 1969, pp. 25–26); Duhem (1914), **2**, 117–18.

[26] Recent books that have rather provocatively described this 'baggage' are Said (1978) and Bernal (1987).

[27] Some of the antipathy between Renan's 'scientific' worldview and Duhem's 'religious' one can be gleaned from Jaki (1984), passim.

[28] Renan (1883), esp. pp. 14–19.

[29] "Islamic science is in large part the plundered spoils of *decadent* Greek science" [italics added] (Duhem 1914, **2**, 179).

[30] Ibid., **2**, 156–71.

[31] Ibid., **2**, 156–57.

[32] Duhem stated that he got this argument from Friedrich Hultsch, who claimed that the

ordering of letters in Arabic is a "sure indication by which one may recognize a work of Greek origin that has been translated into Arabic" (ibid., **2**, 157). Hultsch, who is principally remembered today for his editions of Greek mathematical and scientific texts, does not seem to have dealt with Arabic works, at least as far as I have been able to ascertain. If he did study Arabic, one would have to conclude that he did not get as far as the alphabet.

[33] It is perhaps worth noting here in passing that Duhem's position cannot be simply characterized as anti-Semitic; for one of the people he admired and who Duhem felt had something of the Greek spirit of Proclus and Ptolemy was Maimonides. In this assessment Duhem parted company with Renan, who had lumped Maimonides with Averroes; cf. Duhem (1908), pp. 37–40 (translation, 1969, pp. 33–35) and Duhem (1914), **2**, 140–41.

[34] On the question of consistency between physics and astronomy in Islam, see Sabra (1978) and Sabra (1984), pp. 133–34, esp. n. 3; cf. Ragep (1982), **1**, 129–89.

[35] Martin (1987), p. 303. Duhem expresses this explicitly in his introduction to STP (1908), pp. 1–2 (translation, 1969, pp. 3–4).

[36] See, for example, Kennedy (1966), pp. 366–67; Neugebauer (1975), **1**, p. 1; **2**, 572, 942; Hartner (1975), p. 9; and Goldstein (1980), p. 142. For different viewpoints, see Sabra (1984), esp. n. 3, pp. 145–46 and Ragep (1987), pp. 330–31.

[37] See, for example, Aristotle, *On the Heavens*, Bk. I, Chap. 3, 270b5–16 where he says "our theory seems to confirm phenomena and to be confirmed by them"; cf. *Metaphysics*, Bk. XII, Chap 7, 1072a20–23. For an extended discussion of this point, see Ragep (1982), **1**, 149–61.

[38] Ragep (1982), **1**, 166–74.

[39] For an elaboration, and the possible effect of this discussion on Copernicus, see Ragep (forthcoming), commentary to II.1[6].6–14.

[40] For a discussion of this relationship and further references, see Kennedy (1966), Hartner (1975), and Sabra (1984), n. 5, p. 146.

[41] Duhem (1914), **2**, 129. The translation was due to Carra de Vaux (1893), whose pejorative characterization of Ṭūsī's efforts no doubt met with Duhem's approval: "Le chapitre dont nous allons donner la traduction suffira peut-être a faire sentir ce que la science musulmane avait de faiblesse, de mesquinerie, quand elle voulait être originale".

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DESCARTES AND SOME PREDECESSORS ON THE DIVINE CONSERVATION OF MOTION*

ABSTRACT. Here I reexamine Duhem's question of the continuity between medieval dynamics and early modern conservation theories. I concentrate on the heavens. For Aristotle, the motions of the heavens are eternally constant (and thus mathematizable) because an eternally constant divine Reason is their mover. Duhem thought that impetus and conservation theories, by extending sublunar mechanics to the heavens, made a divine renewer of motion redundant. By contrast, I show how Descartes derives his law of conservation by extending Aristotelian celestial dynamics to the earth. Descartes argues that motion is intrinsically linear, not circular. But he agrees that motion is mathematically intelligible only where divine Reason moves bodies in a constant and eternal motion. Descartes strips bodies of active powers, leaving God as the only natural mover; thus *both* celestial *and* sublunar motions are constant, and uniformly mathematizable. The law of conservation of the *total quantity* of motion is an attempt to harmonize the constancy derived *a priori* with the phenomenal inconstancy of sublunar motions.

1. INTRODUCTION

The physicists of the seventeenth century destroyed one system of the world and replaced it with another. The old system, which Galileo called Ptolemaic but which is equally Platonist and Aristotelian, posited a finite spherical universe with the heavenly bodies moving around the circumference and the earth at rest in the center. According to this system, the heavenly bodies must have an eternally uniform rotary motion, both because they are eternal and incorruptible by nature, and because they are moved by separate incorporeal movers; in contrast, the sublunar elements are naturally corruptible, and move only a limited distance up or down before they are destroyed and changed into other elements. The new physics of the seventeenth century denied this fundamental contrast between celestial and sublunar things: it posited only a single kind of matter present everywhere in the universe, whose various configurations and motions must produce all the phenomena of nature. Thus it becomes a fundamental problem to find simple and universal laws of motion underlying the phenomena. The seventeenth-century physicists all hold some form of the doctrine

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of the conservation of motion: at a minimum, they hold what has misleadingly come to be called the principle of inertia, that a body in motion tends to remain in motion, and does not naturally come to rest. The apparent tendency of sublunar bodies to slow and stop must be explained, like all other phenomena, through the reciprocal impacts of a system of moving bodies. This 'principle of inertia' distinguishes the philosophers of the seventeenth century from their Aristotelian predecessors, and serves as an emblem for their revolution in physics.

It was Pierre Duhem, in his *Studies on Leonardo da Vinci*,[1] who opened the question: what sources did this revolutionary science of motion have in the older tradition? Duhem found a key part of the answer in studying scholastic discussions of projectile motion. Aristotle had held that no motion could exist at any time without an external mover contiguous to the moved body; he had thus been forced to bizarre expedients to explain how a projectile could continue to move after leaving the hand of the thrower. But Duhem found that in addition to Aristotle's theory there was an alternate account, according to which the thrower imparts a certain *impetus* to the projectile: this impetus remains naturally in the projectile, and is sufficient to account for its continued motion. Duhem traced the doctrine of impetus from the Christian neo-Platonist John Philoponus in the sixth century to the students of the scholastic master John Buridan in the fourteenth century; and he tried to indicate the subsequent stages by which the doctrine of impetus developed, taking on increasing mathematical precision, into the dynamics of Galileo and his contemporaries, founded on the natural preservation of motion.

Duhem's work has been challenged by Anneliese Maier. While she agrees that impetus theory provided the historical point of departure for the discovery of the law of inertia, she argues that Duhem has read the scholastic sources too "charitably", and so exaggerated the agreement between impetus theory and seventeenth-century mechanics. Maier argues convincingly that Duhem misread the views of Buridan and his school on the permanence of impetus in the projectile: while Buridan entertains the hypothesis that a celestial impetus might last forever, he and all scholastics agree that any impetus in a sublunar body would perish through the natural resistance of matter. Maier thinks that this indicates a fundamental difference between the scholastic conception of impetus and the modern conception of inertial motion; impetus in a kind of energy which the thrower deposits in the projectile, and which converts itself into motion at each instant until it is depleted; on the modern conception, by contrast, motion is a naturally persisting state of the body and does not need to be explained by reference to any moving force. Thus Maier argues, against Duhem, that the scholastic theory of impetus needed more than just mathematical precision to become modern dynamics; a further conceptual revolution was needed to produce the concept that the motion imparted by a finite agent to a sublunar body is intrinsically permanent.[2]

The history of the laws of motion is extremely complex. We have no reason to suppose that there is a single linear path which leads from Aristotelianism through impetus theory to the principle of inertia; on the contrary, it is clear that different seventeenth-century physicists held different and incompatible laws of conservation of motion, and that these laws did not all share the same historical genealogy. Here I propose to test Duhem's claim of continuity, and Maier's counterclaim of discontinuity, by looking at the particular case of *Descartes's* law of the conservation of motion. Descartes is an interesting case, both because he holds a strong and precise (though false) principle of conservation, and because he justifies this principle by an argument from natural theology. This argument might seem at first to be an *ad hoc* justification, but I will show that it has deep roots in older Aristotelian and Platonist philosophy. I will thus trace one of the many paths which led from the medieval discussions of motion to the modern consensus that motion is conserved; and I will hope to shed light on the meaning of one seventeenth-century version of the law of conservation.

The path which I will indicate from medieval to modern physics is not quite the same as the path which Duhem had suggested. Maier proves, against Duhem, that the impetus-theorists did not think of motion as a permanent being which could remain stable without the continued influence of a moving cause; she therefore concludes that they did not possess the principle of inertia. But, as we will see, Descartes also did not think of motion as stable in this sense; thus Maier's argument cannot be sufficient to demonstrate a break between medieval and early modern discussions of motion. Without in any way diminishing the importance of the modern abrogation of the .... distinction between heaven and earth, we can uphold Duhem's insight that (at least some of) the theories of inertial motion continued a scholastic discussion; but for Descartes, at least, it was not the concept of a permanent impetus in sublunar projectiles that was his starting point. My results on the particular case of Descartes will thus confirm Duhem's main contention against Maier, while disconfirming some of his subsidiary theses.

2. ARISTOTLE AND SOME SUCCESSORS ON CONSTANT ETERNAL MOVERS

In examining the medieval antecedents of Descartes's law of conservation of motion, I want to bear in mind Maier's remark that "the scholastic analogue to inertial motion", the only "constant velocity motion occurring in the absence of resistance", is celestial motion (Maier 1982, p. 99). In this section I will indicate some themes from the history of Aristotelian and Platonist thought about the nature and causes of celestial motion; in the next section I will turn to Descartes's discussions of the law of conservation. I will try to show how Descartes's theological argument for the conservation of all motion, celestial or terrestrial, continues and transforms the Platonist and Aristotelian discussion of celestial motion. I will emphasize the ways in which the older tradition gave Descartes a point of departure for thinking about the laws of motion and the intelligibility of nature, but I will also try to bring out the depth of the disagreement between Cartesian and Aristotelian–Platonist philosophy.

All Aristotelians, and almost all Platonists,[3] claim that the motions of the heavenly spheres are constant and eternal. They confirm this claim by reasoning from the effects, the apparent positions of the planets observed by the astronomers, but they think that the true certainty of the claim lies in the *causes* which necessitate the uniformity of celestial motion. Since Aristotelians and Platonists (like Descartes) assume that all motion requires a mover, the cause of the uniformity of celestial motion must lie in the nature of the movers: a mover which itself varies will produce a variable motion, but a mover which remains constant in itself and always moves a body will produce an always constant motion in that body.

But what is this constant mover which produces the constant motion of the heavens? It is surprisingly difficult for the Aristotelian to answer this basic question, because in different texts Aristotle gives at least two and perhaps three different answers, which may or may not be consistent with each other (some modern commentators believe that they can trace a development in Aristotle's views). In Books I and II of the *De Caelo* Aristotle argues that the heavens are moved circularly around the center of the world by their own nature, in the same way that earth is moved toward the centre and fire away from the centre. But in *Metaphysics* XII (and in other works) Aristotle maintains that the heavens are moved by one or more incorporeal movers separate from the heavens themselves: in the *Metaphysics* the mover at least of the outermost sphere is described as *nous* or Reason.[4] *Nous* moves its sphere only as a final or exemplar cause, by being the good which the sphere desires to attain or imitate by its motion. This seems to imply that the efficient cause of the sphere's motion must be a soul which animates the sphere and desires its good, and so produces a voluntary motion in its body. Thus the basic problem of celestial dynamics for later Aristotelians is to make simultaneously intelligible the statements that nature is the cause of celestial motion, that soul is the cause of celestial motion, and that *nous* is the cause of celestial motion.

This picture of the heavens does not at first seem promising for scientific progress. The heavens are pictured as divine living beings, moving in desire of further divine powers. Duhem regarded these Aristotelian Movers as pagan, animistic, and unscientific, and he thought that John Buridan had made decisive progress by proposing to bury them.[5] Duhem was not all wrong: there are certainly elements of fantasy in the doctrine of the separate movers. But I will try to show that beneath the fantasy this doctrine contained important philosophical ideas, which could be useful even for a philosopher who, like Descartes, had rejected the system of the spheres and the priority of circular motion.

I will try to show this by elucidating some views of the problem of celestial dynamics that were current within the Aristotelian and Platonist tradition. First I will review some of the main data of the problem as posed by the assertions of Aristotle (and also of Plato); then I will show how some major figures of the later Aristotelian and Platonist tradition, in harmonizing Aristotle's different statements with each other (and often also with Plato or with revealed scriptures), were forced to develop Aristotle's theory further than Aristotle himself had done, yielding results of lasting philosophical interest.[6]

I may begin at the top, with the *nous* which is for Aristotle the highest cause of the celestial motions. Some translators render *nous* as 'mind' or 'intellect', but this is often inadequate. The word *nous* sometimes means the act, sometimes the habit, and sometimes the faculty, of intellectual perception; but often (as in the common phrases *noun echein* or *noun kektesthai*, to be reasonable; cp. French *avoir raison*), it means that which we possess or share in when we do or think something rationally. Following Ralph Hackforth, I will translate *nous* in this sense as 'Reason'.[7] Reason is not a 'mind', in the sense of a rational soul: it is what souls participate in, in order to think rationally or rightly. If we are to say that Reason 'thinks', it does not think in the same way that rational souls think, but rather by being the standard by which thought is measured, according to which thought can be called rational or irrational, right or wrong. When St. Augustine wishes to find a Latin equivalent for the Greek word *nous* as used in Plotinus, he sometimes says 'intellectus' but more usually 'veritas', truth: I will not use this rendering, but it is helpful to recall that it is possible.[8]

Plato says in the *Philebus* (28C) that "all the wise agree that *nous* is king of heaven and earth"; and he means, not a rational soul, but Reason itself. There is an objective rational order in bodies, especially in the heavenly bodies, which can be grasped by the rational faculty in us; Plato believes that we can explain the existence of this order only by supposing that there is a separate *nous*, a Reason-itself, and that this Reason has the power to impose at least some degree of rational order on bodies. Plato fills out this account in the *Timaeus* with a hypothetical story of how Reason, as the 'demiurge' or craftsman of the physical world, might impose sufficient order on an originally chaotic matter to produce something like the world we now inhabit. Aristotle too asserts, in *De Anima* III, 5, that there is a separate *nous* or Reason which is the source of intellectual knowledge to the soul, and he follows Plato in asserting that *nous* is king, not directly of the earth, but at least of the heavens. Sublunar motions are not rational or constant enough to be the object of a mathematical science; but celestial motions are, and they must therefore somehow proceed from Reason. Aristotle's disagreement with Plato concerns the means by which Reason communicates rational order to bodies.

Plato's demiurge sometimes resorts to violent means in rationalizing bodies, imposing numerical constraints on a resisting matter. Aristotle, in rejecting these means, is continuing Plato's own critique of Anaxagoras. Anaxagoras too had claimed to derive the world from *nous*, but Plato charges that Anaxagoras' actual explanations relied on merely mechanical constraints, and not on rationality and the best order of things. Aristotle continues this critique by insisting that Reason cannot cause order in things by forcing them to move to their proper positions, but only by being the good or the model which the things aspire to or imitate.

But Plato had already offered a less violent account of how rational order descends from *nous* to bodies, and this Aristotle finds more acceptable. *Nous* cannot directly move bodies, because bodies cannot directly participate in Reason; but souls can participate in Reason, and souls can move their bodies. Plato thinks that souls have an innate internal motion, which becomes rational and orderly when the soul participates in Reason. When the soul communicates this motion to its body, it regularizes and rationalizes the body. Aristotle rejects Plato's doctrine that the soul moves itself, but he accepts that the soul moves the body. Aristotle can therefore give an essentially Platonic answer to the question of the sources of rationality and constancy to the celestial motions: the ultimate source is Reason itself, but this rationality can only communicate itself to the heaven by being first received in the rational soul which immediately moves the heavens. Once this soul grasps Reason as its goal or model, it will not pass back and forth between right and wrong thoughts, but will remain eternally in a constant state of thought and will, and therefore eternally produce a constant motion in its body.

Thus far it is not so difficult to harmonize Aristotle with himself and (up to a point) with Plato. It is a greater challenge to harmonize *Metaphysics* XII with Aristotle's assertion in the *De Caelo* that the heavenly bodies move around the center by their own nature. In *De Caelo* II, 1, Aristotle explicitly rejects the contention that the heavens remain in their circles because of a "psychic constraint" (*ananke empsuchos*). Aristotle's language here echoes Plato's critique of Anaxagoras (like Plato at *Phaedo* 99C, he describes his opponents as seeking a new Atlas to keep the heavens up), but it is clear to any unbiased reader that Aristotle's prime target is Plato. Plato had said (*Timaeus* 36E) that the soul which turns the heaven enjoys an "unceasing [*apaustos*] and intelligent [*emphron*] life for all time". Aristotle too says here that the motion of the heaven is "unceasing [*apaustos*] for infinite time", but he insists that if a soul is needed to constrain a body "which is naturally moved in some other way" (as it would be if, as Plato says, the heavens are made mostly of fire), then this soul must devote itself to violent effort, and will have no "intelligent [*emphron*] leisure"; its ceaselessness will not be a blessing but a curse.

Aristotle's conclusion is that the heaven is moved, not by the constraint or necessitation of a soul, but by its nature. It might seem difficult to harmonize this, not just with Plato, but even with *Metaphysics* XII. But, as all harmonistic commentators note, Aristotle does not deny in the *De Caelo* that the heavens are *moved* by souls, but only that they are *constrained* (*anankazesthai*) by souls. Perhaps they might be moved by souls without violence or constraint, if it is also true that they are moved in circles by their natures. Different commentators, in different ways, try to harmonize the *De Caelo* with the *Metaphysics* by bringing together the statements that *nous* and soul are causes of celestial motion and also that nature is a cause of celestial motion. As we will see, they tend through time to give greater emphasis to the separate movers, and to devalue nature as a cause of celestial motion.

We must first point out that, for an Aristotelian, the nature of a body is not the same as the body: it is the form immanent within the body. To say that a body is moved naturally is to say that it is moved by its form. Since Aristotle holds that a soul is the form of a living body, the obvious way to reconcile the *De Caelo* with the *Metaphysics* is to identify the natures of the heavens with their souls. This is the solution of Alexander of Aphrodisias, but it is rejected by the later Greek commentators, who are trying to reconcile Aristotle with Platonism. The harmonizers of Plato and Aristotle agree that 'natures' are forms which are immanent within bodies and are therefore destroyed with their bodies. If rational souls were natures then they would be mortal at least in principle (although those souls inhabiting immortal bodies would never actually die), and this is unacceptable.[9]

Consequently, such philosophers as Proclus and Simplicius must posit not two but three distinct causes, *nous*, soul, and nature, all working in harmony to produce the motion of the spheres. According to Proclus's scheme, *nous* is an unmoved mover, soul is a self-moved mover, nature or immanent form is a moved mover, and body is a moved nonmover (see Proclus 1968, p. 60 and elsewhere). Proclus goes beyond what we have already seen mainly in his account of the different ways in which soul and nature are causes of motion to bodies. While natures or other immanent forms are said to be the immediate movers of bodies, in fact soul becomes the principal mover, and nature moves bodies only in a peculiar sense. Natures and other immanent forms receive their being from souls, and they can move bodies only when they themselves are first moved by souls: thus they merely communicate motion from souls to bodies. Invoking a distinction from Aristotle (*Physics* VIII, 4), Proclus and his followers say that nature is a principle of being-moved (*arche tou kineisthai*), while only soul is a principle of moving-something (*arche tou kinein*). Nature is a principle of being-moved by endowing the body with an *epitedeiotes*, a preparedness or disposition to be moved; but no actual motion occurs unless some soul initiates it. The heavens, in particular, have a nature which disposes them to be moved circularly (and Proclus endeavours to show that Plato as well as Aristotle believed this); if they did not possess this nature, then when a soul moved them circularly in would move them violently and contrary to nature, as when a human being throws a stone upward; such violent motion could not be regular or eternal, as is the motion of the heavens. This satisfies Aristotle's concerns in the *De Caelo*, while diminishing the role of nature, and preserving soul as the principal cause of motion. Simplicus summarizes the harmonious causality of *nous*, soul, and nature as follows:

> If someone asks which local motion of the heaven comes from nature and which from soul, we say that soul, through the mediation of nature, makes the heaven move [*kineisthai*] in a circle, and that it is one and the same motion. But it has from nature the connatural and unforced disposition [*epitedeiotes*], according to its very form, for being moved [*kineisthai*]; while from soul it has the actuality [*energeia*] of motion towards which it was disposed by nature. So, too, it has from *nous* its turning always and in the same way and according to the same and about the same and in the same [expanding on *Timaeus* 34A]. For by these things, under the leadership of *nous*, the psychic motion which is impressed through nature in body is constituted, and stabilized in the likeness of the activity [*energeia*] of *nous*. Whence also that divine man, having asked why the heaven moves in a circle, says that it is because it imitates *nous*.[10]

The neo-Platonic doctrine that natures, and more transient immanent forms, are intermediate movers, the agents of soul in bodies, is the background for scholastic discussions of *impetus* or *vis impressa*. I will not try to summarize these discussions here: but I want to cite one particular text, from Avicenna in the early eleventh century, on the causes of celestial motion.

Avicenna occupies a key juncture in the history of reflection about the causes of motion. For the Latin scholastics Avicenna becomes, after Averroes, the most authoritative interpreter of Aristotelian philosophy. But Avicenna is a rather loose Aristotelian, as the Greek Platonizing commentators had been: while he is not fully committed to harmonizing Aristotle with Plato, often (sometimes for religious reasons) he preserves Platonizing interpretations of particular Aristotelian doctrines. In particular, he takes up the neo-Platonist interpretation of Aristotle's doctrine of the causes of celestial motion, and he transmits to the West his revised version of this doctrine. Looking at Avicenna allows us to see how the theory of impetus, especially in the celestial case, develops out of the Aristotelian and Platonist concerns we have been discussing. The text of Avicenna I will cite is helpful because it has close echoes with both Simplicius and Buridan, and also with Descartes: Avicenna is certainly one of the key links in the development and transmission of impetus theory (although I make no attempt here to trace all the links in the chain), and he also illustrates very clearly the way a whole tradition thought about how incorporeal movers move bodies.[11]

Avicenna modifies the neo-Platonist doctrine of nature, soul and *nous* in a number of ways (most notoriously by accepting a hierarchy of Reasons shared in by different levels of souls), but his most important modification for our purposes is in his conception of nature. Avicenna does not accept the full neo-Platonist hierarchy of being: he asserts that all souls are the forms of their bodies, and he denies that there can be two substantial forms, soul and nature, within the same body. For this reason, Avicenna goes even further than Proclus and Simplicius in devaluing the role of nature as a cause of celestial motion: he will not identify the natures of the heavens with their souls, but he also cannot accept them as substantial forms inferior to souls, so that they seem to be squeezed out of his system. And yet he must preserve the doctrine of the *De Caelo* that the heavens are essentially different from sublunar things and that they rotate by their nature.

Avicenna devotes a chapter of the *Shifa'* to the proposition "that the proximate mover of the heavens is neither a nature nor a *nous* but a soul, and that the more remote principle is a *nous*".[12] He begins by denying that the rotation of the heavens is "natural", in the sense of proceeding from a nature; he then adds that the motion is "by nature" in a looser sense, in that "its presence in its body is not contrary to the determination of any other nature in its body: for the thing which moves it, even though it is not a natural power, is something natural to this body and not alien to it; it is as if it were its nature".

But Avicenna wishes to find a more positive sense in which the rotation proceeds from a nature. He solves the problem, in essentially neo-Platonic terms, as follows:

Furthermore, every power moves only by the mediation of some inclination [*mayl*], and the inclination is the thing [*ma'na*] which is perceived in the moved body: even if it is forced to rest this inclination will still be perceived in it, resisting the obstacle and seeking motion even while it is at rest. This is doubtless something other than the motion, and other than the moving power, for the moving power still exists when it has completed the motion, and the inclination does not. Similarly, too, in the case of the first motion, its mover does not cease to generate inclination after inclination in its body. Nothing prevents this inclination from being called a nature: for it is not a soul, nor is it from without, nor does it have will or choice, nor can it not move, or move in other than a definite direction; nor, further, it is contrary to the determination of an alien nature in this body. And if this thing [*ma'na*] is called a nature, then you may say that the heaven is moved by nature; but its nature is an emanation from soul, which is multiplied in accordance with the soul's activity of thinking. And it is already clear that the principle of the sphere's motion is not a nature [accepting a variant reading]; and it is already clear that it is not violence; it is therefore doubtless from will.

It is this theory which, in one or another variant, is accepted by Buridan and his school in the fourteenth century. As Duhem noted, Buridan went beyond earlier thinkers in proposing an *imaginatio* according to which all incorporeal movers inferior to God would be eliminated, but for Buridan this was only an *imaginatio*, while the theory of moving *intelligentiae* was the truth.[13] The fourteenth-century Parisians, then, when beyond Avicenna chiefly in the possibilities they considered, not in what they really believed. But even their *imaginationes* were, in the fourteenth-century context, a natural step beyond Avicenna's position. Duhem rightly stressed the importance for these physicists of theological voluntarism: it was open to them (as it had not been to Avicenna) to consider scenarios in which God would do by himself what he is normally thought to do through secondary causes. But it was easy and painless for them to modify Avicenna's scheme in this way, because he had already effectively eliminated the heavenly bodies and their natures as causes of celestial motion. The real mover for Avicenna is the soul, which looks to Reason, then wills the body to move and so produces in it an inclination disposing it to motion: the body itself cooperates only negatively, by not having the form of a sublunar body which would resist a rational circular motion. If we abolish soul and Reason as separate entities, and have God step in to fill their roles, we may retain the basic structure of the doctrine of celestial motion. If soul and Reason are immutable enough to guarantee the rationality and constancy of celestial motion, then *a fortiori* God provides the same guarantee. What has remained is the doctrine that an immutable incorporeal mover, which has rationality intrinsically and of itself, will produce an eternally constant motion which can be the object of a mathematical science. As we will see, this is also the fundamental doctrine of Descartes's dynamics.

3. DESCARTES ON THE DIVINE CONSERVATION OF MOTION

Descartes is not a part of the Aristotelian and Platonist tradition I have been describing. He rejects any essential distinction between celestial and sublunar bodies; indeed, he rejects immanent forms, the finite universe, the celestial spheres, and the priority of circular motion. This is well-known and unsurprising. What is more surprising is the extent to which Descartes is able to use themes from the older tradition in constructing his new physics. Descartes draws his metaphysics (on which his physics is to be based) largely from the Platonizing doctrines of St. Augustine, and he is trying to persuade readers nurtured on scholastic Aristotelianism. It is thus natural for Descartes to call up themes from the Aristotelian and Platonist discussion of the causes of motion; we will see how he uses and transforms them.[14]

Descartes always sets out his philosophy in a definite order, beginning with metaphysics and then turning to physics. Metaphysics concerns God and the soul; physics concerns bodies, their essence and existence and their various motions. Descartes's metaphysics follows the discipline of ascending from the soul of God which Descartes had taken from Augustine, Augustine from Plotinus, and Plotinus, ultimately, from Aristotle's *De Anima* III, 5: first we consider ourselves as rational souls, as potential knowers, and then we raise our sights to consider God as Reason or Truth, as the source of our rationality and the standard by which we judge that we are thinking rightly. Descartes's physics begins with this knowledge of God, and descends to infer a knowledge of the creatures which proceed from him. Descartes uses premises about God to argue that the essence of body consists only in extension, then that a world of bodies actually exists; then, finally, that bodies obey certain universal laws of motion, from which particular physical phenomena are derived.

We may schematically contrast the way physics and metaphysics fit together for Descartes with the way they fit together for Aristotle. Starting from two different parts of his philosophy, the psychology and the celestial physics, Aristotle leads us up to contemplate the divine Reason: in *De Anima* III he takes us from the potential reason in ourselves to Reason absolutely, while in *Physics* VIII and *Metaphysics* XII he takes us from the constancy of the primary celestial motion to immutable Reason as its source. When Descartes ascends, in the *Meditations* or in Part One of the *Principles*, from the human mind to God, he is following the path *De Anima* III (at least as it is interpreted by Plotinus, and taken over by Augustine); but when Descartes descends, in Part Two of the *Principles*, from God to bodily motion, he is reversing the path of *Metaphysics* XII, arguing from the immutability of God to the constancy of the motion which proceeds from him.

This attempt to reverse Aristotle's order, descending from God to bodies, has important implications for Descartes's philosophy. An Aristotelian may simply observe by watching the skies that the celestial motions are constant; or again, he may begin with the fact of inconstant motion and argue that it must be contained and measured by a constant motion. In either case, he discovers God only as the cause of the primary constant motion and not of all motion. Descartes, by contrast, has sought to think away all experience of the physical world in constructing his knowledge of God, and he has no grounds for restricting God's causality to one region of space rather than another. Abstracting from all evidence of the senses, Descartes finds the essence of body to be a uniform spatial extension, lacking any outermost limit: he is therefore led, together with such contemporaries as Galileo, to abolish the distinction between celestial and sublunar physics. Descartes concludes that not only celestial but also sublunar motions are constant, because they all proceed equally from God: they are therefore all equally objects of mathematical physics.

As Duhem would surely have suggested, Descartes's divergence from the traditional Aristotelian view is rooted (at least in part) in a Christian rejection of pagan limitations on the power of God. But it is insufficient to say that Descartes refuses (as any Christian would) to restrict God's power to only the outermost portion of the universe. This would still be compatible with a broad acceptance of the Aristotelian system. For any even moderately Platonizing Aristotelian, there is nothing in the nature of divine Reason which prevents it from ruling over the whole world of bodies; the obstacle comes rather from the nature of bodies, which are not all capable of receiving the divine ordering without resistance and distortion. On an Aristotelian view, the natures of sublunar bodies incline them to move a finite distance and then stop, if they are not first corrupted; such mutable things can receive order only to the extent of observing a rough periodicity in their transformations. Only the heavens receive constant circular motion without resistance, because (as Simplicius says), only they have a natural disposition to receive it, or even more negatively because (as Avicenna says) only they have no other nature which could resist it. Thus for Descartes to assert, against such Aristotelians as these, that God produces a constant motion in sublunar as well as celestial bodies, it is not enough for him to modify their conception of God; he must modify their conception of body as well.

We may contrast Descartes's conception of corporeal nature with Avicenna's conception. Avicenna denies that the heavenly bodies have an intrinsic source of natural motion and concludes that they do not resist the constant motion. Descartes, however, denies that any bodies have an instrinsic source of natural motion and concludes that no bodies resist the constant motion. This follows from Descartes's conception of body as pure extension, which is designed precisely to strip bodies of any natures, immanent forms, or active powers. Only human and angelic minds, exceptional beings within the natural world, remain to counteract the divine determination of bodies toward a constant motion.

This elimination of natures from bodies depends on a theological voluntarism much more radical than Buridan's: it is connected with Descartes's doctrine of the creation of the eternal truths. In a famous passage from a letter to Mersenne, Descartes declares that the eternal truths "have been established by God and depend on him entirely, as much as do all other creatures" (Descartes 1964, vol. I, p. 145):

> Indeed, it is speaking of God as of a Jupiter or Saturn, and subjecting him to Styx and the fates, to say that these truths are independent of him. I beg you, be bold to assert and proclaim everywhere that it is God who has established these laws in nature, as a king establishes laws in his kingdom. (ibid.)

Descartes is here rejecting the Aristotelian view that there is a radical plurality of essences or natures which make each body the kind of thing it is, and so govern its behavior: rather, all bodies are governed by universal laws, and these laws are immutable because God is immutable. Descartes's voluntarism is not an irrationalism: God's laws are not tyrannical whims, but rational truths, proceeding from a God who is the source of truth and rationality. We might therefore derive these laws from a knowledge of God's nature, reversing the procedure of *Metaphysics* XII.

Descartes is applying this principle in Part Two of the *Principles of Philosophy*, art. 36 and following, where he sets out to derive the laws of motion.[15] Descartes begins by saying that God is the universal and primary cause of all motion, and this is in itself not controversial; but Descartes then goes on to assert that the secondary causes of the particular motions of bodies are not bodies themselves, nor forms immanent in bodies, but certain laws proceeding from God. When God preserves and governs the world through his ordinary concourse (as opposed to miracles), he "conserves all that matter in [or by] the same *modus* and the same *ratio* in which he previously created it" (art. 36), or "by the same action and with the same laws with which he created it" (art. 42). In both of these parallel passages (and a third, earlier in art. 36), the conclusion is immediately drawn that, in conserving the totality of matter, God "also always conserves the same amount of motion in it" (art. 36). The argument seems to be that motion is constant because it proceeds from God, or more precisely because it proceeds from the *modus*, *ratio*, action, or laws by which God governs the totality of matter: for "we understand the perfection to be in God, not only that he is immutable in himself, but also that he operates in a supremely constant and immutable *modus*" (art. 36). Because bodies have no active natures of their own, all their motions are governed solely by this constant *modus* or *ratio* of God's operation, and therefore all their motions are constant. The only possible exception would be for motions proceeding from human and angelic minds, and Descartes explicitly refuses to explore this exceptional case (end of art. 40), reserving it for a treatise *De Homine*, which he was never to write.

All this seems too easy. Three questions arise: what is this *modus* or *ratio*, or what are these laws? How do they decide whether bodies move in straight lines or in circles? Finally, how will Descartes meet the obvious Aristotelian objection, that the sublunar motions we ordinarily experience do not remain constant, but change and stop?

The first question has two obvious answers, both of which I find unsatisfactory. The first is that there is no entity which is this *modus*: the *modus* is just the manner of God's operation, and manners should not be reified. I think this answer is wrong: it cannot account for the parallel passage in art. 42, which has *actio* and *leges* instead of *modus* and *ratio*.[16] Descartes recognizes a special ontological status for non-subsistent entities, which are not strictly things but are also nothing: sometimes he calls these entities eternal truths, but *ratio* and *lex* are also among his favorite terms for this kind of being. Thus Descartes says (at Descartes 1964, vol. VII, p. 435) that there is nothing which does not depend on God: "not just nothing subsistent, but also no order, no law, and no *ratio* of truth or goodness". Orders, laws, and *rationes* of truth or goodness are not subsistent things, but they are norms or standards by which things are measured and which serve to guide or channel God's operation among the things. These laws do not subsist in God, as accidents in his substance; rather, they proceed from God, both toward the minds which know them and toward the bodies which obey them. In particular, the *ratio* or *lex* of God's conservation of matter proceeds from God towards the bodies, and, being immutable, always produces in them the same amount of motion.

We can now see what is wrong with the second obvious answer: namely, that the *ratio* and the laws are simply the three 'rules or laws of nature' which Descartes deduces, in art. 37 and following, from the general principle of divine immutability. Descartes says that the rules or laws of nature are "the secondary and particular causes of the various motions which we observe in individual bodies" (art. 37). But Descartes does not intend to suggest that propositions are causes, though he may speak loosely as if they were. Laws or *rationes* or eternal truths may be causes, but eternal truths are not ultimately propositions: they are the real essences that are the subject matter of the propositions and make the propositions true.[17] The laws which cause motion are not Descartes's three propositions, but the reality which those three propositions describe.

I think the true answer is that Descartes conceives the law by which God moves bodies to be something very much like Avicenna's "inclination to motion", which proceeds to the heavenly bodies from their separate movers, and which substitutes for an active nature in the bodies themselves. In various passages Descartes speaks about such an inclination to motion, in terms similar to Avicenna's. Thus in a letter to Henry More, Descartes agrees that "matter left freely to itself, and receiving no impulse from elsewhere", (Descartes 1964, vol. V, p. 404) will remain at rest; but in fact "it is impelled by God, who conserves in it as much motion or translation as he put in it from the beginning". The impulse to motion does not come from the nature of matter, but, Descartes immediately adds, the motion is "no more violent to matter than rest is", because bodies have no contrary action or positive force with which to resist it; motion proceeding from the divine impulse can therefore be said to be natural to matter.

If we understand this similarity between Descartes and the older philosophical tradition (of which Avicenna is a representative), we can also understand the divergence which comes in their answers to our second question: how do the laws (whatever they are) determine whether bodies should move in straight lines or in circles?

Descartes believes that the divine impulse to motion leads bodies to move in straight lines, because only rectilinear motion is rational motion:

> One must say that God alone is the author of all the motions which there are in the world, inasmuch as they *are*, and inasmuch as they are straight, but that it is the various dispositions of matter which render them irregular and curved; just as the theologians teach us that God is also the author of all our actions, inasmuch as they *are*, and inasmuch as they have some goodness, but that it is the various dispositions of our wills which can render them vicious. (Descartes 1964, vol. XI, pp. 46–7)

Now the older tradition had maintained, in very similar terms, that only rational motions proceeded from divine Reason, and that deviations from rational motion were due to the incapacity of matter; but Aristotelians and Platonists identify the rational motion as circular motion.

Descartes, unlike Avicenna, unlike Buridan, and unlike Galileo, cannot possibly regard circular motion as the rational motion; this is ruled out by his abstract consideration of body as bare geometrical extension, without limit or center. Descartes agrees with Avicenna that the will of the divine mover "does not cease to generate inclination after inclination in its body" (Avicenna, as quoted above); these inclinations last only for a moment and pass immediately out of existence, but are continually replaced by equivalent inclinations, because the will of the divine mover is constant. Because of the "immutability and simiplicity of the operation through which God conserves motion in matter" (art. 39), God always conserves motion "precisely as it is in that same moment of time in which he conserves it, having no regard [*nulla habita ratione*] to what perhaps it was a little while before". Avicenna would say that the conservation of circular motion satisfies this condition: the sun's mover always gives the sun an inclination to move westward around the earth, and it always reproduces this inclination in the same direction, westward, without referring to the sun's previous history. But we can say that this inclination is in 'the same direction' from moment to moment only by referring it to the center of rotation, which is (for Avicenna) the center of the universe. For Descartes, however, the universe has no center, and any reference to a center of rotation is extrinsic to the inclination to motion. Thus

> of all motions, it is only the straight which is entirely simple, and whose whole nature is comprehended in an instant: for to conceive it, it is enough to think that a body is in the act of moving in a certain direction, which occurs in each of the instants which can be picked out while it is in motion. Whereas, to conceive circular motion, or any other there may be, one must consider at least two of its instants, or rather two of its parts, and the relation between them. (Descartes 1964, vol. XI, pp. 44–45)

Descartes knows that no actual motion, rectilinear or otherwise, can take place in an instant; but he insists that "all that is required to produce [rectilinear motion] is found in bodies in each instant which can be picked out while they are in motion, but not all that is required to produce circular motion" (ibid.). In terms of a modern mathematical understanding of motion, this is nonsense: there is no sense in which the first derivative of a body's position is 'comprehended in an instant', but the second derivative is not. But in terms of a theory of inclination or impetus, Descartes's assertion makes excellent sense: at each instant what God creates in a body is a bare inclination to move with a certain speed in a certain direction, not rectilinearly or circularly or in any other determinate way; but at each instant God renews in this body an inclination to move with the same speed and in the same direction, and so he produces a uniform rectilinear motion.

So far, we have avoided the greatest and most obvious problem for Descartes's theory of the causes of motion: on Descartes's account, God produces only rational motion, which is uniform rectilinear motion. But God is the only source of motion, excepting the case of finite minds: how then is it that motions other than uniform rectilinear motion are in fact observed, both in the heavens and on the earth?

The way Descartes deals with the difficulty reveals perhaps his most profound divergence from the old Platonic–Aristotelian tradition. The older philosophers held that circular motion was the only rational motion and that this motion was perfectly realized in the heavens: since the phenomenal motions of the heavenly bodies are not perfectly circular, they encouraged the construction of astronomical hypotheses to resolve the phenomenal motions into perfect circles. But since rational motion does not appear to be fully realized beneath the moon, they concluded that sublunar bodies were not fully rational, that their natures prevented them from fully receiving the divine impulse toward order; thus while sublunar bodies obey an approximate rationality, we cannot construct an exact science of their motions, and there is no point in trying. Descartes refuses to accept this solution: for him bodies are purely intelligible in their essence, and he will not consign any portion of the physical world to the realm of mere semi-intelligible phenomena. The world is highly complex, but it must be, in principle, fully intelligible. Thus for Descartes the problem of sublunar physics is the same as the problem of celestial physics: to explain how simple laws or inclinations to rational motion, compounded with one another in complex configurations, yield the apparently disorderly phenomena which we observe.

Now we have already quoted Descartes as saying, in very traditional language, that "the various dispositions of matter" can distort the impulse to motion into crooked and irregular paths, just as our will can distort the divine impulse towards the good. But by Descartes's own view the analogy cannot be perfect: minds have the power to resist the divine determination, and bodies do not. The dispositions of matter can render motion crooked and irregular only by complicating it, by bringing two impulses to motion together in the same part of matter, forcing them to be resolved into a single resultant motion which will no longer be uniform.

This happens when bodies collide, as a plenum of bodies in rectilinear motion inevitably must, unless the whole universe is moving with a single rigid motion. For the Aristotelians, this had been an argument that the primary motion must be circular, since no rectilinear motion could continue indefinitely. Descartes agrees that actual motion must go in circles, but he insists that all motion is intrinsically rectilinear and that a moving body tends to continue in a straight line unless some other body prevents it. Thus circular motion, even in the heavens, cannot simply be posited: it must be explained as the result of collisions of bodies moving in straight lines.

Descartes insists that collisions are not violations of the law of conservation of motion; they are just circumstances dictating that this law must be observed in a complicated way. Motion, or rather the impulse to motion continually proceeding from God, remains constant: but this constant power "now applies itself to some and now to other parts of matter" (Descartes 1964, vol. V, p. 405), so that in a collision one body can transfer some of its motive force to another. Descartes attempts to calculate how much motion bodies will lose and gain in collisions, and so discover universal rules of impact. Descartes's way of calculating the quantity of motion is crude and unargued, and all his subsequent conclusions are wrong. He is struggling, with inadequate data and inadequate conceptual equipment, to explain how a simple constant force, governed by a universal law of conservation, can produce phenomena of indefinite complexity, so that "even this continual mutation in creatures is an argument of the immutability of God" (art. 42). If Descartes had little success in explaining phenomena through simple laws, he at least succeeded in laying down a challenge for other physicists.

Now, in setting out Descartes's thought on the causes of motion, and on the role of God in establishing the law of conservation, it would have been possible to start at the end, with Descartes's law of the conservation of the total quantity of motion in the universe. But this would give a distorted picture of the way Descartes thinks about conservation. Descartes's fundamental idea, shared with Aristotelians and Platonists, is that God communicates his constancy to the world by continually reproducing a constant impulse to motion in a given body. Because Descartes makes this conservation universal, not restricting it to bodies with apparently uniform motion, he is forced to invoke the transference of motion in collisions in order to save the phenomena.

In the process, Descartes speaks of an amount or quantity of motion, and formulates what looks like a modern conservation law, with a specified "conserved quantity". But this is just an accidental by-product of Descartes's great innovation, which is to extend to all the parts of a uniform space the divine conservation of motion formerly restricted to the heavenly bodies.

My report on Descartes generally supports Duhem's thesis of continuity between medieval and early modern discussions of the causes of motion. At a sufficiently abstract level, Descartes's theory of the causes of motion is very close to the older theory represented by Avicenna; the great difference, of course, is Descartes's conception of body as extension, which abolishes the finite universe and the privileged status of the heavens. The study of Descartes, again, confirms Duhem's view of the importance of medieval discussions of impetus: but we must consider impetus theory in a broad way, not as a particular explanation of the motion of projectiles, but as the Platonist (or Platonizing Aristotelian) discussion of the relation between separate and immanent forms as causes of motion. The doctrine of a natural permanence of impetus, which Duhem claimed to find and Maier claimed not to find in various fourteenth-century figures, proves not to be important for Descartes: conservation of motion is consistent with the doctrine that all motion always proceeds from a mover, as long as that mover is God.

Perhaps our most surprising result is the continued vitality, for Descartes, of the Aristotelian and Platonist doctrine of separate incorporeal movers. In a way this would have pleased Duhem, for Descartes is certainly dismissing any angelic movers, and letting the will of God take over their functions. But Duhem suggests that "the instant when the stars stopped being perceived as moved by divine beings" marked the divide between ancient and modern science (Duhem 1913, p. ix). This division does not work, at least not for the one peculiar strand of early modern science which is Cartesian physics. The less naturalistic the Aristotelian account of celestial motion became, the closer it came to Descartes. Descartes could not have used an account of bodies as naturally disposed to motion by their own substantial form; he could and did use an account of bodies as moved by a separate divine Reason. Precisely because the separate movers were outside the natural order, they could survive the destruction of the old world picture, and play a constructive role in one variety of the new physics.

NOTES

* I would like to thank Daniel Garber, Alison Laywine, and Ian Mueller for their comments. Since I have not seen a text of Professor Westman's remarks, either before or after he delivered them, I cannot respond to his criticisms.

1 Duhem (1913). My summary of Duhem's work corresponds closely to his own review of (what he saw as) his chief results in his preface to this volume.

2 Here I am summarizing Maier's main points in criticism of Duhem in her article 'The Significance of the Theory of Impetus for Scholastic Natural Philosophy', collected (in English translation) in Maier (1982), pp. 77–102.

3 By an 'Aristotelian' I mean here any writer who accepts Aristotle as an authority in philosophy, and who therefore prefers not to disagree with Aristotle when he can avoid it; some Aristotelians will be more ready than others to disagree with Aristotle on special occasions, or to interpret Aristotle's text in implausible ways to harmonize him with other authorities. How far Aristotle himself was an Aristotelian depends on how far he tried to remain consistent with himself, an issue I will not address here. A 'Platonist', similarly, is a writer who accepts Plato as an authority. Thus the Athenian and Alexandrian philosophers of the fifth and sixth centuries A.D. are (to varying degrees) both Aristotelians and Platonists at once. The histories of Aristotelianism and Platonism are closely interwoven, and it is a serious mistake to look only at Aristotelians (or, worse, only at 'pure' Aristotelians) in studying the medieval background to early modern science.

4 I will frequently keep *nous* in the original, and use it as a technical term within an English context. It is translated into Arabic as *'aql*, which often becomes *intellectus* in Latin, but in the context of celestial physics becomes instead *intelligentia*. In citing Arabic texts, I will retranslate the Arabic *'aql* back into the Greek *nous*. For what follows, I will be chiefly interested in the paradigmatic case of the outermost heaven; I will not explore the problem of the plurality of unmoved movers and their relation to the first mover.

5 See Duhem (1913), p. ix. He adds there that the moment when Buridan made this proposal marked the line "separating the reign of ancient science from the reign of modern science".

6 I will necessarily be brief and will discuss only selected thinkers and texts. I give some interpretations which are controversial, particularly of the *Philebus* and *Timaeus* (where I generally agree with Hackforth and with the consensus of the ancient Platonists), and of Aristotle *De Anima* III, 5 (where I agree with Alexander of Aphrodisias). I cannot defend these interpretations adequately here; this is inevitable, in a study not primarily devoted to Plato and Aristotle. I will develop these interpretations at length in other works.

7 See Hackforth (1945) and Hackforth, 'Plato's Theism', collected in Allen (1965).

8 Compare Plato: "*nous* is either the same as truth, or it is of all things the most like to it and the truest" (*Philebus* 65D).

9 Simplicius *In Aristotelis Physicorum Libros Quattuor Posteriores Commentaria*, in (1882), vol. 10, p. 1219, quotes Alexander as saying this in his (now lost) commentary on *De Caelo* Book II. Simplicus remarks there that Alexander is interpreting Aristotle "harmoniously to his own opinion about the soul", namely that soul is inseparable from the body (and thus mortal), as a nature would be; it is clear that this implication for the

soul is why all the Platonizing commentators object to Alexander's doctrine of the causes of celestial motion. They routinely refer to the doctrine of the soul's mortality as "his [Alexander's] own opinion", and accuse him of forcing Aristotle to agree with him; in fact their charge suits themselves better than Alexander. Their arguments (besides the doctrine of immortality) for denying that Aristotle could count the soul as a nature come from Aristotle's description of the soul as the entelechy of a certain kind of natural body: soul is therefore something superadded to a body which already possesses a nature.

[10] Simplicius *In Aristotelis De Caelo Commentaria*, in (1882), vol. 7, p. 382; Simplicius is commenting on *De Caelo* II, 1, which he is harmonizing with *Timaeus* 34A and other Platonic texts. The 'divine man' is not (as the editor wrongly says) Plato, but Plotinus; the reference is to the opening sentences of Plotinus II.2, 'On the Motion of the Heaven', which in turn are explicating *Timaeus* 34A.

[11] While I do not mean to devalue the role of John Philoponus in the history of impetus theory, I do think it is important to bring out the role of Avicenna, who is much more squarely in the mainstream of the history of philosophy and science. Duhem, pp. VI–VII, says that the late Greek and Arab philosophers do not even mention impetus theory, and he implies that this is because they despised its (supposed) Christian origins. While everyone now knows that Duhem was wrong about this, I think it is important to bring out just how universally Platonists and Platonizing Aristotelians accepted some version of impetus theory.

[12] This chapter, from which the subsequent quotations are drawn, is Book IX, Chapter 2 of the *Metaphysics* of the *Shifa'*. I translate from Ibn Sina 1960, in which the chapter is pp. 381–93.

[13] Buridan proposes this *imaginatio*, explicitly qualifying it as such, at Buridan (1942), pp. 180–81, and in other works. Throughout this work (e.g., at p. 132), and in other works, Buridan continues to uphold and presuppose the theory of moving *intelligentiae*.

[14] For a discussion of Descartes's use of the older tradition, see my unpublished dissertation, Menn 1989. I am better able there than here to defend some controversial interpretations.

[15] The *Principles of Philosophy* is found in Descartes (1964), vol. IX, pt. 1. I will refer to the different sections of Part Two of the *Principles* simply by their article numbers.

[16] The French translator also uses "les mesmes loix" for the "eadem ratione" of art. 36.

[17] Thus Descartes says at Descartes (1964), vol. I, p. 152: "he [God] is the author of the essence as well as of the existence of creatures: but his essence is nothing other than these eternal truths"; and there are quite a few parallels. It is *not* possible to read Descartes as explaining essence in terms of eternal truths; in context, the explanation is clearly the other way around.

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DUHEM AND KOYRÉ ON DOMINGO DE SOTO

ABSTRACT. Galileo's view of science is indebted to the teaching of the Jesuit professors at the Collegio Romano, but Galileo's concept of mathematical physics also corresponds to that of Giovan Battista Benedetti. Lacking documentary evidence that would connect Benedetti directly with the Jesuits, or the Jesuits with Benedetti, I infer a common source: the 'Spanish connection', that is, Domingo de Soto. I then give indications that the fourteenth-century work at Oxford and Paris on *calculationes* was transmitted via Spain and Portugal to Rome and other centers where Jesuits had colleges, and figured in the rise of mathematical physics at the beginning of the seventeenth century. A result of these researches is their vindication of Duhem, as contrasted with Koyré, on the origins of modern mechanics.

Pierre Duhem and Alexandre Koyré, both eminent French historians of science, held radically different views of the importance of Domingo de Soto for the evolution of modern science. For Duhem, Léonardo da Vinci was the linchpin in a development that stretched from the *Doctores Parisienses* to Soto, and Soto himself was the proximate source of Galileo's early writings and of the ideas contained in his later works (Duhem 1906–1913). Duhem based his analysis on two of Galileo's early manuscripts, now numbered 46 and 71 in the Galileo Collection in Florence, which had been transcribed and published by Antonio Favaro in the National Edition of 1890 with the titles *Juvenilia* and *De motu* respectively. For Koyré, on the other hand, Soto was merely an enigma, a Spanish scholastic isolated from the main flow of European thought (Koyré 1964). In his view neither Soto nor the Parisian doctors nor Léonardo figured importantly as sources of Galileo's science. Following Favaro's lead, Koyré preferred to see the whole of that tradition summarized in the writings of two of Galileo's Italian predecessors, Francesco Buonamici and Giovan Battista Benedetti (Koyré 1939, 1978). The first he discerned behind Galileo's MS 46 and the second behind his MS 71. The medieval and Renaissance development that had been traced in such detail by Duhem might be of antiquarian interest, but it was not at all necessary for Koyré's understanding of Galileo and the 'new science' he had brought into being.

Some years ago, at a conference in this Center, I focused on the

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debate between Duhem and Favaro as recorded in Favaro's 1916 review of Duhem's *Études sur Léonard de Vinci* and his 1918 resumption of that review in an essay entitled 'Galileo Galilei e i Doctores Parisienses' (Favaro 1916, 1918). My conclusion then was that, eminent though both were as scholars, neither had gone far enough in his researches; if they had, the dispute between them could have been dissolved in terms of what I was then developing as a 'qualified continuity thesis' (Wallace 1978, 1984b). In this essay I wish to enlarge on that theme by focusing not on Favaro but on Koyré, and by doing so in light of a third Galileo manuscript that was completely misjudged by Favaro, excluded by him from the National Edition, and as a consequence was unknown to both Duhem and Koyré. I refer to MS 27, the manuscript containing Galileo's treatises on Aristotle's *Posterior Analytics*, recently transcribed and edited by William F. Edwards and myself (Galilei 1988). This manuscript provides yet stronger support for Duhem's continuity thesis – but in a way that is somewhat surprising in that it allows one to integrate Koyré's findings into it and so include Benedetti as another possible link between Galileo and Soto. The intermediary that makes the linkage possible is the one that enabled me to dissolve the Duhem–Favaro controversy well over a decade ago, namely, the Jesuit tradition at the Collegio Romano. It is clear now that Galileo's MS 27 derives from lectures given at the Roman College, and, in light of that derivation, that MSS 46 and 71 derive similarly from the same source (Wallace 1986). What is more problematical is how to relate Benedetti to the Roman Jesuits. I shall therefore start with the Benedetti–Jesuit relationship and then work back from this to Domingo de Soto.[1]

BENEDETTI AND JESUIT SCIENCE

At first glance there would seem to be little that would connect the Collegio Romano, the Jesuit university founded by Ignatius Loyola in Rome in 1551, with Giovan Battista Benedetti, the patrician of Venice whose life spanned the years from 1530 to 1590. Benedetti's visits to Rome apparently were few. He is recorded as having lectured there on the science of Aristotle in the winter of 1559–1560, when the Collegio was but a fledgling institution, but to my knowledge had no contact with Jesuits at that time. From the Collegio side, in the years up to Benedetti's death there seems to have been little appreciation of his scientific work on the part of its philosophy professors, although he was known to the eminent mathematician on the faculty, Christopher Clavius. Such tenuous connections offer little basis for a documentary analysis of possible ties between Benedetti and the Roman Jesuits (Wallace 1987b, nn. 1–5).

In the absence of such evidence, I shall turn to a conceptual study and focus instead on the role of mathematics in the study of nature as an apt basis for comparison. In it I aim to show that by the time of Benedetti's death in 1590, the faculty at the Collegio Romano had come to a view of mathematical physics very similar to his. This would seem to be an important consideration, for it was such a conception of mathematical physics that channeled into MSS 27, 46, and 71 of the young Galileo and thence exerted an influence on the development of his science. Thus the *terminus ad quem* of my investigation is Galileo's writings on motion and mechanics around the year 1590. The *terminus a quo* is somewhat more problematical, and I will come to that later. For the moment I shall identify it simply as 'the Spanish connection', based on the facts that, on the one hand, Benedetti's father was a Spanish philosopher and physicist (or physician) and that many of his own professional contacts were with Spaniards, and, on the other, that the early Jesuit professors at the Collegio Romano were also Spanish and imported from the Iberian peninsula several ideas that proved seminal in the new mathematical physics (Wallace 1987b, nn. 6–9).

Starting, then, in somewhat ahistorical fashion with the *terminus ad quem*, let me characterize briefly the concept of science that emerges clearly in Galileo's early treatises on motion in MS 71 and that continued to dominate his later writings down to the *Two New Sciences* of 1638. This was very much a mathematical physics that proposed itself as a *scientia* and presented its reasonings in the form of *demonstrationes*; its model was ostensibly that of Archimedes, but the ideal was already Aristotle's as formulated in his *Posterior Analytics*. Working out the implications of his new *scientia* (in effect a *scientia mixta* or *scientia media*, subalternating physics to mathematics), Galileo was sharply critical of the causal analyses found in Aristotle's *Physics* and *De caelo*, while at the same time he was intent on searching out, in an Aristotelian mode, the *verae causae* of natural phenomena. Local motion (*motus localis*) was his major concern; to explain this he invoked the principal concepts used by Aristotle – nature and violence, time, place and space, force and resistance, causality – although he rejected others associated with the medium through which the moving object passed, e.g., Aristotle's teaching on the void and his solution to the projectile problem. In their place Galileo substituted the scholastic concept of *impetus*, which he used to explain both violent and natural motion. His most important methodological innovation was his clever use of *suppositiones* when framing his demonstrations, making them amenable to the use of limit concepts and to applications in experimental situations where a mathematical ideal could be closely approximated in the physical world (Wallace 1987b, nn. 10–14).

Much of my recent research has been directed at showing how this view of science is indebted to the teaching of Jesuit professors at the Collegio Romano, whose lecture notes on logic and natural philosophy were the proximate source of Galileo's MSS 27 and 46 and prepared for the *De motu antiquiora* of MS 71. But those who are acquainted with the works of Benedetti will surely have noticed how closely Galileo's concept of mathematical physics just sketched by me corresponds to that of Benedetti. Such correspondence suggests points of comparison between Benedetti and the professors of the Collegio. To develop it, we must look in detail at Benedetti's main theses and then see how these compare with related teachings among the Jesuits whose lecture notes I have studied (Wallace 1987b, nn. 15–16).

BENEDETTI'S MATHEMATICAL PHYSICS

For convenience let me divide my consideration of Benedetti's physics into two parts, the first concentrating on its logical and methodological foundations, the second on its treatment of problems relating to local motion. With regard to the first, there can be no doubt that, from the outset of his career, Benedetti wished to reinforce his arguments as much as possible with 'mathematical demonstrations' (Benedetti 1553); in his last and most important work he identifies his basic disagreement with Aristotle as based "on the unshakable foundation of mathematical philosophy, on which I always take my stand" (Benedetti 1585, p. 196). This presupposes, of course, a difference between physics and mathematics, of which Benedetti was well aware: "balances or levers are not pure mathematical lines", he writes, "but are physical, and as such exist in material bodies" (144).[2] Again, "since balances are material and are sustained . . . not by a mathematical point but by a line or a physical surface having a material existence, some resistance arises to the motion of the arms" (153). Yet he wished to use mathematical principles, such as that "a sphere touches a plane at only one point" (155), to establish physical conclusions. The only way he could do this, he recognized, was through the use of appropriate suppositions and thought experiments. It is thus important to recognize how frequently the terms *suppositio* and *imaginemur* (and their variants) recur in Benedetti's writings. Well known are his disagreements with Tartaglia and Jordanus Nemorarius in his solution of mechanical problems; invariably these are occasioned by the divergent *suppositiones* on which the respective solutions are based. For example, Benedetti frequently reproves Tartaglia for supposing that the "lines of inclination" going from the ends of a balance to a distant center of gravity are parallel (150). Yet on some occasions he makes the same supposition himself, noting that the line of inclination is *fere perpendicularis* to the beam of the balance or that, if the angle it makes is not a right angle, the deviation is negligible. But, when discussing the imagined case of a balance situated close to the earth's center, he rightly insists that the approximation cannot be made and that the simplifying supposition cannot be employed in a rigorous proof (143).

Such suppositions are important in the treatment of problems in statics, but they are crucial for the development of a science of dynamics. Benedetti was intent on discovering the *verae causae* – an expression that occurs repeatedly in his writings – of various kinds of motion in the universe, both natural and forced. An important contribution was his study of horizontal motions on the earth's surface; here he was convinced that the only truly natural motion is circular, for this alone can be perpetual (184). But, he reasons, there is "no noteworthy difference" between a perfect sphere and a plane surface of small extent. For this reason one will encounter no difficulty in moving a sphere along a horizontal surface; indeed, it can be moved by "a force no matter how small" (156). In another context he qualified an argument to specify that it holds only "when all impediment is removed" (154). Such insights, plus Benedetti's frequent allusion to the natural tendency of a body when released from a sling to move in a straight line, shows how close he came to the principle of inertia later formulated by Sir Isaac Newton.

Moving on to his study of problems relating to local motion, we can treat these under three headings, namely, those relating to motion in general, those relating to falling motion, and those relating to the movement of projectiles. With regard to the first, Benedetti was Aristotelian in his conviction that nature is an inner source of motion in a body; even forced or violent motion he saw as caused by an *impetus* or *virtus movens* impressed within a body. But unlike Aristotle he seems not to have invoked a sharp dichotomy between natural and violent motion, or between curvilinear and rectilinear motion, regarding the latter two as mathematically comparable (194). He does not discuss explicitly the possibility of a *motus medius*, i.e., one intermediate between the natural and the violent, but for him horizontal motion for limited distances would answer to that description. And in the case of reflex motion, he invokes the principle that a circle touches a line at only one point to argue that no intermediate rest (*quies medius*) interrupts the upward and downward motion of a body, making its motion truly continuous (184).

Benedetti is most known, of course, for his study of falling motion, especially for his argument *contra Aristotelem et omnes philosophos* that velocity of fall is dependent not on weight but on specific gravity, and therefore is conditioned by the medium in which the body falls and the resistance it encounters (Maccagni 1983). He proposed that velocity of fall increases with distance of travel because impetus builds up naturally in the falling body, and that all bodies would fall with the same speed *in vacuo*, where buoyancy and resistive effects can be neglected. Gravity and levity became for him relative concepts, so that air has no weight in air, nor water in water. And he analyzes the case of a body falling through the center of the earth to argue that it would oscillate about the center, on the analogy of the motion of the bob of a pendulum of exceedingly long length (Benedetti 1985, pp. 174–85, 368–69).

Equally ingenious is Benedetti's study of projectile motion, which is dominated by his skillful use of the concept of impetus, already referred to. This he regarded as a force impressed on a body from without but that moves it from within, decreasing gradually and continually with the body's motion (160). Most motions involving trajectories of bodies he saw resulting from a composition of motions, partly natural and partly forced (161), and in this is seen as having noticeably advanced beyond the teaching of Tartaglia.

COUNTERPARTS IN JESUIT TEACHINGS

Such was the contribution of Benedetti to the foundations of mathematical physics by the time of his death in 1590. The question I now would raise is this: how similar were the teachings of Benedetti as I have just outlined them to those at the Collegio Romano during the years, say, from 1560 to 1590? An answer is difficult because of the paucity of records that have survived from this period. At the beginning, Francisco Toletus taught the physics course in Rome during the academic year 1560–61, and his printed text gives indication that his ideas were fairly similar to Benedetti's. But only a year or two later, Benedictus Pererius took over the course in 1562–63, and, as his textbook indicates, set it on a path almost diametrically opposite to his predecessor's. Decidedly antimathematical and Averroist in his approach, Pererius combatted most of the Benedetti's theses concerning motion, not naming him explicitly or even being aware of his teachings, but simply rejecting out of hand the principles on which they were based (Giacobbi 1977).

This mentality apparently persisted at the Collegio for some fifteen years, and then gradually changed owing to two factors: the influence of Clavius, who fought strenuously to give mathematics a respectable place in the curriculum, not merely in its own right but also as an adjunct to natural philosophy; and the advent of a new physics professor, Antonius Menu, who was much interested in 'calculatory' techniques and imported them where possible into his lectures. A series of professors who followed Menu – Paulus Vallius, Muzius Vitelleschus, and Ludovicus Rugerius – developed their teachings on motion and the heavens along lines more acceptable to Clavius, and thus closer to Benedetti. Finally one of Clavius's special students, Giuseppe Biancani, synthesized all this work by systematically elaborating a mathematical physics capable of dealing with the problems of natural philosophy (Giacobbi 1976).

I shall elaborate more fully on this development later in the essay. Suffice it here to call attention to Vallius's commentary on the *Posterior Analytics*, particularly to his treatise *De praecognitionibus*, which was appropriated by Galileo in his MS 27 (Galilei 1988); this, taken in conjunction with Clavius's preface to his *Elements* and Biancani's later emendations, shows how *suppositiones* can be employed to uncover causes and supply *demonstrationes* in these difficult subject matters. Menu and Vallius recovered the concept of *impetus* and showed how it, and other notions in the scholastic tradition, could improve Aristotelian teachings as these were being advanced by the peripatetics of their day (Wallace 1981c). Vitelleschus and Rugerius built on these foundations. Vitelleschus is particularly important for his awareness of Benedetti's analysis of falling motion, though he knew it only through a work by Jean Taisnier that plagiarized Benedetti's *Demonstratio* of 1554 (Maccagni 1967). The reference occurs in Vitelleschus's lectures on the *De caelo* of Aristotle (given in 1590), where he questions Aristotle's laws of motion as stated in Books 4 and 7 of the *Physics*, and directs his students to the treatises of Bradwardine and Taisnier on the ratios of motions. In the same manuscript Vitelleschus echoes Benedetti's sentiment against the authority of Aristotle, stating that it is safer to abandon some of his teachings than it is to interpret them, for the authority of a philosopher should be used to confirm the truth, not abandon it, seeing that truth is the philosopher's friend. Rugerius then took up Vitelleschus's teachings on the ratios of motion, noting that Aristotle's rules for comparing motions labor under severe difficulties. For a fuller discussion of how they might be revised he then refers his students to the commentaries of Toletus and Soto, among others, in their commentaries on the *Physics* (Wallace 1987b, nn. 53–57).

In the writings of Jesuit professors from Menu to Rugerius, therefore, one can find illuminating discussions of the internal causes of motion, of the possibility of motion in a void, and of an intermediate or neutral motion (neither natural nor violent) that can endure perpetually. One can find too a rejection of the *quies medius* in reflex motion; a rejection of Aristotle's dynamical laws of motion; a sophisticated discussion of gravity, including the distinction between extensive and intensive gravity, similar to Benedetti's notion of specific gravity; a rejection of the notion that air has weight in air based on Archimedian principles; and detailed analyses of the factors that cause bodies to accelerate as they fall. Not all these teachings are the same as Benedetti's, but one gains the impression that, had the Venetian mathematician visited the Collegio in the years following the publication of his last work, he would have found a compatible atmosphere in which to advance his researches (Wallace 1987b, nn. 58–59).

THE SPANISH CONNECTION

This, then, brings me back to *terminus ad quem* with which I began this discussion. Most of the ideas I have just sketched are to be found, in various ways, in the lectures of Jesuits in Rome around 1590, in Galileo's MSS 27, 46, and 71, likewise composed around 1590, and in Benedetti's publications, probably known to Galileo through Jacopo Mazzoni, with whom he studied in 1590. I suspect that it was a fusion of ideas gleaned from Benedetti and the Jesuits that lay behind the various drafts on motion in Galileo's MS 71. Yet I have found no documentary evidence that would connect Benedetti directly with the Jesuits, or the Jesuits with Benedetti, in the development of these concepts. Was there a common source from which they could have derived? I suspect that there might have been, and I would like to speculate about this as the *terminus a quo* of my investigation – the 'Spanish connection' to which I have alluded above.

A plausible candidate for the origin of a mode of thought that would allow mathematics to enter into an experimental study of motion is none other than the Spanish Dominican who first proposed that the motion of falling bodies is uniformly accelerated with respect to time – *uniformiter difformis* is the expression he used – and who was seen by Duhem on this account to be a scholastic precursor of Galileo (Duhem 1906–1913). I refer, obviously, to Domingo de Soto. Soto was known to the Jesuits; indeed, Toletus had studied under him at Salamanca before joining the faculty of the Collegio, and Rugerius, as we have seen, called attention to his superior treatment (along with Toletus's) of Aristotle's dynamic laws of motion. Now, in his commentary and his questions on the *Physics*, Soto assimilated his doctrine on impetus to his teaching on *gravitas* and taught that a falling body accelerates continuously because of the impetus being built up in it during its travel – ideas very similar to those found in Benedetti. These notions are not fully developed in an incomplete edition of Soto's *Physics*, published at Salamanca around 1545, but they are present in the edition of 1551 as well as in the more widely diffused second edition of 1555, both also printed in Salamanca. Between 1545 and 1550, moreover, Soto was present at the Council of Trent, which took place just north of Venice. As the most illustrious theologian in the Dominican Order, he was surely known to Abbot Gabriel de Guzman and the two Spanish Dominicans Benedetti praises so lavishly in his *Resolutio* of 1553 and his two versions of the *Demonstratio* of 1554 and 1555, directed, as we saw, "against Aristotle and all philosophers". While in northern Italy, it is also possible that Soto became acquainted with experimental work being done there on laws of fall, which would have buttressed his own rejection of Aristotle's teaching on this subject. And finally, though I have found no mention of Soto in Benedetti's *Speculationes* of 1585, it may be no mere coincidence that Soto's *Physics*, both commentary and *quaestiones*, was reprinted in Venice in 1582 with an introduction that gives fulsome praise to his ability as a natural philosopher (Wallace 1987b, nn. 63–68). All bits of coincidental evidence, but all pointing to Soto as a link that could ultimately tie together Benedetti, Galileo, and the Jesuits of the Collegio Romano.

SOTO'S SECOND ENIGMA

Earlier I remarked that Soto was an enigma for Koyré, but I did not elaborate on Soto's enigmatic status. Actually two enigmas can be associated with Domingo de Soto. The first is how he came to know that the motion of heavy bodies in free fall is *uniformiter difformis* with respect to time, and the second is how this knowledge might have been transmitted to Galileo. The first enigma was what puzzled Koyré and served as the subject of an essay I published years ago with the title 'The Enigma of Domingo de Soto: *Uniformiter Difformis* and Falling Bodies in Late Medieval Physics' (Wallace 1968). The second enigma, to my knowledge, was not explicitly addressed by Koyré, although it posed the problematic on which much of his *Études galiléennes* was based. Let us address this second enigma now, for, if we can cast light on that, we may additionally be able to fill a lacuna in Duhem's thesis about Soto and his importance for Galileo's science. We can do so through a study of books and manuscripts written by Jesuits in Italy and Portugal in the century following Soto's publication of his *uniformiter difformis* doctrine.

Galileo mentions Soto twice in MS 46, in a *Tractatus de elementis* that occupies the last part of the manuscript. We now know that this *Tractatus*, as well as other treatises written by Galileo at Pisa around 1589–1591, were based on lectures given by the Jesuits mentioned above (Wallace 1977). Some of these lectures were published, but the majority survive only in manuscript. They were based on scholastic and Renaissance authors, whom they cite extensively, and are otherwise prosaic teaching notes. What makes them somewhat distinctive is the attention they pay to nominalist teachings deriving from the *Calculatores* of Oxford University and the *Doctores Parisienses*.

The development of these lecture notes took place in Rome at the Collegio Romano over a period of some thirty years. There the introduction of calculatory thought is traceable to Toletus, himself a Spaniard, who taught the course in natural philosophy in 1560 and imported ideas he had learned from Soto at Salamanca. Some of this material was taken up by two other Spanish Jesuits, Pererius, mentioned above, who taught natural philosophy at the Collegio between 1558 and 1566, and Francisco Suarez, who taught theology there between 1580 and 1585. Fortunately these authors published their materials, which have been analyzed in some detail by Christopher Lewis (Lewis 1980). Lewis picked out for examination the use by all three of calculatory language in the following areas of natural philosophy: (1) when discussing problems of action and reaction; (2) when treating the intension of forms in alteration and identifying distributions of qualities as uniform, uniformly difform, etc.; and (3) when analyzing the ratios of motions following the tradition of Thomas Bradwardine.

Of the three Jesuits, Toletus undoubtedly made fullest use of the *Calculatores* in these areas, even referring to "the calculator Suisset" (i.e., Swineshead) by name in his treatments of reaction and alteration. He also had the clearest understanding of calculatory terminology, although he frequently departed from positions held at Oxford and favored instead those developed at Paris. In treating expressions such as *uniformiter difformis*, moreover, Toletus made the interesting comment that "these [terms] should be very carefully considered in order to understand many matters that are met with in physics". Suarez likewise took up uniform difformity in some detail when analyzing the action of natural agents in his *Disputationes metaphysicae* of 1597, although he rejected the view (apparently subscribed to by Toletus) that velocity could be viewed as an intensity of motion, which would be expected of one subscribing to Mertonian developments in kinematics. Pererius, predictably, showed the least acquaintance with, or interest in, the calculatory tradition, although he was acquainted with some of its terminology. In discussing the dynamical laws given by Aristotle in the seventh book of the *Physics*, for example, Pererius accepted and defended them without even a nod in the direction of Bradwardine, thus showing little sympathy for the mathematical physics developed two centuries earlier at Merton College, Oxford (Lewis 1980).

As already noted, partially because of his antimathematical bias Pererius was replaced after 1566 and succeeded by a series of other professors. Lecture notes survive from only two who taught between then and 1585, viz., Hieronymus de Gregorio and Antonius Menu, but the second of these, Menu, enjoyed the longer tenure and seems to have had the greater influence. Menu revived the approach of Toletus and had a notable effect on the Jesuits mentioned above who taught natural philosophy at the Collegio between 1585 and 1592, namely, Vallius, Vitelleschus, and Rugerius, all of whose lecture notes survive in whole or in part. Although some details are lacking, these four professors supplied the materials on which Galileo's early notebooks on the *De caelo* and *De generatione* and the early versions of his *De motu* were based, and so serve to explain Galileo's knowledge of the calculatory tradition (Wallace 1987a, n. 14).

Menu is of particular importance for standing at the head of this fifteen-year tradition, which used Mertonian terminology but usually applied it in ways more consistent with teachings in vogue at Paris in the fourteenth century and so associated with the *Doctores Parisienses*. Indeed, Menu cites these doctors when treating the question whether the world could have existed from eternity and when discussing the ratios of the elements. He was also favorable to their adoption of impetus, or *virtus impressa*, as necessary to explain the motion of projectiles. Particularly striking are his arguments in favor of the proposition that "the motion of a simple or compound body through a void will be successive, for granted that it would encounter no extrinsic resistance, there would still be intrinsic resistance" to overcome. These are clearly those of the *Parisienses*, adopting the calculatory stance of the Mertonians but applying it to physical problems in the tradition of Jean Buridan, Albert of Saxony, and others who worked in fourteenth-century Paris (Wallace 1987a, nn. 15–16).

The lecture notes of Vallius, Vitelleschus, and Rugerius do not employ these particular arguments, but they nonetheless touch on all the matters pertaining to the mathematical or calculatory tradition that are to be found in Galileo's early writings. The latter's notes in MS 46 are written in the form of a questionary based on Aristotle's *De caelo* and *De generatione*. The questions wherein analytical languages in the Mertonian and Parisian traditions occur most frequently are in the treatises *De alteratione* and *De elementis*, where inquiries are made into the intension and remission of forms, the parts and degrees of qualities, and the number and quantity of the elements. There seems little doubt that all of these materials are derived from lectures given at the Collegio some time prior to 1591. The precise author is difficult to identify, however, since correspondences can be found between Galileo's notes and passages in Rugerius, Vitelleschus, Vallius, and Menu, and indeed all the way back to Pererius and Toletus. At the present stage of research Vallius seems to be the most likely candidate, for, although his surviving lecture notes are incomplete, the portions that survive show closest agreement with Galileo's text. There is excellent evidence, moreover, to connect Galileo's MS 27, the one containing questions on Aristotle's *Posterior Analytics*, with Vallius's lectures on logic, which were completed in the summer of 1588 and manifest a good knowledge of nominalist positions on science and demonstration (Wallace 1987a, nn. 19–21).

My study of all these materials thus encourages me to go considerably beyond Christopher Lewis in identifying likely sources of Galileo's knowledge of the calculatory tradition. Suffice it to mention that citations from Walter Burley and William Heytesbury, as well as Bradwardine and Swineshead, are to be found in these Jesuit lectures. And not only do such citations occur in discussions involving intension and remission, latitudes of qualities, and maxima and minima, but they also occur in discussions of local motion and of Aristotle's dynamical laws involving ratios between forces, resistances, and velocities of motion. Vitelleschus, for example, cites experimental evidence against the Aristotelian formulations in Book 7 of the *Physics* and refers his students to Bradwardine's *De proportione motuum* for an alternative view. Rugerius likewise discerns difficulties with Aristotle's rules and, as already noted, sends his students to Toletus and Soto for more satisfactory treatments of the ways in which velocity varies at the beginning, middle, and end of motion (Wallace 1987a, nn. 22–24).

Of the natural philosophers who taught physics at the Collegio Romano after Rugerius down to 1626, I have thus far located *reportationes* of lectures by four other Jesuit professors: Robert Jones, an Englishman, who taught in 1592–93, Stefano Del Bufalo, who taught in 1596–97 and again in 1598–99; Andreas Eudaemon-Ioannis, who taught in the intervening year 1597–98, while Del Bufalo had the course in metaphysics; and Fabiano Ambrosio Spinola, who taught in 1625–26. Of these, the treatments of the first and the last, Jones and Spinola, show less concern with the calculatory tradition. Del Bufalo, on the other hand, has a rather full discussion of alteration, degrees of qualities, intension and remission of forms, and action and reaction – in the last of which he mentions the teaching of the *Calculator* and contrasts it with those of Pomponazzi, Buccaferreus, Flaminio Nobili, Franciscus Neritonensis, and Zabarella. In his discussions of *gravitas* and *levitas*, moreover, he mentions the *Parisienses* and compares their teachings with those of Geronimo Borro and Buonamici – and this in 1597, the year in which Buonamici's *De motu* had just appeared. It is noteworthy that all of Del Bufalo's notes located thus far are in the National Library at Lisbon, where they had been taken from the Jesuit college at Evora, having been sent there from Rome by October of 1603, as recorded in one of the codices containing them (Wallace 1987a, nn. 25–28).

The other professor who deserves mention for his calculatory interests is Eudaemon, who, as already mentioned, had the course in natural philosophy in 1597–98. In addition to his lectures on the *Physics*, *De caelo*, and *De generatione*, he left a *tractatus* in two books on action and passion and a *quaestio* on the motion of projectiles, both of which are written in the calculatory manner. As I have pointed out in my *Galileo and His Sources*, Eudaemon is of some importance for the fact that he discussed "the ship's mast" experiment with Galileo at Padua, and, along with Biancani, also teaching there, could have influenced Galileo's use of calculatory terms in his *De motu accelerato* fragment and later writings (Wallace 1984a; 1987a, nn. 29–30).

The first book of Eudaemon's work on natural agency, entitled simply *Tractatus primus*, is prefaced by five definitions and nine suppositions; it then develops twenty-one propositions, with proofs and corollaries, all relating to the ways in which qualities come to be mathematically distributed as a result of such agency, with occasional geometrical diagrams in the margins illustrating the text. Noteworthy among the definitions are the third and the fifth, the third stating that "something is said to be distributed uniformly difformly when it diminishes in the same ratio as the distance increases," and the fifth explaining how quantitative attributes can be ascribed to a quality that is uniformly difformly distributed. Following the definitions Eudaemon begins his suppositions, which he prefaces with the note:

> Because the matters with which we shall be concerned are physical, it is necessary to take some propositions from our physical disputations that can be presupposed as principles in this treatise. Things that are commonly conceded in physical science or are sufficiently proved and explained may be seen in our disputations on *De generatione* and on the *Physics*. And since this treatise is principally mathematical, it will not be necessary to note and prove propositions that come from mathematics.

This notation, and the nine suppositions that follow it, are important for the fact that they show Eudaemon adopting the stance of a mathematician and attempting to develop a mathematical physics of natural agency, even though he was a philosopher entrusted with the main sequence of courses at the Collegio. Also noteworthy is Eudaemon's first supposition, which reads as follows:

> We presuppose that every natural agent acts *uniformiter difformiter* on a quantified subject when applied to it. Physicists commonly concede this, at least with respect to some parts of the sphere of activity, because we see that when close the agent acts more vehemently and when farther way less so; therefore, the closer the greater, the farther the lesser; therefore, as the distance increases the action decreases; therefore the action is uniformly difform.

Noteworthy here and throughout the treatise is the preoccupation with the expression *uniformiter difformis* as applicable to natural agency (Wallace 1987a, nn. 31–33).

The second book of this same treatise is titled *De iis quae in actione et passione physica contingunt* and it begins, like the first, with definitions, ten in number, then notes a single supposition, and concludes with proofs of thirty-one propositions, some of which contain substantial numbers of corollaries. The reason for this development is not transparently clear at first reading, but it all becomes intelligible when we get to the *Quaestio de motu proiectorum* that follows immediately after the second book. The entire treatise on natural agency had occupied fifty-one closely written folios; that on the motion of projectiles coming after it takes up seventy-two more. Divided into three articles, it inquires first whether the projector moves the projectile immediately at a distance, then whether the *vis movens* is within the projectile itself, and finally whether the *virtus movens* is located in the medium, and if so, how (Wallace 1987a, n. 34). Somewhat surprisingly, considering the fact that his predecessors at the Collegio had all adopted the impetus explanation of projectile motion, Eudaemon ends up by rejecting an impetus in the projectile and by putting the *virtus movens* in the air. I have not yet analyzed his arguments in detail, but I suspect that his reason for doing so is to subsume projectile motion under natural agency so as to show that it slows down uniformly difformly. This, we may recall, was Soto's position, for he held that falling motion is accelerated and projectile motion decelerated in the same quantitative way, namely, *uniformiter difformiter*.

If such was Eudaemon's thesis in this manuscript, undated but probably written in 1599, his discussions with Galileo at Padua around 1604 take on special significance. At that time Galileo was looking for a principle on which he could construct his new science of motion, as we know from his letter to Paolo Sarpi. His telling Eudaemon that he had experimented with a stone dropped from the mast of a ship first at rest and then in motion shows that both were still interested in the problem of impetus. Eudaemon could therefore have been a source that directed Galileo's attention around 1604 to calculatory treatments of uniform acceleration and deceleration, later to be reflected in the *De motu accelerato* fragment on which the *Two New Sciences* would be based (Wallace 1987a, nn. 35–37).

Let us look back, then, at the situation at the Collegio Romano from the time of Pererius, say 1566, when he taught, or 1576, when his textbook was published, to Eudaemon in 1599. In all of that time there were many references to the *Calculatores* and *Parisienses* and how they impacted on theses in natural philosophy. Not one, however, is to be found in a printed text – all occur only in manuscript sources. It is not surprising, therefore, that influences deriving from this tradition have thus far been overlooked by scholars and so have not been seen as a significant factor in the growth of mathematical physics among the Roman Jesuits toward the end of the sixteenth century.

THE JESUIT TRADITION IN PORTUGAL

To move now to the Iberian peninsula, a situation similar to that at the Roman College existed in the Jesuit colleges there, particularly in those at Evora and Coimbra. The Coimbran *Cursus philosophicus* was a five-volume course, first published at Coimbra between 1592 and 1605 and reprinted often thereafter. My researches have shown that natural philosophy in Portugal became less technical and mathematical from the end of the sixteenth century onward, and this possibly explains why there is no conspicuous use of calculatory terminology in the famous *Cursus*. A goodly number of manuscripts from Evora and Coimbra dating from the 1570s and 1580s are still extant, however, and these show the same patterns deriving from the *Calculatores* and the *Parisienses* as do the lecture notes from the Collegio Romano.

Lectures on the *Physics* and *De caelo* for the years 1570, 1582, 1587, and 1588 by professors named Juan Gomez de Braga, Luis de Cerqueira, Antonio del Castelbranco, and Manuel a Lima respectively are all extant. Some of these Jesuits taught at Evora, others at Coimbra, but the substance of their notes is all the same; in some cases the wording is repeated almost exactly, suggesting a transmission of notes from one professor to another. In addition, notes from a Trinitarian, Marcus de Moura, who taught at Lisbon in 1588 are available, and his lectures are substantially the same as those given by Manuel a Lima at Evora in the same year. The same could be said of an anonymous set of lectures on the *Physics*, *De caelo*, and *Meteorology* given at Coimbra in 1580 (Wallace 1987a, nn. 40–42).

The anonymous lectures of 1580 are a good place to start, for their author gives a key to the source of most of the materials the others contain. The fifth chapter of his commentary on the seventh book of the *Physics* begins with two questions: (1) whether the velocity of local motion is to be ascertained from the quantity of space it traverses as from an effect, and (2) whence the velocity of motion is to be judged as from a cause. Following his replies to these queries the author writes: "These last two questions are treated more fully by Domingo de Soto and can be studied there. For this reason, and especially because of limitations of time, we will pass over them quickly." And his reply to the first question indicates the extent of his dependence on Soto, which I give in the slightly clearer formulation of Cerqueira, who repeated this material at Coimbra two years later, in 1582:

> Sometimes the mobile is moved so difformly with respect to time that, taken [any] part of time in which it moves, the velocity it has at the middle instant will exceed the velocity it had or will have at one terminal instant of that time by the same amount as it is exceeded by the velocity it had or will have at another [terminal instant]. Such a motion is said to be *uniformiter difformis* with respect to time, and it is found in heavy and light bodies when they move naturally, since the more they depart from their starting point the greater is the velocity with which they move.

This, of course, is the teaching developed by Soto around 1550, which is reiterated in most of these lecture notes preserved in Portugal throughout the 1570s and 1580s. It is further explained and extended to projectile motion by Cerqueira, and by Manuel a Lima again in 1588, in the following terms:

> It is customary to ask at this place why it is that things that are moved naturally in rectilinear motion are moved more swiftly at the end than at the beginning of their motion, whereas those that are moved violently are moved more swiftly at the beginning .... The reason for this is that, just as the force that exists in the hand of the thrower when

conjoined with the stone . . . impresses on the stone a certain impulse that moves it when separated from the hand of the thrower, so also gravity and levity, impelling the heavy and light thing to its natural place, impresses by such motion a certain impulse through whose agency the motion of the heavy and light thing is made swifter. And this impetus gets more intense as the heavy and light objects come closer to their natural places, which is to be understood in terms of the relation of each to the *terminus a quo*. For if one and the same stone were now to descend from the middle of a tower and later from its top, it would descend much more swiftly at the end of the later motion than at the end of the earlier. For the longer the space that is traversed the greater is the impetus impressed by levity and gravity throughout the motion, since it is continually intensified until the thing arrives at its natural place. And since this impetus effects in the heavy or light thing a motion similar to that which arises in it from gravity or levity, Aristotle referred to it as an increase of gravity and levity; others, however, speak of it as accidental gravity and levity, since it is lost as soon as the motion stops.

I give this as one example relating to the ratios of motions; similar materials relating to action and reaction, wherein the opinions of Heytesbury and the *Calculator* are discussed, could also be mentioned. But perhaps this is sufficient for present purposes to show evidences of a Jesuit mathematical tradition on the Iberian peninsula in the latter part of the sixteenth century (Wallace 1987a, nn. 43–46).

To return briefly to Italy, I would add that Biancani, who had studied under Clavius at Rome in the 1590s, wrote detailed defenses and justifications of mathematics and mathematical physics as sciences in the Aristotelian sense, wherein he shows considerable competence as both a philosopher and a mathematician. These he explicitly directed against Pererius and the authors of the Coimbran *Cursus philosophicus*. Biancani taught principally at Parma, where Giambattista Riccioli was in turn his student. And Riccioli is of some importance for his verification of Galileo's experiments on falling bodies. In his *Almagestum novum* of 1651 Riccioli recounts that he had first started experimental work in this field with two other Jesuits in 1629, and then with yet another in 1634. At that time he obtained permission, he says, to read Galileo, whom he first thought to be in error but later found to be correct. Of his early work Riccioli writes that

at that time I had not yet come to the better and more evident experiments manifesting not only an inequality in the motion of heavy bodies but the true increment of their velocity, increasing *uniformiter difformiter* toward the end of the motion.

What is interesting here is Riccioli's use of Parisian terminology deriving from Soto when describing the results to which he had finally come. This seems to me a fairly good indication that such terminology had been part of his training too, and persisted in his mind to the middle of the seventeenth century, i.e., to 1651 (Wallace 1987a, pp. 58–62).

CONCLUDING REMARKS

From all these indications it would seem that the fourteenth-century work at Oxford and Paris on *calculationes*, transmitted via Spain and Portugal to Rome and other centers where Jesuits had colleges, figured in the rise of mathematical physics at the beginning of the seventeenth century. The circumstances of this transmission may help to clear up two problems that have bothered historians of science. The first of these is the disparity between Galileo's use of calculatory language and that of the Mertonians, which has recently been analyzed by Edith Sylla (Sylla 1986). Such disparity is readily understandable when one considers that Galileo acquired that language at several removes from its initial formulators. The second is the lack of a consistent attitude on the part of the Jesuits toward the use of mathematics in the study of nature. This becomes intelligible in terms of the tensions that developed within the Order between the mathematicians and the philosophers, and the censorship that was invoked to present a 'united front' to the outside world. Vallius had difficulty with censors within his own Order when he attempted to have his Collegio Romano lectures on logic and natural philosophy published in the early 1600s, and we know that Biancani ran into the same difficulty with censorship when he wrote in 1615 and 1620 in support of Galileo (Wallace 1984a, pp. 141–48). Invariably the theologians and the leadership within the Order sided with conservative confreres among their philosophers rather than with progressive confreres among their mathematicians whenever Church teaching was involved. As a result, the period between about 1560 and 1650 presents a somewhat ambivalent picture of the Jesuits' role in the development of mathematical physics. But the manuscript record, official positions aside, shows that solid progress was being made during those decades, wherein foundations were laid for later important contributions to the sciences from within the Society of Jesus.

A yet more important result of these researches for this conference is their vindication of Duhem, as contrasted with Koyré, in the work of these French historians on the origins of modern mechanics. Koyré's fortunes have declined in recent years with the discoveries by Stillman Drake and others of the extensive experimental program on which

Galileo had embarked between 1604 and 1610. This has sounded the deathknell for Koyré's appraisal of Galileo as a Platonist or rationalist who had no need of experiment to found his 'new science'. My own work tarnishes Koyré's image a bit more, for it shows that his neglect of medieval and scholastic sources vitiated much of the reasoning behind his *Études galiléennes*, the part relating to Benedetti alone excepted. But if Koyré has been devalued, as it were, the same cannot be said of Duhem. Duhem's emphasis on Soto, it turns out, was well founded. One would no longer wish to maintain that Soto was the proximate source of Galileo's science. But whether one traces Soto's influence through the Jesuits in Italy or in Spain and Portugal, or by a parallel route through Benedetti, there seems little doubt that Soto played a pivotal role in promoting a mathematical analysis of local motion.[3]

NOTES

[1] The further development of this essay is a conflation of two previously published studies, the first focusing on the Benedetti–Jesuit relationship (Wallace 1987b) and the second on influences on the Jesuits that derived from Domingo de Soto (Wallace 1987a). Neither of these studies, on the one hand, is readily available; both, on the other, are heavily documented with references to source materials and to Latin texts. Since readers of this journal are not primarily interested in history, I have pruned much of the documentation from my presentation here. However, to make available a detailed justification of my thesis for those who might be interested, I have included parenthetical references to the footnotes of the two studies in the body of the text.

[2] The numbers here and following continue the page enumeration of the previous citation in the text.

[3] Preparation of this essay was supported in part by a grant from the National Endowment for the Humanities, an independent agency of the U.S. government.

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THE DUHEMIAN HISTORIOGRAPHICAL PROJECT

ABSTRACT. Duhem regarded the history of physical science as carrying a twofold lesson for the practicing physicist. First, history revealed the slow, groping, yet continuous development of physical theory toward a true description of the relations among natural entities. Second, history also unmasked false explanations and metaphysical beliefs that might seduce the unwary scientist into following an unfruitful line of research. This paper brings forth the central images underlying Duhem's historiographical project and uses the papers by S. Menn and W. A. Wallace to ask what Duhem's enterprise actually meant in practice. I argue that the main question is the following: What is to count as the proper space of historical meaning and explanation? 'Strong' Duhemians, such as S. Menn, purchase the *longue durée* at the cost of making historical agents into completely passive transmitters of conceptual homologies; 'weak' Duhemians, such as W. A. Wallace, shorten the temporal distance between agents and permit thereby a modicum of conflict and negotiation within physical theory while still seeking to preserve long-term conceptual genealogies. Both positions, it is argued, allow insufficient room for actors' categories to determine the space of cultural analysis.

If anyone doubts whether the spirit of Duhem is alive, these two papers must put that hesitation to rest. Steven Menn and William A. Wallace present similar stories of continuity. In each case, a genealogy has been exhibited; hidden conceptual pathways have been documented and exposed with skill; and in each account, a celebrated early modern scientific achievement has been shown to be tied to a tradition, however complex. History, and in this case, the history of science, is regarded as a repository of conceptual traditions. Like Olympian runners passing the torch, the conceptual complexes pass from one thinker to the next – not in a straight line, to be sure, for sometimes the lines of transmission are lost in the mists of the past. But, in the end, the torches are brought home, the routes of transmission mapped, the concept or method delivered safe and sound.[1]

Both contributors believe that their histories, in some sense, vindicate Pierre Duhem's historiographical project. But since they do not explicate Duhem's understanding of history,[2] it seems appropriate to touch briefly upon some of its central features, to ask in what way our contributors have construed the terms of that enterprise, and finally, to ask why it is worth our while to rake through the somewhat neglected texts

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of the distant past in search of resemblances – family ones or otherwise. Along the way, I suppose that we shall need to ask just what might be meant by an 'intellectual tradition'.

Duhem's notion of the history of physical theory had for him no mere decorative role. In *The Aim and Structure of Physical Theory* he subtitles one section, 'The Importance in Physics of the Historical Method' (Duhem 1962, p. 268). There and in *Physics of a Believer*, Duhem represents history as a "progressive evolution" (Duhem 1962, p. 220f) both in the sense that "no physical theory has ever been created out of whole cloth" (Duhem 1962, p. 221) and in the sense that logic alone cannot justify the choice among physical theories. In a famous formulation, Duhem writes: "It is not possible to compare an isolated consequence of theory with an isolated experimental law. The two systems must be taken in their integrity: the entire system of theoretical representations on the one hand, and the entire system of observed data on the other." This process of comparing entire systems of representation does not happen overnight. It is a slow, evolutionary movement guided by *bon sens* and characterized by "the hesitations, the gropings and the gradual progress obtained by a series of partial retouchings".[3] One observes this movement both in the long history of universal attraction and in the relatively short history of the formation of electrodynamic doctrines between 1819 and 1823.

Given this involvement of science history in both discovery and justification, Duhem believes that certain beneficial consequences follow. First, he thinks that history can assist the physicist broadly in avoiding seductive explanatory fads – "the gossip of the moment", "unreasoned exaggerations of the present time", "the mad ambitions of dogmatism as well as the despair of Pyrrhonian scepticism".[4] Here Duhem was undoubtedly influenced by the example of the history of philosophy in the nineteenth-century French philosophy curriculum; history of science, like its counterpart, could exhibit to the physicist the uncertainty spawned by differing opinions, systems, and philosophical sects (Goldstein 1968; Fabiani 1983). But, history could also point the physicist toward something more stable and certain. Duhem believes that the history of physical theory over the *longue durée* reveals an increasing correspondence between idealized and actual relations among entities. He reiterates this point using different images, but a *morphological* analogy appears to govern his general sense. "The naturalist", he writes, considers the diverse organs – vertebral column, cranium, heart, digestive tube, lungs, swim-bladder – not in the particular and concrete forms they assume in each individual, but in the abstract, general schematic forms which fit all the species of the same group. Among these organs thus transfigured by abstraction he establishes comparisons, and notes analogies and differences; for example, he declares the swim-bladder of fish analogous to the lung of vertebrates. These homologies are purely ideal connections, not referring to real organs but to generalized and simplified conceptions formed in the mind of the naturalist; the classification is only a synoptic table which summarizes all these comparisons. (Duhem 1962, p. 25)

The zoologist naturalizes the homologies (i.e., shows that they "correspond to real relations among the associated creatures brought together and embodied in his abstractions" [ibid.]) when he establishes that general resemblances turn out to be actual blood-relationships. In *Physics of a Believer* Duhem provides another suggestive image:

Physical theory is neither a metaphysical explanation nor a set of general laws whose truth is established by experiment and induction: . . . it is an artificial construction manufactured with the aid of mathematical magnitudes; . . . the relation of these magnitudes to the abstract notions emergent from experiment is simply that relation which signs have to the things signified; . . . this theory constitutes a kind of synoptic painting or schematic sketch suited to summarize and classify the laws of observation; it may be developed with the same rigor as an algebraic doctrine, for in imitation of the latter it is constructed wholly with the aid of combinations of magnitudes that we have ourselves arranged in our own manner. (Ibid., p. 277)

One is tempted to regard Duhem's evolutionist account of physical theory as a kind of secularized theology of history – both providential and redemptive – unveiling the Divine Plan as a directed scientific tradition and redeeming the physicist who grasps the natural classifications toward which physical theory moves. Roger Ariew and Peter Barker draw attention to an important image from *Physics of a Believer* where Duhem articulates this directionality in a physicalist idiom:

It is not enough for the cosmologist to know very accurately the present doctrines of theoretic physics; he must also be acquainted with past doctrines. In fact, it is not with the present theory that cosmology should be analogous, but with the ideal theory toward which present theory tends by continual progress. It is not the philosopher's task, then, to compare present-day physics to his cosmology by congealing science at a precise moment of its evolution, but rather to judge the tendency of theory and to surmise the goal toward which it is directed. Now, nothing can guide him safely in conjecturing the path that physics will take if not the knowledge of the road it has already covered. (Duhem 1962, p. 303; Ariew and Barker 1986, p. 149)

As Ariew and Barker notice, Duhem provides in this passage a physicist's image of history – the trajectory of a ball. At any moment in its flight, we cannot know the ball's end-point or, by analogy, no particular artificial classification or 'synoptic painting' can show us the end-point of a physical theory. For history is filled with attractive metaphysical systems and mechanism builders, mere explanatory illusions that sustain our faith in the ultimate discovery of natural law. Only by judging the direction of physical theory over time can the physicist make successful inductions toward natural classification that permit new and old laws to be connected within a single account. Duhem thus appears to subscribe to the following slogan: the history of descriptive laws in science is continuous and progressive; the history of explanatory schemata is neither.[5]

These are the briefest outlines of Duhem's historiographical project as expressed in two of his most explicitly philosophical writings. Consider now how this enterprise was to be put into practice. In *Aim and Structure*, Duhem makes much ado about the history of the "memorable discovery of universal attraction" as an instance of the progressive evolution of physical theory. This history, says Duhem, cannot possibly include everything that the Ancients uttered about the heavy and the light. "Let us retain", says he, ". . . only what prepared the way for the Newtonian theory, *by neglecting systematically everything not tending to that goal*" (Duhem 1962, p. 222; my italics). Later in the essay he informs us that:

> From the first half of the seventeenth century all the materials which were to be used in constructing the hypothesis of universal attraction were assembled, cut, and ready to be put into operation; but it was not yet suspected what an extension this work would have. (Ibid., p. 246)

Duhem's historical method reminds us of the ball in flight. Like Aristotle's projectiles, there is no natural motion without a goal; yet, like Galileo's idealized cannon ball, we must imagine away all terrestrial hindrances. Duhem's method of classifying historical homologies is possible only after all the textual underbrush has been removed.

This is not the place to pursue the reception of Duhem's historiographical project – interesting though that would be. From what we do know at present, however, one conclusion seems clear: Duhem's greatest following came not from physicists and philosophers, but from historians.[6] And from within the canon of general historical writings, Duhem was perceived as having something strikingly new to say. Duhem made medieval culture and 'the origins of modern science' into significant topics of investigation. As the historian A. Dufourcq wrote in 1913:

The origins of science are less known than its discoveries. We profit from its conquests, enjoy its benefits without any concern about the source from which they derive. Yet there is no more interesting study. In no domain is human progress secured by some spontaneous and necessary evolution. It is important to know the conditions in which science was born, the conditions in which its progress accelerates so that our future procedures may be better oriented. For this reason the works of Duhem must be highly esteemed. They establish on the basis of vast evidence that the principles on which modern science rests were formulated before Newton, before Descartes, before Galileo, before Copernicus, before Leonardo himself, by the masters of the University of Paris during the 14th century. (Dufourcq 1913; quoted and translated in Jaki 1984, p. 409)

The rhetoric of origins, the humbling of Renaissance authority, the implicit praise of Medieval Christianity, the allusions to Gallic pride all must have carried with them important symbolic meanings within the Republican politics of the fin-de-siecle French historical profession. Not surprisingly, Duhem's work also met with excitement among certain elements of the Church. Jaki cites an interesting review of the third volume of Duhem's Leonardo studies by the Jesuit Father H. Bosmans:

I remember, many years have gone by since, I was then a student of theology and philosophy, busy with things very different from the science of mechanics. . . . In order to get respite from the metaphysics of the masters of the Middle Ages, or, to tell frankly, to have a laugh for a moment, my camarades and myself read aloud a page from the physics of those old scholastics. To laugh! And how right it seemed to be! The whole world thought the same. We have long since had second thoughts about these outbursts of hilarity. Duhem's book taught me how many prejudices still remain to be corrected. (Bosmans 1914; quoted and translated in Jaki 1984, p. 410)

Duhemian historians praised the discovery of a new site of 'origins' and exulted in the challenge to the autonomy of the Renaissance; but significantly, they did so while ignoring Duhem's metaphysical and epistemological theses. The search for conceptual continuities and homologies linking medieval and early modern science had become an end in itself.

In this comment, I would like to suggest that the quest for continuities and traditions still leaves open certain difficulties that belong squarely on the doorstep of Duhem's account of physical theory.

Both of our papers begin by identifying a *terminus ad quem*. Wallace identifies this as a certain conception of science – a "mathematical physics" he calls it, characterized by a certain ideal of reasoning found in the *Posterior Analytics*; a form of reasoning that Galileo displays in a certain type of analysis of local motion. For Menn, the *explanandum* is more limited. It is not an entire conception of science, but a specific doctrine, namely, the conservation of motion and, even more specifically, that version of the conservation of motion formulated by Descartes. These termini are, presumably, what Duhem would identify as natural classifications.

Now, before we proceed further, we must ask why these termini have been chosen and not others – for example, William Gilbert's notion of "the magnetic substance common to loadstone and iron and the earth itself" (Gilbert 1958, Bk. II, chap. 1, p. 72) or Giordano Bruno's theory of natural magic. Duhem's own criterion is clear: the terminus must be "the reflection of an ontological order". That is evidently not what we have here. Menn says explicitly: "Descartes is an interesting case, both because he holds a strong and precise (though false) principle of conservation, and because he justifies this principle by an argument from natural theology." By this standard, Menn might have chosen Kepler's solar force law or Kepler's polyhedral scheme for ordering the planets and begun his story with Plato. On the other hand, Wallace makes no reference to the truth status of his terminus in this paper, but in his other writings it seems clear that he believes that Galileo did discover certain truths about nature and that he arrived at these truths from the use of a correct scientific method.[7]

So much for the problem of the *ad quem*; but where do we seek the *terminus a quo*? Here, fortunately, Duhem showed the way. For, as we know, since Duhem was the first to track down the conceptual lineages and since he initiated this undertaking nearly a century ago in quite a different context than our two authors, we can well understand his biases, in part because we have the benefit of hindsight. Duhem's *termini a quo* were all Parisian. This is because he believed that Paris was the eternal city, that everything good came from Paris,[8] including, of course, the famous Condemnations of 1277 and the method of hypothetical reasoning *de potentia Dei absoluta*. And, even in his own time, Duhem continued to look to Paris for all good ideas in physics – an attitude not shared by our authors, although the attitude is not unknown among many colleagues in literature, fashion design, and the culinary *scientiae mediae*.

Thus, we cannot allege that our contributors suffer from Parisocentrism. Yet, in different ways, they wish to recommend to us the method of filiations that led Duhem to Paris. Professor Menn wishes to take us back to Mecca via the medieval Arab Avicenna, while Professor Wallace has been edging further away from the High Middle Ages and now focuses his gaze on the Scholastic presence in the Mediterranean Basin from about 1550.[9] For Wallace, while the genealogies to medieval Paris, Oxford and Rome can still be made, his recent studies of Galileo's sources concentrate principally on sixteenth-century materials that now spread out from Rome to Jesuit academic outposts in Salamanca and Coimbra. In short, Professor Menn is, in a sense, closer to the original Duhem in searching out *causae remotivae*, while Professor Wallace's *causae* are *proximiorae*.

Does this make a difference? I think that it does. In my opinion, Professor Menn is forced into a more difficult position because the temporal and, I think, also the cultural distance is too great to sustain a common space of meanings. Let me mention some of these difficulties. First, we have no evidence that Descartes knew or understood Avicenna's notion of *mayl* at all, let alone through a source discussed by Menn. This leaves Menn arguing for a "strong possible resemblance". Second, Menn must explain why it is that Descartes's views *differ* or *diverge* so much from Avicenna's. Avicenna, for example, denies that heavenly bodies have an intrinsic source of motion and concludes that they do not resist constant motion. Yet, writes Menn, "Descartes claims that *any* bodies have an intrinsic source of natural motion and concludes that *no* bodies resist the constant motion". How and why does Descartes accomplish this? Professor Menn says that "it follows from Descartes' conception of body as pure extension, which is designed precisely to strip bodies of any natures, immanent forms or active powers". Evidently, the notion of body as pure extension does not appear in Avicenna. We are left to wonder, then, how it is that Descartes 'strips down' bodies so completely that no medieval Aristotelian could recognize them. Finally, Menn tells us that, "at a sufficiently abstract level Descartes' theory of the causes of motion is very close to Avicenna". But surely if one pulls the 'historical camera' back far enough, one can connect concepts from all times and places; and it might even be tempting to follow Carl Gustav Jung into the realm of the collective unconscious of seminal archetypes, Arthur O. Lovejoy into a realm of 'unit-ideas' or Pierre Duhem into a lineage of increasingly perfect homologies. I am not suggesting that Menn would want to go this far, but his tendency to read history for resemblances threatens to blend into a search for conceptual identities.

Professor Wallace's attempt to vindicate Duhem is, I think, more successful because the termini of his analysis are much closer in time. Galileo could, as it were, actually talk to and borrow 'software' from some of his sources. Wallace wants to make sense of Galileo's commitment to a certain view of science and to make sense of fragments of a scholastic calculatory vocabulary (especially the *uniformiter difformis* expression). In this kind of story, there are at least hints of a history of science that moves away from Duhem's philosophical history of disembodied concepts, one that acknowledges the *integrity* of particular cultures within which specific forms of scientific life are discernible.

But, on such a view, notions of 'transmission', 'continuity', and 'source', as well as 'influence' and 'reception' all become deeply problematic. For what, after all, is a 'source'? If it is a text, then it can only acquire specific historical meaning from being read. And since reading is an active, time-bound process, we cannot say that any two people, let alone any two groups, will read the same text in the same way (Chartier 1987, pp. 183–239). Nor can we assume that practices of reading in the seventeenth century, the Victorian age or today are quite the same. Nor can we assume that Domingo de Soto had the same *theological* objectives at the Council of Trent that Etienne Tempier had in 1277. Nor can we assume that Galileo, who was neither a Dominican nor a Jesuit, had the same objectives that de Soto had when he used the expression *uniformiter difformis*.

At the end of his paper, Professor Wallace throws some illumination on this problem. He informs us that Galileo's use of calculatory language was not quite the same as the fourteenth century Mertonians (see Sylla 1987). "Such disparity", writes Wallace, "is readily understandable when one considers that Galileo acquired that language *at several removes* from its initial formulators" (my italics). But again, what is a "remove" if not a different context of meaning that must be understood in its own terms?

The second point that Professor Wallace makes in his conclusion is that the Jesuits themselves did not hold a consistent attitude toward the use of mathematics in the study of nature, that there were tensions within the Order between the mathematicians and philosophers and that the Order papered over these difficulties through censorship. Even

Paolo Valla at the Collegio Romano experienced difficulty in publishing his lectures on logic and natural philosophy in the early 1600s and Giovanni Biancani later encountered resistance when he supported Galileo between 1615 and 1620. Here again we cannot assume that the mechanisms and motives of post-Tridentine ecclesiastical censorship had the same political meanings as Bishop Tempier's Condemnations of 1277. Attention to such political and social factors shows promise of righting an imbalance within Duhem's original account. For Duhem analyzed conflicts between the faculties of theology and arts at the University of Paris in the thirteenth century but failed to carry forward his study along those lines into the sixteenth and seventeenth centuries.[10]

Where, then, does this leave the Duhemian historiographical project? As a practical resource for guiding physicists, it seems to have attracted no significant audience. Even those few physicists who engaged in historical work seem not to have followed Duhem's dicta; if anything, the Quantum revolution encouraged the search for discontinuities.[11] To professional historians, on the other hand, Duhem presented a corpus of texts that simply could not be ignored – however they might be interpreted. And, in pressing for conceptual resemblances between the 'well known' and the 'newly found' he succumbed to that malaise, understandable and common among historians, of *overestimating* the value of an archival find. Nowhere is this more evident than in his notion of 'precursor'. If Alexandre Koyré undervalued Galileo's scholastic debts, Duhem, for his part, had an impoverished notion of 'source'. Too readily he was willing to regard the early moderns as passive recipients of scholastic language and concepts. Thus, Duhem pictures Osiander as a kind of receptacle who transmits unchanged a Greek doctrine while omitting the rhetorical and polemical context of his anonymous 'Letter to the Reader'. Similarly, Duhem's Copernicus in *To Save the Phenomena* voices a 'misguided' methodological realism; but Duhem ignored the way in which Copernicus tried to persuade the Pope that correcting the calendar and the order of the heavens should be part of a common agenda of Church reform (Duhem 1969, pp. 61–91; see also Westman 1987 and Westman 1990).

The Duhemian project, in other words, has tended to regard the 'learning of the schools', (Dear 1988) the inheritance of the universities, as sources that influenced passive historical actors, rather than as *resources* that were actively used, altered, emended, believed and – dare we use the term? – misunderstood and misrepresented by early modern propagandists of natural knowledge. With great subtlety and erudition, both of these papers have laid the groundwork for such an analysis. I do not underestimate for a moment the philological, paleographical and philosophical difficulties that they have had to conquer; indeed, I doubt that we would be able to focus our problem quite so finely were it not for their struggle with these texts. For having brought us to this point, we are all in their debt.

NOTES

[1] The image is consciously Duhemian: "By virtue of a continuous tradition, each theory passes on to the one that follows it a share of the natural classification it was able to construct, as in certain ancient games each runner handed on the lighted torch to the courier ahead of him, and this continuous tradition assures a perpetuity of life and progress for science" (Duhem 1962, pp. 32–33).
[2] For an insightful discussion of Duhem's understanding of physics in relation to history, see Martin (1990).
[3] Duhem (1962), p. 253; On *bon sens* and logic, see Martin (1987).
[4] Duhem (1962), p. 304; cf. p. 270: "By retracing for him the long series of errors and hesitations preceding the discovery of each principle, it puts him on guard against false evidence; by recalling to him the vicissitudes of the cosmological schools and by exhuming doctrines once triumphant from the oblivion in which they lie, it reminds him that the most attractive systems are only provisional representations, and not definitive explanations".
[5] It seems to have gone unnoticed that Thomas Kuhn adopts this position in Kuhn (1957), pp. 264–65. I intend to develop this observation further in a retrospective review of Kuhn's book to appear in a future issue of *Isis*.
[6] Stanley Jaki has assembled a significant quantity of very useful information – much of it in the form reviews of Duhem's books – that permits one to make this statement (Jaki 1984, chaps. 9–10). Unfortunately, Jaki's apologetic and defensive tone compromises many of the judgments he makes about Duhem and his work.
[7] See W. A. Wallace, 'Galileo and Reasoning *Ex Suppositione*', in Wallace (1981), p. 149.
[8] I owe this insight to my colleague Amos Funkenstein.
[9] See his 'Pierre Duhem on Galileo', in Wallace (1981), pp. 303–19.
[10] A. Funkenstein's important study of the transition from medieval to early modern forms of scientific and historical reasoning makes the problem of God's attributes the fulcrum of the analysis in a consistent way such that Duhem failed to provide. Furthermore, Funkenstein explicitly disavows the thesis that medieval theological speculation *necessarily* produced early modern science (see Funkenstein 1986, pp. 360–63).
[11] See, for example, Pauli (1955) and Westman (1984); Fleck (1979); Holton (1973); also De Broglie in Duhem (1962), pp. v–xiii.

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SCIENCE AND THEOLOGY IN THE FOURTEENTH CENTURY: THE SUBALTERNATE SCIENCES IN OXFORD COMMENTARIES ON THE *SENTENCES**

ABSTRACT. Both Pierre Duhem and his successors emphasized that medieval scholastics created a science of mechanics by bringing both observation and mathematical techniques to bear on natural effects. Recent research into medieval and early modern science has suggested that Aristotle's subalternate sciences also were used in this program, although the degree to which the theory of subalternation had been modified is still not entirely clear. This paper focuses on the English tradition of subalternation between 1310 and 1350, and concludes with a discussion of the theory advanced by Thomas Claxton early in the fifteenth century.

In the second of the focal essays for this conference, Pierre Duhem provides a distillation of his celebrated punctuated continuity thesis: "When we see", he says, "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 . . . 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 honour by the Italian Renaissance".[1] From his early work in the history of statics to the magnum opus of the *Système du monde*, it was Duhem's contention that the seeds of seventeenth-century science could be found in the scholastic writers of the fourteenth century. And while the thesis sustained much criticism and has undergone considerable revision, Duhem's contemporary rivals were forced to concede limited recognition of the principle; as I have noted elsewhere, even George Sarton observed that in the history of science, "there are no unbegotten fathers except Our Father in Heaven".[2]

The particular aspect of Duhem's work that I should like to focus upon here involves the application of mathematics – and particularly geometry – to the effects of nature. Duhem's historical researches and those of his successors have emphasized that medieval scholastics created a science of mechanics by bringing both observation and mathematical techniques to bear on natural effects. But while the successes of this program were evident, the foundations were certainly not: in particular,

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precisely what justified the extensive application of mathematics to natural philosophy? Medieval readers of Aristotle's *Posterior Analytics*, *Ethics*, *Metaphysics*, and *Physics* were well aware that the Philosopher had cast a dim view on the unrestrained application of techniques of one science in the domain of another. Whether this position was part of an emerging theory of scientific method that Aristotle never brought to full fruition,[3] or reflects fundamental contradictions in the historical development of his thought is still unclear.[4] But the fact remains that Aristotle's discussion presented medievals with an enormous methodological problem.

Aristotle's discussions of disciplinary autonomy carried with them a significant exception, the so-called subalternate sciences. Autonomous in their own right yet dependent upon other sciences for their first principles, the subalternate sciences comprised a relatively small class of disciplines, particularly between mathematics and natural philosophy. For a number of reasons, the medieval development of subalternation theory holds special significance in a reassessment of Duhem's historical work. First, the initial object of Duhem's work – statics – was included among these sciences, both by Aristotle and his medieval readers.[5] A far more problematic step had been taken by the end of the fourteenth century, for it seems clear that at least in some circles the new techniques of dynamics and kinematics developed during the course of that century were being associated with subalternation: in the 1380s, for example, Henry of Langenstein at Paris included the *latitudines formarum* among such sciences, adjacent in his *arbor scientiarum* to the more traditional disciplines of astronomy and music.[6] Recent research into Galileo's early thought also suggests that the subalternate sciences were a source for his ideas about scientific method and the correct reading of the book of nature, although it is still not entirely clear whether the conception of the subalternate sciences upon which Galileo was drawing was in fact the same as that which Aristotle had prescribed,[7] and as Professor McMullin has suggested, the subalternate sciences in the sixteenth century were 'an ambiguous heritage'.[8] Finally, in the intervening period, during which according to Duhem a 'superstitious archaism' was responsible for the rejection or at least neglect of earlier theories of the *moderni*,[9] we find both Pietro Pomponazzi and Galileo's teacher Francesco Buonamici lamenting the excessive injection of mathematics into natural philosophy[10] – that is, precisely the objection that Aristotle had made in the *Posterior Analytics* and which the subalternate sciences had sought to overcome.

It is rather well known that Duhem concentrated much of his historical research on the University of Paris, ignoring or at least deemphasizing the parallel achievements across the Channel. In spite of – or perhaps because of – this, I should like to focus my attention on the English and especially Oxford tradition of the subalternate sciences, particularly during the crucial years between 1310 and 1350. The source upon which I shall draw will not be commentaries on the *Posterior Analytics*, the work in which Aristotle discussed his theory of subalternation most directly, but rather medieval commentaries on Peter Lombard's *Sentences*, in which subalternation was frequently discussed in prologues. I have done so for several reasons. As a pragmatic consideration, while the *Posterior Analytics* remained a popular text in Europe during the fourteenth century, it seems to have been less popular at Oxford, where some of the most interesting scientific work was taking place. In fact, during the period 1250–1400, Oxford scholars seem to have produced fewer than half the number of commentaries on the *Analytics* that one finds at Paris during the same period.[11] Second, commentators on the *Sentences* were generally older and perhaps more mature than those in the Arts Faculty who commented on the *Analytics*. And finally, in general, discussions of subalternation found in commentaries on the *Sentences* were divorced from Aristotle's text, and thus open to considerably more interpretive latitude.[12]

At the outset, a concession is in order. Despite the considerable number of commentaries on the *Sentences* produced at Oxford during the fourteenth century, and while many contain discussions of *scientia*, not all commentators chose to discuss the subalternation of the sciences. About 1334, Robert Holcot, for example, produced a quodlibetal question 'Utrum theologia sit scientia', frequently a *topos* for discussion of subalternation, but Holcot's treatment omits any such material.[13] At Cambridge, Robert of Halifax (Franciscan, fifty-sixth lector at the Cambridge Convent, ca. 1336) included in his prologue the question, 'Utrum scientia quam potest theologus habere . . . sit practica vel speculativa'. Such questions frequently were sources for discussions of subalternation, in part because of Aristotle's reference in *Posterior Analytics* in part because of Aristotle's reference in *Posterior Analytics* though the latter is not formally subalternated to the former. Yet

Halifax's question likewise omits such material.[14] And finally, the copy of Richard Fitzralph's commentary on the *Sentences* preserved in Oxford, Oriel College MS 15 contains a prologue with a similar discussion,[15] but which once again omits reference to subalternation.[16]

When combined with the relative neglect of Aristotle's *Posterior Analytics*, one might be inclined to suggest that it is slightly at variance with the idea that periods of intense scientific change are both preceded and accompanied by appeals to philosophical analyses of scientific method.[17] In fact, however, I believe other factors are at work. First, discussions of subalternation were frequently reserved for prologues to commentaries, and as the fourteenth century progressed, such prologues often came to be reduced in length and eventually omitted entirely.[18] Doubts about the scientific status of theology and the resulting reduction in the size of prologues seem to be responsible in part for omissions of discussions of subalternation. As a result, when commentators chose to discuss the topic, they sometimes found room for it in related questions in Book I, as Adam Wodeham did, for example, in distinction I, question 3, in a reservation entitled, 'whether the same conclusion can be demonstrated in different sciences' (Utrum eadem conclusio possit demonstrari in diversis scientiis).[19] Second, in the thirty or forty years prior to 1330, the problem of subalternation had been debated thoroughly in commentaries on the *Sentences*, and a variety of positions were now available. As newer, more topically significant questions arose, subalternation passed from primary importance because it was already a topic on which a core consensus, if not unanimity rested.

If we turn now to the elements of that consensus, we find several rather interesting developments in the theory itself. The first, and clearly most pronounced, is the tremendous influence exercised by Robert Grosseteste; indeed, the extent to which fourteenth-century scholars achieved a general consensus at all is attributable to Grosseteste's authority. In almost every commentary that discusses subalternation, he is cited repeatedly, sometimes more frequently than Aristotle himself. Thus the Benedictine scholar Robert Graystanes cites Grosseteste's commentary on the *Analytics* nine times in a rather short question on subalternation, frequently quoting substantial sections of the text, while citing Aristotle's text only three times.[20] He apparently considered the commentary so important that he procured a copy for the Durham College library.[21] Graystanes's contemporary, John of

Reading, cites Grosseteste's commentary twenty-eight times, Aristotle's text only twelve times.[22] And even when commentators fail to cite Grosseteste explicitly, as is the case for Robert Cowton and William of Nottingham, it seems clear that Grosseteste's positions stood behind their texts.[23]

The reasons for this infatuation with Grosseteste's commentary are not too difficult to determine. For Franciscans like John of Reading and William of Ockham, Grosseteste's previous ties to the Order provided a fraternal link and, as I have argued elsewhere, perhaps even an authoritative text in the convent library. But for the Benedictine Graystanes and the Augustinian author of an anonymous commentary found in Balliol College MS 63, and indeed for the Franciscans as well, the motive was one of clarity and elucidation: where Aristotle's explanation of subalternation was elliptical or incomplete, Grosseteste provided a more complete mechanism. As subsequent readers saw it, Grosseteste's contributions could be summed up under four ideas. The subalternating and subalternate subjects were nonidentical, for otherwise there would be no need to transcend the original science in demonstration, and indeed one would fall victim to Aristotle's injunction against importing the elements of other disciplines. Second, these two subjects were distinguished by the so-called superadded condition that made the middle term of the syllogism the nexus of subalternation. Third, subalternate sciences were subalternated simultaneously to two superior sciences, each providing a critical element in the demonstration.[24] And finally, because of the superadded condition imposed in subalternation, the subalternate subject possessed an accidental rather than essential unity.[25]

All of these positions can be found to some degree in Aristotle's text itself. What impressed fourteenth-century readers of Grosseteste's commentary was the degree to which the latter could extend the former. Thus, in an argument dependent on the superadded condition, Robert Graystanes first quotes Aristotle, then follows with Grosseteste's elucidation:

> This is confirmed by the Philosopher, in *Posterior Analytics* I: the subalternate science always concerns being *per accidens*; whence in the same place, chapter 12, the Lincolnien [says], 'It must be known that the inferior science superadds a condition through which it appropriates to itself the subject and properties of the superior science. And there are in the conclusion of the subalternate science, as it were two natures, viz. a nature which it takes from the superior, and a proper nature which it superadds'. So far [Grosseteste].[26]

For Graystanes, as for many of his contemporaries, Grosseteste did not replace Aristotle as much as extend the Philosopher's original meaning.

Aside from Grosseteste, there was one other crucial context for discussions of subalternation. Fourteenth-century commentators frequently affiliated their discussions of subalternation with discussions of the unity of science. For example, John of Reading made subalternation the subject of two questions in his prologue and balanced them against a still more substantial question on the unity of science, referring back to his discussions of subalternation no fewer than seven times. And as I have already noted, Adam Wodeham likewise introduced an argument from subalternation into a question on the appropriateness of the same conclusions to diverse sciences. The reasons for this affiliation are not difficult to ascertain. First, Aristotle himself had associated the two topics, for it appears at least three times in *Posterior Analytics* I, particularly because it is clear that when he discusses the relationship between the subalternate and subalternating sciences, the underlying assumption is that there are well-defined criteria that distinguish the sciences.[27] Second, one should not underestimate the influence of Peter Aureol's commentary on the *Sentences* for this aspect of the discussion. Aureol had linked the two topics very closely, and his work became a target for several subsequent readers, including both Ockham from one side and Reading from another.[28] Finally, there seems to be a particular link between this discussion and the original theological text of the Lombard's *Sentences*. Fourteenth-century discussions of the unity and subalternation of the sciences in particular were frequently cast in the psychological language of the *habitus* of science. Briefly, medievals argued that corresponding to each act of the intellect, there was a habit residing in the soul that reproduced this act and thus allowed for such things as recall and cogitation. As a result, discussion of the unity of science and by extension subalternation was married to epistemological discussions of the unity of *habitus*, topics that Aristotle had treated separately in the *Posterior Analytics* and the *Categories*, respectively.[29] The broker for this marriage seems to have been, in part, Lombard's text, for the opening chapter of distinction 1 draws upon Augustine in maintaining that "every doctrine is of things or of signs" (Omnis doctrina de rebus vel de signis).[30] Clearly, the original intent of the passage was exegetical, but the door was also open to discussions of the relationships between names and things, and with them came discussions of science.

Aside from this association with discussions of unity of science, there is some evidence to suggest that early fourteenth-century commentators developed their positions on subalternation by drawing upon logical techniques developed in the middle ages. This is perhaps best exemplified by drawing once again upon John of Reading. Reading begins his discussion of subalternation by reviewing the now-familiar criteria for subalternation that he has gleaned from Robert Grosseteste, and in particular, the notion that the subalternate science must be subsumed under two subalternating sciences. The requirement of the superadded condition imposed on the subalternate subject precluded an inferior science that was merely a contraction of the superior. This leads John to an extended discussion of predication, in the course of which he lists seven ways by which logical ascent and descent can occur in superior and inferior sciences. It would take us too far afield to discuss each of these, but the significant fact is that John concludes that only one of them – descent under a quidditative concept that is *per se* one to a concept that is one *per accidens* and that joins the higher subject with an accidental property – produces subalternation.[31] It should, in fact, come as no surprise that John concludes that these are Grosseteste's criteria dressed up in more recent garb. But in fact, the purpose of John's discussion and the reason for his reference to logical ascent and descent was to distinguish between the descent and ascent of supposition theory and the doctrine of subalternation, for at least in John's account, there seems to have been some conflation of these issues in the years immediately preceding John's tenure at Oxford.[32]

It is rather well known that Duhem was often not kindly disposed to medieval uses of logic, particularly at Oxford. On occasion, he was inclined to see the use of logic as going beyond legitimate purpose and becoming an artificial exercise, a 'logical acrobatics'.[33] But at least in the case just cited, Duhem probably would have agreed that Reading's extensive logical discussion served the purpose of setting the theory of subalternation on firm ground and avoiding unwarranted and illegitimate claims on the theory.

John's purpose, at least in this context, was to guard against unlimited appeals to subalternation as a technique justifying cross-disciplinary work. But at about the same time – that is, about 1320 – the association of subalternation with discussions of the unity of science and with the notion of scientific habits, seems to have produced some inclination to expand the criteria for subalternation of one science to another. William of Ockham, for example, called attention to a strict and broad sense of subalternation, and in the first question of his prologue noted that while Aristotle mentioned explicitly only a few pairs of subalternating/subalternate sciences, he intended to permit cross-disciplinary work in other sciences subordinated in other ways.[34] Likewise, despite John of Reading's determination to hold a conservative line on the expansion of subalternation, even he notes in his discussion of generic descent under a subject that while the inferior is "not properly and completely subalternated to the [superior science], . . . in a certain way perhaps it can be said [to do so]".[35] The anonymous author of a commentary in Balliol 63, while discussing the relationship between two sciences, one of which depends on the other for the certitude of its truths, notes that this is "not properly subalternation, but nevertheless . . . has a similarity with subalternation".[36]

Although to my knowledge, no one in the early fourteenth century explicitly tied the expansion of subalternation to the new attempts to quantify qualities, statements such as these may well have convinced proponents that those attempts were legitimate. It is hardly surprising that such ideas might be held implicitly rather than explicitly, for if indeed there was consensus on the theory of subalternation, it was on this issue of cross-disciplinary transmission that consensus was tested most severely. I have already alluded to John of Reading's otherwise conservative tendencies. His contemporary, Robert Graystanes, was even more direct: speaking of the resolution of principles within the subalternate and subalternating sciences, Graystanes notes that strictly speaking, such resolution cannot occur except within the proper genus. "For", as he points out, "the geometer cannot resolve beyond what his faculty permits; rather, it is proper that resolution should remain primarily within its own limits . . .".[37] Writing at approximately the same time, but in London, Walter Chatton expressed much the same sentiments in a direct response to Ockham's more liberal discussion.[38] If fourteenth-century scholars found that subalternation provided room for legitimate growth in new techniques, they retained a healthy caution about extending it too far.

On several occasions in this paper, I have referred to a consensus about subalternation. In doing so, I do not wish to imply that there was agreement in every detail of the theory, for as we have seen, certain aspects were open to continued discussion; the consensus rather extended to a core of material that I have tried to identify. In view of that, I should like to conclude by discussing the ideas of a Dominican scholar at Oxford from the early fifteenth century, Thomas Claxton.[39] Claxton's work has often been taken as the culmination of a revival of Thomism at Oxford early in the fifteenth century,[40] but while this may be true in general, it would seem that his ideas about subalternation and the unity of science depend as much on his reading of subsequent fourteenth-century sources as a review of Aquinas. The second question to Claxton's prologue investigates the well-worn issue of whether the theology that we possess in this life is in fact *scientia*, and in his own autographed copy preserved in Cambridge, Gonville and Caius College MS 370(592), he devotes some twenty-seven closely written folios to the issue. He begins by proposing six principal arguments against the view: it is not demonstrative, nor is it evident, intuitive, and abstract; it is neither practical nor speculative; it is not numerically one; and finally, it has no determinate subject.[41] He proposes to treat each of the issues plainly, openly, and truthfully, so that his students should understand, for, as he tells us, the theologians of old very often were deficient in logic. And despite his long narration of these six arguments, he maintains that he will not dwell on prior opinions excessively, since only those which by merit of fame or nearness to truth must be treated. Others, he trusts, will be refuted by apparent truths, since if a little error in the beginning leads to a great one at the end, so also truth always easily declares itself.[42]

In his preliminary definitions, Claxton says that there are three genuine senses of science and one that is spurious. Science is first, the evident assent of the mind to what is, was, or will be. Second, in the strictest sense, it may also be evident assent that is caused syllogistically, and this, he says, is what Aristotle had in mind when he referred to science as the cognition of a conclusion in a demonstration. Third, it may refer to a habit of the mind created from many assents collected together, each having a certain coordination to the other by comparing the first subject and predicate. This, he says, is the way Aristotle speaks of natural science in the *Physics*, the *Metaphysics*, *De caelo et mundo*, and many other places. All three are equally valid ways of referring to science. As for the spurious sense, Claxton says that some doctors of theology suggest that science is merely true assent without vacillation, whether evident or inevident. This allows them to call faith science, but Claxton notes that this is false, because it does not make sufficient distinction between opinion and science. And although Augustine speaks in this way, he does so *modo vulgaris loquendo*, and this sort of expression ought to be avoided by those in the schools.[43]

It is in his responses to the fifth and sixth principal arguments – on the unity and subject of science – that Claxton discusses subalternation. A science, or for that matter, anything can be one in several senses: one numerically *per se*, one specifically, one generically, one *per conclavationem* (literally, being locked up together), or one by attribution and coordination.[44] The latter he singles out for special consideration, because he says this sense of unity is a bit more restricted and is in fact the way sciences – he lists natural philosophy, ethics, arithmetic, and music – are said to be one by attribution or coordination to one first principle, that is, the first proposition in which the primary predicate equally (although not really) is predicated. And in a reference to a popular fourteenth-century analogy that derives ultimately from the *Metaphysics*, he says this sort of unity is like that of the people of England, since they have one king, one law, one polity.[45]

Claxton then gives a more concrete example, the case of natural philosophy and medicine. Both have the same first principle in which the primary predicate (the so-called *tertium adiacens*) is predicated, namely "Some body is mobile" ('Aliquod corpus est mobile'). But they are not the same science, because the first predicate is applied to natural philosophy and medicine through separate first principles: in natural philosophy according to place, quantity, etc., in medicine according to health and infirmity or the excesses of the humors and other such things. Thus, in spite of their attribution to one first principle, the coordination is held under different species, and they are not one science. One science, says Claxton – echoing Aristotle – is of one genus, that is, of one subject and the things joined to it by proper first principles.[46]

This discussion of the unity of science leads Claxton to subalternation. In subalternation, one needs not only the same subject in each science, but also equally significant first proper principles whose subjects and predicates are *per se* superior and inferior. For example, medicine, he says, is subalternated to natural philosophy because under the latter's first principle 'corpus est mobile', medicine has its own first principle, 'corpus animale est mobile ad infirmitatem vel sanitatem vel neutralitatem', in which both subject and predicate are related as superior and inferior. The same thing is true of geometry and perspective, through the respective first principles 'figura est terminata' and 'linea visiva terminatur ad oculum faciens visionem'. But, Claxton adds, another part of perspective is subalternated to natural philosophy under a different first principle, viz., 'animal est mobile ad visionem a corpore lucido sibi obiecto'.[47] It is clear that what allows Claxton to speak in this way about partial subalternation is once again the propositional notion of science and a unity based on attribution and coordination, although elsewhere it is clear that he takes issue with previously formulated versions of such ideas, particularly those of Ockham and Holcot.[48]

Still later, Claxton elaborates on this subordination of principles. In his response to the sixth principal argument of the question, concerning the relationship between the theology of the wayfarer and the blessed, he notes that no science is subalternated to another unless the principles of the subalternating and subalternate sciences compare by means of an addition (*habet se ex additione*) as plainly in the subject as in the predicate. But clearly in the two theologies concerned, there are not two such differentiated subjects, but an identical one.[49] Although he does not cite Robert Grosseteste anywhere in this question – perhaps an indication of the distance between discussions in the opening years of the fifteenth century and those of a century earlier – his ideas about subalternation are consistent with the received tradition that derived ultimately from the bishop of Lincoln. In this sense, Claxton's commentary represents still a further distillation of the late medieval tradition of subalternation.

It should be clear that this tradition was vitally dependent upon the theological context in which it developed, an issue that returns us once again to Duhem. As Stanley Jaki has reminded us in his recent biography,[50] Duhem's ideas about medieval science were inseparable from his understanding of medieval theology. The fuller treatment of this theological tradition of subalternation would require equal consideration of positions developed at Paris and elsewhere during the fourteenth century, for indeed in addition to native positions, Claxton cites the imported ideas of Gregory of Rimini and other Parisians.[51] But this must remain for another occasion.

NOTES

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1 Duhem (1987), p. 341.

2 Livesey (1987), p. 365; Sarton (1959), p. 38.

3 McKirahan (1978).

4 Graham (1987). The structure of his thesis is set out at pp. 14–17.

5 *Posterior Analytics* I. 13 78^{b}38. Version P of the so-called *Liber Jordani de ponderibus*, which Duhem was particularly keen to attribute to Jordanus or at least suggest stood behind Jordanus' work, begins, "Cum scientia de ponderibus sit subalternata tam geometrie quam philosophie naturali, opertet in hac scientia quedam philosophice, quedam geometrice probari." See Moody and Clagett (1960), p.151. Duhem (1905–1906), vol. 1, pp. 128–32.

6 Henry of Langenstein, *Expositio prologi Bibliae*; Vienna, Österreichische Nationalbibliothek CUP 3900, fol. 49vb. See Steneck (1975).

7 James G. Lennox (1986) attempts to tie Galileo to the tradition of the mixed sciences and presents a stimulating discussion of Aristotle's position but fails to consider whether the tradition received by Galileo was in fact developed further by medieval scholastics. William Wallace's recent research (in particular [1981] and [1984]) has demonstrated that Galileo's early work was heavily influenced by Jesuit scholars at the Collegio Romano. But a glance at the sources cited by one of Galileo's chief Jesuit sources, Paulus Valla, suggests that the tradition upon which Valla and perhaps his colleagues were drawing was developed in medieval commentaries on the *Sentences*, not exclusively the *Posterior Analytics*.

8 McMullin (1978), esp. at 211–12.

9 Duhem (1987), p. 340.

10 Pomponazzi (1525), fol. 6va, 9va–vb; Buonamici (1591), p. 525.

11 By my own count, based on surviving commentaries listed in Lohr (1967–1974), there were some thirty-six commentaries on the *Posterior Analytics* produced at Paris during the period, against only sixteen at Oxford.

12 Concerning this latitude, see Glorieux (1941) at 1863–1865.

13 Muckle (1958). The date of Holcot's work has been the subject of much discussion. See Courtenay (1987), p. 252 n. 5 and 268–69.

14 Paris, BN lat. 15880, fol. 36ra–38vb; BN lat. 14514, fol. 277vb–280rb. Concerning Halifax, see Courtenay (1987), p. 272.

15 Oxford, Oriel College 15, fol. 3ra–4va. This prologue is best treated as an anonymous one, since Fitzralph's authorship has been rejected by Leff (1963), p. 176.

16 One might also add Stephen Patrington's *Repertorium diversorum auctorum* (Florence, B. Laurenziana MS Plut. XVII sin. cod. 10) to this list. Patrington's work, written before 1389, is a handy source for investigating particular topics discussed in the fourteenth century, since it is preceded by an index of topics which he excerpted. While it contains a good deal of material on *scientia* (Sections 35a–36a; pp. 33–35) and especially *scientia large et proprie* (Section 35b2; p. 34), Patrington does not seem to have included subalternation.

In all such cases, however, an element of caution is in order. While such treatments of *scientia* do not contain explicit references to subalternation, they are frequently valuable for ancillary materials. For example, in Halifax's treatment just mentioned, it is clear that his position on the definition of speculative and practical sciences is somewhat flexible, and thus coordinates with the flexibility in the definition of subalternation that will be discussed below. The prologue in Oriel 15, fol. 3rb contains a definition of *scientia* that might be compared with the one reviewed retrospectively by Thomas Claxton early in the next century.

A somewhat more useful discussion is found in Walter Chatton's *Reportatio* from about 1322–23. See below, note 38.

[17] Kuhn (1970), pp. 87–88.

[18] Courtenay (1987), pp. 251–58.

[19] Adam Wodeham: forthcoming, d. I, q. 3, pp. 226–50 at 246. I should like to thank Dr. Wood for allowing me prepublication use of her edition. I have examined the section of Cambridge, Gonville and Caius College MS 281(674) that contains this portion of the text (fol. 137ra–rb), and one might note that the argument based on subalternation seems to have been singled out in the margin for special attention by one reader.

[20] Robert Graystanes, *Questiones super Sententias*, Prologue, q. 3; London, Westminster Abbey MS 13, pp. 141b–143a. Like many of the questions in this commentary, question 3 contains a lengthy addition to article 2 which is to be found at pp. 156b–157b. Graystanes lectured on the *Sentences* probably around 1322. For a description of the manuscript, see Robinson and James (1909), p. 72. For a list of the questions contained in the text, see Kennedy (1986). Concerning Graystanes's career, see Emden (1957–1959), vol. 2, p. 814 and Tachau (1988), pp. 161, 209.

[21] In the earliest catalogue of Durham College books, we find the record "... Lyncolniensis super librum posteriorum et expositio super metaphisica ex procuratione eiusdem", where Graystanes is identified in the preceding record; Blakiston (1896), p. 37. The volume was also included in the catalogue made ca. 1390: "Lincolniensis super libros posteriorum et Fernandus super methephisicam in j volumine." See Salter *et al.* (1942), p. 243. Given Graystanes's use of Grosseteste's commentary in his commentary on the *Sentences*, it would be interesting to inspect this volume, and the further identification of Fernandus de Hispania as the author of the second commentary would facilitate this. But Fernandus's *Metaphysics* commentary survives in only one known copy, Oxford, Merton College MS 281, which unfortunately seems never to have contained Grosseteste's commentary on the *Posterior Analytics*.

[22] Concerning Reading's career, see Livesey (1989), esp. chapter I. For a discussion of Reading's use of Grosseteste, see chapter II.1.

[23] Thus, for example, Cowton refers to the thing superadded to the object of the subalternating science, a feature of Grosseteste's theory that becomes virtually universal in the fourteenth century. See *In Sententias. Prologus*, q. 2; Theissing (1970), p. 262, lin. 4–6. William of Nottingham's discussion of subalternation occurs in q. 1, a. 3 of his commentary; Cambridge, Gonville and Caius College MS 300(514) fol. 4ra–5rb. At fol. 5ra, he notes, "semper scientia subalternata addit aliquam rationem extraneam super subiectum scientie subalternantis". Somewhat more perplexing is William of Alnwick's treatment in Prologue, q. 5, a. 1 (Assisi, B. Communale 172, fol. 29v-34r). Alnwick neither cites Grosseteste explicitly nor uses arguments suggesting familiarity with his commentary. But the discussion occurs in response to Aquinas's position, and elsewhere, in question

1, it is clear that Alnwick's position on subalternation is governed by his underlying contention that the real problem of faith and reason lay not in this or that mechanism of subalternation, but in their compossibility at all. This question has been edited by d'Souza (1973); note especially the brief discussion of subalternation there at p. 475.

[24] Despite Grosseteste's authority, some aspects of the theory were open to modification. The anonymous author of Merton College MS 103 (produced around 1300) cites Grosseteste in his treatment of subalternation (fol. 216ra–216va), but also gives four conditions for subalternation, the second of which is (fol. 216ra–rb): "Secunda condicio est quod principia scientie subalternantis descendant in principia scientie subalternate et in omnes conclusiones eius, ita quod nulla sit conclusio in subalternata quin possit probari per principia subalternantis, et propter hoc medicina non subalternatur geometrie, quia non quoad omnes conclusiones dependet ex principiis geometrie, licet quoad aliquas, ut quod vulnera circularia tardius sanantur." But under such a condition, it is difficult to see how a science should be subalternated simultaneously to two superior sciences.

[25] Robert Grosseteste (1981), pp. 148–50. For a more complete discussion of Grosseteste's theory of subalternation, see Laird (1983), chapter II and Livesey (1989), chapter II.2.

[26] Robert Graystanes, *Questiones super Senentias*, Prologue, q. 3; London, Westminster Abbey MS 13, p. 142b: "Confirmatur per Philosophum I *Posteriorum*; scientia subalternata semper est de ente per accidens, unde Lincolniensis ibi et est c. 12, 'sciendum quod scientia inferior superaddit condicionem per quam appropriat sibi subiectum et passiones superioris scientie. Et sunt in conclusione scientie subalternate, sicud due nature, natura scilicet quam accipit a superiori et natura propria quam superaddit.' Hec isti."

[27] Note, for example, that the discussion of subalternation first appears at *Posterior Analytics* I.7, where Aristotle introduces the topic by appealing to the three elements of demonstration: the conclusion, the axioms, and the underlying genus. And medieval scholars, John of Reading especially, drew special attention to Aristotle's discussion in I.28 on the unity of science and its cohesion. Much the same can be said of Aristotle's discussion in I.13, where the theory of subalternation is most elaborate.

[28] It would take us too far afield to discuss Aureol's positions on both issues. I have discussed them and the objections made by Ockham and Reading in Livesey (1989), chapter III.4 and in Livesey (1985).

[29] For the latter, see *Metaphysics* IV.2 1003^b19–23 and especially *Categories* VIII 8^b28–34.

[30] Peter Lombard (1971), T. I, pars ii, p.55; Augustine (1962), I, c. 2, n. 2 (p.7).

[31] John of Reading, *Scriptum in I librum Sententiarum*, Prol. q. 6; Florence, BN Centrale, Conv. Soppr. D.IV.95, pp. 86–87. I have edited the text of this question in Livesey (1989).

[32] For a more extensive discussion of this argument, see Livesey (1989), chapter II, section 6.

[33] Duhem (1913), p. 442: "La solution des sophismes se présente donc comme un légitime exercice de Logique, tant qu'elle demeure un exercice. Mais la gymnastique qui ne se propose plus simplement de fortifier et d'assouplir le corps, la gymnastique qui cesse d'être un moyen et se prend pour une fin, devient acrobatie; de même, en toute étude, l'exercice artificiel qui perd de vue l'objet réel pour lequel il a été combiné devient une acrobatie; ainsi la casuistique morale ou juridique peut dégénérer en acrobatie, ainsi la solution des problèmes peut prêter à l'acrobatie mathématique et la solution des sophismes à l'acrobatie logique.

Au temps de Guillaume Heytesbury, cette acrobatie logique était le *sport* en vogue à l'École d'Oxford."

[34] William of Ockham (1970), Prol., q. 1 (pp. 14–15). William of Ockham (1974), III, ii, chap. 21 (pp. 541–42). The influence of Ockham's ideas on subsequent writers in the fourteenth century has been a hotly debated topic. Duhem, of course, regarded Ockham as a seminal figure for much of the scientific work of the fourteenth century, a position that found a number of supporters in subsequent generations of historians. Weisheipl (1968) in particular and Herman Shapiro (1957) attempted to show the relationship between Ockham's positions on crucial physical and metaphysical issues and the calculatory tradition. But more recently, Courtenay (1987) and Tachau (1988) have called attention to the fundamental differences between Ockham and his reputed followers, arguing that the existence of an 'Ockhamist school' may have been assumed too quickly. On the issue of subalternation, one can find early opponents of Ockham, such as Chatton (see note 38 below), whose arguments seem to derive more from an underlying disagreement about *metabasis* than from the mechanism of subalternation. Such a fundamental debate is one that very likely will be with us for some time, but certainly one additional piece of evidence that needs to be considered in greater detail is the commentary on the *Sentences* found in Merton College MS 284.

From the paper on which the text was written, it is clear that the commentary was produced after 1340, and based on other considerations, probably before the mid-century. The text seems to have been a notebook of some anonymous student, who selected excerpts from other previous commentaries, perhaps as a way to prepare for his own. But what is also clear is that Ockham's position figures prominently, and when other positions are given, Ockham is granted the last word. The text itself closely resembles the *Ordinatio*, although it differs in ways that led Gerard Etzkorn (1987) to suggest that the author may have been using the lost *Reportatio* of book I.

Unfortunately, the sections of Prologue, qq. 1, 11, and 12 that contained material on subalternation were not excerpted by the author, leading one again to the fact that about the middle of the century the topic seems to have suffered a decline in interest. But question 2 of MS 284, corresponding to question 2 of Ockham's prologue, contains much material on the definition of *scientia* and the distinction between *quia* and *propter quid* demonstration, issues that were central to both Aristotle's and Ockham's ideas about subalternation. In particular, at fol. 7v, an objection to Ockham's theory of cognition is made, viz., if Ockham were correct, only one experience would be necessary to know a principle of art and science; Ockham's resolution is likewise given, that if the principle falls under the most specific species, then certainly only one experience suffices. But if the principle falls under what is common to several *species*, several experiences are required, following what Aristotle says in *Posterior Analytics* II on induction. Likewise, at fol. 9v, Ockham notes the fundamental distinction between *quia* and *propter quid* demonstration: the same thing is proved, but through different media. At fol. 10r, he follows up what has been said about individual and universal cognition by defining particular and universal demonstration, providing examples of each, the second of which relies heavily on the temporal element in the demonstration. And Ockham's tendency to reinterpret Aristotle is also exemplified shortly thereafter (at fol. 10r), when in the context of Aristotle's statement that to know is to understand the cause of the thing, Ockham notes that not every demonstrable should have a cause properly speaking; rather, it suffices that there should be something prior to which the thing agrees primarily in predication.

[35] John of Reading, *Scriptum in I librum Sententiarum*, Prol. q. 6; Florence, BN Centrale, Conv. Soppr. D.IV.95, p. 86: "Scientia ergo de superiori et inferiori nec est una nisi sit genere relato, nec se habet sicud scientia subalternans et subalternata. Assumptum – quod scilicet scientia de inferiori non subalternetur scientie de superiori, non sic proprie subalternata illi et complete, licet aliquo modo posset forte dici – patet."

[36] Oxford, Balliol College MS 63, fol. 67r–85v at 70r: "Unde si non sit hic proprie subalternatio, est tamen hic quedam subalternatio que similitudinem habet cum subalternatione." F. Pelster (1955, at pp. 30–31) concluded that the author was an Augustinian at Oxford, and while Roger Mynors suggested that MS 63 as a whole probably was produced not long after 1330, it is somewhat difficult to determine the date of this commentary. At fol. 69r, it refers to Peter Aureol, which places the text after 1317; but other than William de Ware, whose commentary is still earlier than this, the other authors cited in the text cannot be identified. It is clear that elsewhere the author was relying upon Aegidius Romanus for his information about subalternation, although as I have noted above, there is a heavy dose of Grosseteste in the commentary as well. The author notes that there are three modes of subalternation: (1) in which the inferior science serves (*famulatur*) the superior science, (2) in which the inferior science adds a condition to the subject of the superior, and (3) in which the inferior and the superior sciences consider the same truth, the former *modo grosso*, the latter *modo subtili*. (1) and (3) are mentioned by John of Reading in question 7 of his prologue, immediately after discussion of Richard Connington's theory (Florence, BN Centrale, Conv. Soppr. D.IV.95, p. 93); in the text, the position is not attributed to a specific author, and may well have been a common one at Oxford at the time.

[37] Graystanes, *Questiones super Sententias*, Prologue, q. 3; London, Westminster Abbey MS 13, p. 143a: "Respondeo negando hanc consequentiam. 'Visio linee in verbo est perfectissima eius cognitio. Ergo subalternat sibi omnes alias notitias de verbo.' Sed oportet addere quod visio in verbo esset causa aliarum cognitionum quod non est verum in proposito. Unde eadem conclusio potest cognosci perfectius et imperfectius ut forte methaphysicus, quia cognoscit per tres causas aliquam conclusionem, cognoscit perfectius quam geometer, qui cognoscit per unam causam tantum. Ipsi enim considerant multas easdem conclusionis cum scientia unius conclusionis; una non est subalternata alteri eiusdem conclusionis, ut cognitio quam habet geometer de alia cognitione ad illam quam habet metaphysicus de eadem, ut enim prius habitum est. Subalternans est principium respectu subalternate, et eadem conclusio non potest esse principium respectu sui ipsius. Maior etiam non est verum nisi intelligitur sic, quod resolutio in cognoscentibus non stat nisi ad perfectum cognoscibile vel perfectissimam cognitionem illius generis. Non enim potest geometer resolvere ulterius quam permittit sua facultas, sed oportet quod resolutio sua stet in prima limites suos".

[38] In his *Reportatio*, Prologue, qu. 1, a. 3, Chatton refers to Ockham's view that while the same proposition cannot be proven in distinct sciences when one defines science as the habit of one conclusion, it can so pertain to different sciences when science is taken as a collection of habits. Thereupon, Chatton divides his response into two parts, one restricted only to theology, the other to sciences in general, and with respect to the latter, he argues that such propositions proved in distinct sciences are not similar *in essendo* but rather in *significando*. "For although by possessing several sciences, one can apply premises which one forms according to concepts formed in the other, nevertheless the practitioner in any science . . . will form the conclusion from cognitions of a different

ratio in being from the conclusion of any other science." And in direct response to Ockham's argument, Chatton observes that even supposing that the conclusions are subordinated (he does not use – here or elsewhere – the term subalternation), still the conclusion is proved through middle terms of different *rationes* by different practitioners. See the text edited by Reina (1970), at 296–302.

[39] For details on Claxton's life, see Emden (1957–1959), vol. 3, p. 426. Claxton was at the Oxford Convent by 1404 and a regent master in 1413. His commentary gives a retrospective look at the previous century, and thus offers not merely a picture of current views, but also fifteenth-century criticisms of the early fourteenth-century positions outlined in this paper.

[40] Courtenay (1987), p. 363.

[41] Cambridge, Gonville and Caius College, MS 370(592) fol. 10v–14r.

[42] Ibid., fol. 14r: "In ista questione, sic procedam: primo enim declarabo materias tactas in titulo questionis nude, aperte, et vere, ut studentes intelligant. Veteres enim theologi sicut in loquendi logica plurimum defecerunt. Sic veritates inventas vel quas crediderunt se invenisse in culto et imperito modo loquendi sequentibus se tradiderunt. Unde et parvus error in principio in maximum dilatatus est, qua ex causa Philosophus III *Metaphysice* c. 11 in textu commenti 6. Veteres philosophos, theologos ut orpheum, ysiodum, et philosophos etiam reprobat naturales qui adinventas veritates sub tegumentis poeticis vel mathematicis a sequatibus suis velaverunt. Opiniones autem paucas tangam, quia solum eas que merito fame aut propinquitatis ad veritatem tangende sunt. Alie enim scita veritate faciliter reprobantur, quia sicut falsitas inconveniens plurimis se involuit, ut patet ex sententia Philosophi I *Celi et mundi* dicentis parvus error in principio maximus est in fine, sic et veritas semper facile vincit et se declarat."

[43] Ibid., fol. 16r–v.

[44] Ibid., fol. 28v.

[45] Ibid., fol. 28v.

[46] Ibid., fol. 28v–29r.

[47] Ibid., fol. 29r: "Ex hiis patet quod ad hoc quod aliqua scientia sit alteri subalterna son solum opertet quod sit de subiecto eodem de quo est scientia cui subalternatur sed quod habeat equipollenter aliquod principium proprium primum cuius subiectum sit per se inferius ad subiectum et predicatum per se inferius ad predicatum primi principii illius scientie cui subalternatur. Verbi gratia, philosophia medicinalis subalternatur philosophia naturali quia sub isto principio eius primo 'corpus est mobile' habet primum suum principium proprium 'corpus animale est mobile ad infirmitatem vel sanitatem vel neutralitatem,' sicut perspectiva sub hoc principio geometrie, 'figura est terminata,' capit hoc principium sibi proprium 'linea visiva terminatur ad oculum faciens visionem,' et ideo perspectiva subalternatur geometrie, licet secundum aliquas eius partes subalternetur philosophie naturali capiens istud principium suum sub primo principio eius dicto 'animal est mobile ad visionem a corpore lucido sibi obiecto.' Similiter ars de ponderibus capit proprium principium suum hoc, scilicet 'corpus gravius elevat levius' sub primo principio philosophie naturalis, et ideo sibi subalternatur. Et sic de musica que capit hoc suum principium, 'ex proportione 1 ad 4 fit diapason et plenus tonus,' sub hoc principio arismetice, 'omnis numerus est alteri proportionalis' et ideo sibi subalternatur, et sic de aliis".

[48] Thus, at fol. 31v, Claxton resolves an objection that his notion of unity of science is too strong, allowing all the sciences in the world to be one numerically in the way that

geometry is one; he replies that although all the sciences in the world might be one by aggregation, this is not the way that geometry is one, since sciences are said to be one not merely by aggregation, but by aggregation *and coordination* to one first principle, and such coordination is not found in diverse sciences.

Claxton's objections to Holcot and Ockham occur in the response to the sixth principal argument, at fol. 32r and 35v.

[49] Ibid., fol. 36r: "Sexta conclusio est hec: Viatorum theologia non est sanctorum theologie subalterna. Probatur ista conclusio sic. Nulla est facultas vel scientia alteri subalterna nisi cuius principium habet se ex additione tam aperte subiecti quam aperte predicati vel realiter vel equivalenter ad primum principium alterius vel cui subalternatur. Sed non sic se habet theologia viatorum ad theologiam beatorum. Ergo non est sibi subalterna. Patet consequentia et maior ex premissis et minor sic probatur. Idem est subiectum theologie viatorum et theologie beatorum, et utriusque theologie primum principium alteri equipollet. Ergo primum principium theologie viatorum non est inferius primo principio theologie beatorum, scilicet ea inferiorite que requiritur ad subalternationem. Patet consequentia et antecedens per hoc, quod utriusque theologie est primum principium 'Deus est summum bonum possibile.'" He then sums up his earlier remarks that subalternation is not merely dependent on a simple relationship of superiority and inferiority of certainty: "Septima conclusio est hec: licet viatorum theologia sit inferior id est imperfectior quam beatorum theologia, non tamen est ei subalterna. Ista conclusio patet ex premissis".

[50] Jaki (1984), pp. 393–400.

[51] For example, at fol. 35v.

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CHRISTOPHER CLAVIUS AND THE CLASSIFICATION OF SCIENCES

ABSTRACT. I discuss two questions: (1) would Duhem have accepted the thesis of the continuity of scientific methodology? and (2) to what extent is the Oxford tradition of classification/subalternation of sciences continuous with early modern science? I argue that Duhem would have been surprised by the claim that scientific methodology is continuous; he expected at best only a continuity of physical theories, which he was trying to isolate from the perpetual fluctuations of methods and metaphysics. I also argue that the evidence does not support the conclusion that early modern doctrines about mathematics and physics are continuous with the subalternation of sciences from Grosseteste, Bacon, and the theologians of fourteenth-century Oxford. The official and dominant context for early modern scientific methodology seems to have been progressive Thomism, and early modern thinkers seem to have pitted themselves against it.

When considering the various historical doctrines relating science and mathematics, we should keep in mind three important facts. (1) Early modern science considered mathematics as the foundation of physics or natural philosophy – witness Galileo's famous assertion that "the great book of nature is written in the language of mathematics and its characters are triangles, circles, and other geometrical figures" (1960, p. 25). (2) It wasn't always that way. Aristotle in the *Physics* discussed how the mathematician differs from the physicist (1930, II, chap. 2). He asserted that physicists deal with physical bodies and their essential attributes; physicists treat of surfaces and volumes, lines and points, but as the limits of physical bodies. Mathematicians also treat of surfaces and volumes, points and lines, but not as physical, separating them from their essential attributes and from motion. Geometry investigates physical lines, but not qua physical; the more physical branches of mathematics such as optics, harmonics, and astronomy, investigate mathematical lines, qua physical, not qua mathematical. Instead of mathematics being the foundation of physics, Aristotle conceived of mathematics and physics as different sciences separated by their different objects. And (3) the medievals were not univocal in their support of the Aristotelian position. They interpreted Aristotle's remarks so variously that they can be considered as making up at least two distinct

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traditions, roughly that of Thomas Aquinas and that of Robert Grosseteste.

Thomists standardly held that physics, metaphysics, and mathematics are not part of one large science, but constitute three (or more) radically different sciences, differing in their subject and method: metaphysics considers being in common, physics considers natural being, and mathematics considers quantified being. Some such schema was adopted widely, with many variations. What all such schemas had in common was the rejection of a universal science (whether or not based on mathematics). Mathematics usually filled the lowest rank in these classifications (typically, the least perfect of the speculative sciences). Mathematical sciences, such as astronomy, astrology, and optics were called middle sciences because they were thought to occupy a middle position between mathematics and physics; they were thought to depend on mathematics, but also to consider the mathematical object as applied to a physical object. The typical doctrine was that the middle sciences were more mathematical than physical, since the mathematical object was more essential to them than their condition of application. Thus, the middle sciences would have been unfit to provide support for physics. Implicit in all this is the doctrine that a higher science cannot derive its principles from a lower science, so that physics cannot derive principles from mathematics or from any middle science.

The followers of Grosseteste, including Roger Bacon and scholars from fourteenth-century Oxford, held a doctrine that could easily have been derived from the writings of Aristotle, but that seems discontinuous with the Thomist line. While agreeing with the basic intuition that the higher sciences provide the reason for the lower sciences (the subalternated sciences), Grosseteste disagreed about the status of the important composite sciences. He argued that composite sciences have an additional nature about which the higher sciences say nothing; ultimately he asserted that only mathematics can provide the reason for a subalternated science and even for natural philosophy. Roger Bacon followed him in this. In the *Opus Majus* and *Opus Tertium*, Bacon detailed a view of human knowledge as a hierarchy of knowledge in which mathematics is antecedent to natural philosophy and to metaphysics: "without mathematics no science can be had" (1859, p. 35; also 1928, I, p. 109). Steven Livesey, in his paper, discusses this tradition in fourteenth-century Oxford.

All three above facts can be subsumed under the heading of scientific methodology and have relevance to the now-popular variation to Duhem's thesis of the continuity of physical theory, namely, the thesis of the continuity of scientific methodology. The questions I wish to pose are: (1) would Duhem have accepted such a thesis? and (2) to what extent is the Oxford tradition of classification/subalternation continuous with early modern science? The answer to the first question is relatively simple. Duhem would have been surprised by the claim that scientific methodology is continuous; he expected at best only a continuity of physical theories, which he was trying to isolate from the perpetual fluctuations of methods and metaphysics. Duhem addressed the issue of the classification or subalternation of sciences in medieval science, though in bits and pieces, here and there. What he said allows us to surmise his views. In the *Système du Monde*, Duhem spoke harshly about Roger Bacon's arguments against infinite divisibility, which use the certainty of geometry to oppose the existence of indivisibles. Duhem then praised John Buridan's view, in which "the proposition, continuous magnitude is not composed of indivisibles, is not viewed by Buridan as a corollary whose truth is assured by the necessity of not contradicting geometry"; Duhem said that Buridan "sees in it a principle whose truth the geometer is obliged to admit in order to construct his science . . . . Far from geometry's certainty guaranteeing the truth of the proposition, it is the truth of geometry that is subordinated to the correctness of the proposition; and the correctness of the proposition is not for geometry, but for physics or metaphysics to establish" (1985, pp. 19–20). Duhem detailed an interpretation of Buridan as keeping separate a geometry which considers lines and surfaces as nothing but constructions of the mind and a physical geometry, in conformity with reality, which only treats bodies. By reasoning about the former, we achieve results in conformity with measurements carried out on real bodies (1985, pp. 32–33). But Duhem recognized that Buridan's views were not accepted by his successors: "Doubtless Buridan's notion was too profound since it does not appear to have been adopted by even his most faithful disciples. Albert of Saxony and Marsilius of Inghen . . . did not hesitate to rely on geometry in order to refute the hypothesis [of infinite divisibility]" (1985, p. 20).

The answer to the second question is considerably more complex (and controversial). Setting aside the general question of the continuity of scientific methodology, I wish to ask specifically about the early modern scholastic doctrines concerning the relations between mathematics and physics.

Now, the broad outlines of seventeenth-century Scholasticism were Thomist. There was a renaissance in Thomistic philosophy during the second half of the sixteenth century. This renaissance was felt most strongly in Jesuit philosophy. Saint Ignacius of Loyola, founder of the Jesuits, advised the Jesuits to follow the doctrines of Saint Thomas in theology. Naturally, it would be difficult to follow Saint Thomas in theology without also accepting much of Aquinas's and Aristotle's philosophy. Loyola's advice was made formal in the Jesuits' *Ratio studiorum* of 1586: "In logic, natural philosophy, ethics, and metaphysics, Aristotle's doctrine is to be followed" (Rochemonteix 1889, vol. IV, p. 8n).

The flavor of the advice can be captured through a circular from the chief of the Order of Jesuits to the Superiors of the Order, written just after the end of the Council of Trent and imbued with the spirit of the Council and Loyola's advice. The circular announces specific doctrines "that must be held in theology and in philosophy", for example, "Let no one defend anything against the axioms received by the philosophers, such as: there are only four kinds of causes; there are only four elements; there are only three principles of natural things; fire is hot and dry; air is humid and hot" (Rochemonteix 1889, vol. IV, pp. 4n–6n). These 'axioms' are sufficient to banish Stoic, Epicurean, and Atomist philosophies; moreover, the circular also rejects doctrines that might have been accepted by non-Thomist scholastics – Ockhamists, for instance: "Let no one defend anything against the most common opinion of the philosophers and theologians, for example, that natural agents act at a distance without a medium." The circular continues with specific opinions that Jesuits must teach and hold as true – all in conformity with Thomist doctrines and against Averroist and Franciscan doctrines – but the crux of the matter seems to have been: "Let no one introduce any new opinion in philosophy or theology without consulting the Superior or Prefect." The circular ends with: "Let all professors conform to these prescriptions; let them say nothing against the propositions here announced, either in public or in private; under no pretext, not even that of piety or truth, should they teach anything other than that these texts are established and defined. This is not just an admonition, but a teaching that we impose."

Later circulars reaffirmed the same position. For example, the Jesuit Thomist stance is upheld even in a discussion of the thorny question of divergent authorities. The following can be read in a circular from another General of the Jesuits, to the Superiors, written in order to express clearly the basic tenets underlying the *Ratio studiorum* of 1586:

No doubt we do not judge that, in the teaching of scholastic theology we must prohibit the opinion of other authors when they are more probable and more commonly received than those of Saint Thomas. Yet because his authority, his doctrine, is so sure and most generally approved, the recommendations of our Constitutions require us to follow him *ordinarily*. That is why all his opinions whatever they may be . . . , can be defended and should not be abandoned except after lengthy examination and for serious reasons. (Rochemonteix 1889, vol. IV, pp. 11n–12n)

This interpretation of Loyola's advice draws a fine line between following Thomas's opinions *ordinarily* and abandoning them for extraordinary reasons, after lengthy examination. But the circular continues: "One should have as the primary goal in teaching to firm up the faith and to develop piety. Therefore, no one shall teach anything not in conformity with the Church and received traditions, or that can diminish the vigor of the faith or the ardor of a solid piety." The intent of the circular is clear. The primary goal in teaching is the maintenance of the faith, and nothing should be allowed to interfere with it. And since the received traditions are known to conform to the faith, they should be taught and novelties are to be avoided. The circular continues:

Let us try, even when there is nothing to fear for faith and piety, to avoid having anyone suspect us of wanting to create something new or teaching a new doctrine. Therefore no one shall defend any opinion that goes against the axioms received in philosophy or in theology, or against that which the majority of competent men would judge is the common sentiment of the theological schools. . . . Let no one adopt new opinions in the questions already treated by other authors; similarly, let no one introduce new questions in the matters related in some way to religion or having some importance, without first consulting the Prefect of studies or the Superior.

It is not surprising that the philosophy textbooks written by Jesuit authors – those of Portuguese Jesuits, the Coimbrans, and such Collegio Romano Jesuits as Franciscus Toletus – though not identical with one another, generally preserved basic Thomistic doctrines. The same can be said about the question of the classification of sciences as reflected in the *Ratio studiorum*, including the key questions about the utility of mathematics to natural philosophy and the status of such mathematical sciences as astronomy and optics.

The extreme Jesuit view about mathematics and natural philosophy can be represented by Ludovico Carbone (a non-Jesuit) (1599; cf. Wallace, 1984, especially pp. 126–48). Carbone details eleven doubts about the mathematical sciences. Some of these doubts concern the type of abstraction characteristic of mathematics: mathematicians consider bare quantity without any connection to substance; the intelligible matter they arrive at when they set aside sensible matter is merely fictive and cannot be defined in terms of true genus and difference; they abstract from being and the good; they abstract from motion and the natural forces that produce it; they abstract from all kinds of cause and so cannot use causal reasoning in any of their demonstrations. (1599, pp. 240–43). Though Carbone was not a Jesuit, he studied at the Collegio Germanico, annexed to the Jesuits' Collegio Romano. His works were influenced (perhaps overly so) by those of Collegio Romano professors. In any case, one can find similar views in the works of some Jesuits, Piccolomini and Pereira, for example (see Crombie 1977.) Carbone's views about mathematics and the mathematical (or middle) sciences fit very well with sixteenth-century scholastic doctrines, of Thomist descent, about the order and classification of the sciences.

It is against this background that Christopher Clavius proposed his reform of mathematics, arguing its importance to natural philosophy. In an essay for the *Ratio studiorum* on the teaching of mathematical disciplines he wrote:

> Physics cannot be understood correctly without [the mathematical disciplines], especially what pertains to that part concerning the number and motion of the celestial orbs, of the multitude of intelligences, of the effect of the stars, which depend on the various conjunctions, oppositions, and other distances between them, of the division of continuous quantities to infinity, of the tides, of the winds, of comets, the rainbow, halos, and other meteorological matters, of the proportion of motions, qualities, actions, passions, reactions, etc., concerning which the *calculatores* wrote much. (1901, p. 472)

In the same vein, Clavius disputed the common opinions that the mathematical sciences are too abstract and fictive:

> It will contribute much to this if the teachers of philosophy abstained from those questions which do not help in the understanding of natural things and very much detract from the authority of mathematical disciplines in the eyes of the students, such as those in which they teach that mathematical sciences are not sciences, do not have demonstrations, abstract from being and the good, etc. (1901, p. 471)

Obviously Clavius has in mind the kind of views represented by Carbone and others. This is significant, since Clavius was responsible for the training of Jesuit mathematics professors and the content of their teaching.

Clavius concentrated on showing that mathematics and mathematical sciences are useful and more certain than the other sciences. However, a science's degree of certainty is not proportional to its degree of perfection, so that, in spite of his pronouncement that physics cannot be understood correctly without the mathematical sciences, his view does not alter the basic Thomistic scheme. That scheme claims that the subjects of the sciences are different, and more or less perfect; it is consistent with it that a less perfect science, such as mathematics, might have results that are certain, with respect to a particular subject, such as abstract or quantified being. The difference between Clavius and Carbone is not really a difference of theory but a difference of emphasis within the same general theory. Clavius, the champion of mathematics in the Collegio Romano, does not seem to appeal to the Oxford doctrine of classification/subalternation in order to defend mathematics.

It would be pleasant to think that the early modern doctrines about the relations between mathematics and physics are continuous with the subalternation of sciences from Grosseteste, Bacon and the theologians of fourteenth-century Oxford. Unfortunately, thus far the evidence does not support such a conclusion. The official and dominant scholastic context for early modern doctrines seems to have been progressive Thomism, and early modern thinkers such as Descartes seem to have pitted themselves directly against it. From his earliest writings, the 'Private Thoughts', for instance, we have Descartes's dream of a chain of sciences that would be no more difficult to retain than a series of numbers (1974, p. 214), and from Rule I of the *Rules for the Direction of the Mind*, we have an explicit denial of the doctrine that the sciences should be distinguished by the diversity of their subjects, "all the sciences being in effect only human wisdom, which always remains one and identical to itself, however different are the objects to which it is applied" (1974, p. 360).

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THE REALISM THAT DUHEM REJECTED IN COPERNICUS

ABSTRACT. Pierre Duhem rejected unambiguously the strong version of realism that he believed was held by Copernicus. In fact, although Copernicus believed that his theory was clearly superior to Ptolemy's, he seems to have recognized that his theory was at best only approximately true. Accordingly, he recognized that his arguments were not demonstrative in the traditional sense but probable and persuasive. Duhem regarded even the belief in probably true explanations as misguided. Nevertheless, Duhem recognized that, even if metaphysical intuition does not enter into the content of physical theories, the rejection of hypotheses could be explained only by appeal to common sense. Hence, Duhem held a qualified instrumentalism according to which physical theories are not realist, but the terms of ordinary experience and empirical laws are realist. Accordingly, Duhem rejected the complete subordination of science to philosophy as well as the complete separation of science from philosophy. Duhem's history of cosmological doctrines reflects his belief in the origin of the subordination of science to philosophy and of the struggle to achieve the proper balance without being driven to the opposite extreme of their complete separation.

1. INTRODUCTION

The figures of Copernicus and Galileo hover over two of Pierre Duhem's multi-volume historical studies like specters making timely appearances as Duhem discovered and uncovered a text that in some way anticipated a concept found clearly for the first time in our modern heroes (*Etudes*; *Système*). In Duhem's plan those earlier texts created a possibility that with time became more probable and then was to become certain as Copernicus and Galileo appear in full, adopting an available and prepared conceptual space, defining its content and fulfilling an expectation. There was nothing inevitable about these developments; on the other hand, they did not emerge *ex nihilo*. As it turned out, Duhem never completed his most ambitious drama and, perhaps worse, he did not live to produce a definitive, if you will, cinematic version, leaving us instead with an often diffuse and repetitious series of sketches in which the leading characters make only cameo appearances.

Be that as it may, these are the Copernicus and Galileo of Duhem's histories, and it is with them that we must be content. In the works on

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which this examination primarily rests, *The Aim and Structure of Physical Theory* and *To Save the Phenomena*, Copernicus and Galileo play something of a symbolic role for Duhem as representatives of a commitment to a version of realism of which Duhem definitely disapproved. Their cameo appearances in his major historical surveys occasionally suggest a somewhat less stereotypical portrayal and a far more positive evaluation, yet Duhem apparently believed that the childishly naive realism of the commonsense view of science could be traced to Copernicus and Galileo. These authors require separate treatment; I will focus on Copernicus even though Duhem made little distinction between them on the question of their realism. Moreover, the problems concerning Duhem's positivism and realism are complicated even more by the intricately tangled relations of his philosophy, science, and history. My aim is to define as precisely as possible Duhem's views on realism. My procedure involves (1) identifying the realism that Duhem rejected in Copernicus, (2) comparing Duhem's reading of Copernicus with recent interpretations of Copernicus, and (3) projecting the result into conditions that a realist interpretation of Duhem must meet if it is to be consistent with his principles and practice. The risk in trying to project how Duhem's account might look today is obvious, but I trust that the distinction between explication and appropriation can be made sufficiently clear so as to avoid misunderstanding and deception.

2. THE REALISM THAT DUHEM REJECTED IN COPERNICUS

The exposition follows the order in which the two works mentioned above appeared. In *Aim and Structure*, Copernicus first appears in a context where Duhem cites St. Thomas Aquinas approvingly for having held that astronomical hypotheses that save the appearances are not necessarily true nor are they sufficiently *demonstrable*, for it is possible that the appearances might be better preserved by some other hypothesis yet unknown by men. Duhem points out that this opinion agrees with a number of passages in Copernicus and Rheticus. Duhem explains that in the *Commentariolus* Copernicus presents the fixity of the sun and the mobility of the earth as postulates that the reader is asked to concede. Of course, Duhem adds that in *De Revolutionibus* Copernicus "professes an opinion concerning the reality of his hypotheses which is less reserved than the doctrine inherited from Scholasticism and expounded in the *Commentariolus*" (*AS*, pp. 41–42). After citing Osiander's 'Letter to the Reader', Duhem criticizes Kepler's disapproval in the following words: "This enthusiastic and somewhat naive confidence in the boundless power of the physical method is prominent among the great discoverers who inaugurated the seventeenth century" (p. 42). Duhem concludes the paragraph with a quotation from Cardinal Bellarmine's letter to Foscarini about speaking *ex suppositione*, and Duhem comments: "In this passage Bellarmin [sic] maintained the distinction, familiar to the Scholastics, between the physical method and the metaphysical method, a distinction which to Galileo was no more than a subterfuge" (p. 43).

The passages and opinions to which I have just referred appear in the second part of Chapter 3 on representative theories and the history of physics. Here Duhem attempts to explain the role of natural classifications and explanations in the evolution of physical theory, and in the second part of that chapter he contrasts the views of physicists and philosophers on representation and explanation. Duhem makes it clear that even the natural classifications towards which he sees physical theory evolving are not explanations (pp. 31–32). Although the explanation of a theory yields to another explanation, it is in the representative part that Duhem locates what appears as a natural classification. Duhem's preference for the empiricist reading of Newton's law of universal gravitation, a reading that Duhem commends to the physicists of the nineteenth century, shows that he does not regard natural classifications as explanatory, nor is there any indication that he views physical theories as requiring the discovery of the causes of phenomena (pp. 47–49). On the contrary, Duhem complains that in the nineteenth century, hypothetical theories offered as more or less probable explanations of phenomena led to an extraordinary multiplication of such theories. He comments: "The noise of their battles and the fracas of their collapse have wearied physicists and led them gradually back to the sound doctrines Newton had expressed so forcefully" (p. 53). What doctrines? Certainly not the inductive method without hypotheses, which Duhem elsewhere deplores (pp. 190–200, 284), but deductive and inductive procedures by way of hypotheses that lead to a condensed representation. Such a representation is not explanatory, nor does it lay bare the causes of phenomena. Duhem is wary not only of causal explanation but even of probable explanation (pp. 37, 53). These he expects to rise and fall, but that which corresponds to natural classification is found in the representative parts:

It is not to this explanatory part that theory owes its power and fertility; far from it. Everything good in the theory, by virtue of which it appears as a natural classification and confers on it the power to anticipate experience, is found in the representative part; all that was discovered by the physicist while he forgot about the search for explanation. On the other hand, whatever is false in the theory and contradicted by the facts is found above all in the explanatory part; the physicist has brought error into it, led by a desire to take hold of realities . . . .

When the progress of experimental physics goes counter to a theory and compels it to be modified or transformed, the purely representative part enters nearly whole in the new theory, . . . whereas the explanatory part falls out in order to give way to another explanation.

Thus, by virtue of a continuous tradition, each theory passes on to the one that follows it a share of the natural classification it was able to construct, . . . and this continuous tradition assures a perpetuity of life and progress for science.

This continuity of tradition is not visible to the superficial observer due to the constant breaking-out of explanations which arise only to be quelled. (pp. 32–33)

The remainder of Duhem's substantive comments about Copernicus in *Aim and Structure* serves to support this depiction of the evolution of physical theories. The role of the Copernican Revolution in the evolution of the principle of universal attraction consists in the destruction of the geocentric system. What survives of Copernicus's speculations about a natural appetition or sympathy in celestial bodies is not the cause of the phenomenon, and not the analogy of the motion of iron toward a magnet, but rather the idea that follows almost as a corollary to the rejection of geocentricity, namely, that other bodies must be like the earth (pp. 225–31; compare *Système*, X, p. 320).

Before trying to draw conclusions from this summary of *Aim and Structure*, we must consider Duhem's appendices to the second edition published in 1914. In the preface to the second edition, Duhem claims that he had not been brought to doubt his principles but that time had given him an opportunity to make them precise and to develop them, referring to one article published about a year after the first appearance of *Aim and Structure* in serial form and a second article published in 1908 as "clarifications and additions" (p. xvii).

Inasmuch as *To Save the Phenomena*, the next work to be examined, did not appear until 1908, the consideration of the articles appended to *Aim and Structure* now will not disturb the order of the explication.

In the first article, 'Physics of a Believer', Duhem acknowledges that in order to legitimate the assertion that as physical theory progresses, it becomes ever more similar to a natural classification as its ideal end, he must appeal to metaphysics (*AS*, p. 298). The legitimation of the assertion and its further elaboration exceed the limits of his methods as a physicist. The propositions of cosmology and the theorems of physics are radically heterogeneous, that is, they can neither agree with nor contradict one another. "However, between two propositions bearing on terms of different natures it is nevertheless possible that there would be an *analogy*, and it is such an analogy which ought to connect cosmology with theoretic physics" (p. 301). Duhem goes on to explain that this analogy obtains only "between the metaphysical explanation of the inanimate world and the perfect physical theory arrived at the state of a natural classification" (p. 302). We do not and never will possess this perfect theory. Duhem's severe restrictions here are apparently intended to caution the philosopher that no proof is possible, that his theoretic scaffolding is shaky, and that he must have an accurate and minute acquaintance with physical theory (pp. 302–3). In addition, he must know its history because his challenge is to understand theory and its development well enough to be able to perceive trends, that is, to judge the tendency of theory and to surmise the goal towards which it is directed. "So the history of physics lets us suspect a few traits of the ideal theory to which scientific progress tends, that is, the natural classification which will be a sort of reflection of cosmology" (p. 303). In the concluding section of this essay Duhem allows himself to speculate about the analogy that he sees between general thermodynamics and the essential doctrines of Aristotelian physics; "we recognize in these two doctrines", he concludes, "two pictures of the same ontological order, distinct because they are each taken from a different point of view, but in no way discordant" (p. 310).

In the final essay, 'The Value of Physical Theory', Duhem adds the following reflections. Only of propositions which claim to assert empirical facts can we say that they are true or false (*AS*, pp. 333–34). Of propositions introduced by a theory we can say that they are neither true nor false, but only convenient or inconvenient (p. 334). No physicist, however, is entirely satisfied with this state of affairs. Physical theory confers on us a certain knowledge of the external world which cannot be reduced to merely empirical knowledge nor to the utility of the theory. Duhem concludes that it would be unreasonable to work for the progress of physical theory unless this theory were the increasingly better defined and more precise reflection of a metaphysics, unless physical theory tends to a natural classification, the nature of which corresponds by analogy to a certain supremely eminent order (pp. 334–35).

On the assumption that these clarifications and additions are consistent with *Aim and Structure*, these are the conclusions that can be drawn. A natural classification is not causally explanatory and does not appeal to causes as explanations of the phenomena ordered in physical theory. Duhem consistently regarded physical theories as representations or condensations of laws and phenomena. The evolution of physical theory depends on hypothesis and experimentation, but no mechanical rules suffice to assure progress. The dangers that Duhem perceived in metaphysics, metaphysical commitments, and the desire for explanation are typically empiricist: dogmatism, premature closure, and the fact that metaphysical speculations are even more resistant to definitive correction. Whereas a hypothesis or even a theory will be forever abandoned, once-abandoned metaphysical conceptions are often rehabilitated. On the other hand, the drive to perfect physical theory, the fact that hypotheses are rejected, and that over time there is progress suggested to Duhem that there is a natural classification toward which physical theory tends, but that the justification for this surmise could be located only in a kind of metaphysical intuition. In Duhem's opinion, Copernicus fit in the history of scientific progress, but his belief in the absolute truth of his hypotheses was not only unnecessary and excessive but might have been disastrous if not for Osiander's 'Letter'. Kepler's view was naive, and Galileo's agreement with the pragmatic reading of hypotheses was disingenuous.

In *To Save the Phenomena*, Duhem treats Copernicus more extensively and spells out in more detail his understanding of Copernicus's arguments and beliefs. From Copernicus's dedication to the Pope in *De Revolutionibus*, Duhem takes Copernicus to assert not only that hypotheses should be true, but that Copernicus believed that he had succeeded in *proving demonstratively* the truth of his hypotheses, that is, demonstrative proof as understood by Duhem – only by way of uniquely true hypotheses. In order to do so, Duhem objects, Copernicus would have had to show that his hypotheses were not merely sufficient but even necessary for saving the phenomena. Duhem admits that Copernicus at best implied this larger claim, but Rheticus was quite explicit on the point, namely that astronomy should be constructed on hypotheses that are founded in the very nature of things as the causes of the observed phenomena (pp. 61–65).

Now if we consider these claims along with Duhem's appreciation for Osiander's opinions about hypotheses and with the fact that between 1571 and 1582 toleration of hypotheses was ebbing, it is clear that

Duhem's central concern is the freedom of hypotheses from philosophical and theological constraints. In other words, Duhem sees Copernicus's realism as a two-edged sword. If astronomy is subject to theory, whether theological or physical, then the only hypotheses that will be permitted are those that are in conformity with the prevailing theory and the prevailing realism. Duhem's principal objection to realism is that it requires absolute, apodictic certainty, and once someone believes himself to be in possession of such certainty, he imposes this view on other sciences. Hence, Duhem objects to the claim of absolute truth. Such a claim closes off inquiry and places too many constraints on imagination. Duhem deplores the philosophical-theological imperialism of the prevailing realism of the second half of the sixteenth and the first half of the seventeenth century when, in his view, a mistake was made that was not corrected until the nineteenth century.

The conclusion of *To Save the Phenomena*, however, introduces an important qualification and a significant twist (pp. 113–17). Duhem takes the single assertion that astronomical hypotheses should be physically true and distinguishes two propositions: (1) that the hypotheses of astronomy are judgments about the nature of heavenly things and their real movements, or (2) that the experimental method can serve as a control on the correctness of astronomical hypotheses and thereby come to enrich our cosmological knowledge with new truths (p. 116). Duhem retains the view that Copernicus held the assertion in the first sense, a view which Duhem judges to be illogical, but manifest and seductive. Then comes the new twist: Beneath this clear but false and dangerous sense lay the idea of the unification of the theories of celestial and terrestrial motions. This true but hidden meaning of the same principle gave birth to the scientific efforts of Newton whose dynamics by means of a single set of mathematical formulae represents the motions of the stars, of the tides, and of falling bodies. The final paragraph reads:

> Despite Kepler and Galileo, we believe today, with Osiander and Bellarmine, that the hypotheses of physics are mere mathematical contrivances devised for the purpose of saving the phenomena. But thanks to Kepler and Galileo, we now require that they save *all the phenomena* of the inanimate universe *together*. (p. 117)

Do these references to Kepler's and Galileo's efforts mean that metaphysical knowledge has entered into the content of physical theory? I think not. Once the motions of the planets, tides, and falling bodies have been represented successfully by Newton's dynamics, the true idea of unification emerges from the false and dangerous sense which Copernicus held. In other words, we have physical laws that are provisional and approximate and hence not true and that approach a natural classification to which corresponds analogously and *only* analogously a metaphysical order or cosmological knowledge to which alone we can ascribe truth.

If there was any inclination to interpret Duhem's second reading of the assertion about hypotheses as realist, Duhem's reference to Newton, his characterization of the result as a representation, and his continued insistence on physical hypotheses as mathematical contrivances suggest that his view here is broadly consistent with the one presented in *Aim and Structure*. In sum, the realism that Duhem attributes to Copernicus and that he consistently rejects can be collected in the following assertions: (1) Astronomical hypotheses must be true, and (2) Astronomical hypotheses must be demonstrated to be true, that is, the hypotheses must be sufficient and necessary to save the phenomena as the causes of the phenomena. In other words, Duhem clearly attributes to Copernicus the view that hypotheses are not only convenient and not merely possibly true, but are absolutely true. According to Duhem, Copernicus believed in the absolute, infallible, and unrevisable truth of his hypotheses, and that he believed himself to have demonstrated the truth of his hypotheses.

3. RECENT INTERPRETATIONS OF COPERNICUS

Duhem's instrumentalist interpretation of ancient astronomy has been criticized clearly elsewhere (Lloyd, Mittelstraß). Duhem's account of Copernicus does not seem to be consistent with recent interpretations of Copernicus, even between two interpretations which are in some respects hostile to one another. Edward Rosen rejected the exclusively fictionalist interpretation of Ptolemaic astronomy, and he commended Copernicus for remaining silent on the question of the reality of his own epicycles and deferents (1984, chap. 3 and p. 59; 1961, pp. 93–94). It might be added, however, that in either case the answer would have constituted an embarrassment for Copernicus. Noel Swerdlow and Otto

Neugebauer (to whom I refer later briefly as 'Swerdlow' for reasons stated by Neugebauer himself in his prefatory remarks) interpret Copernicus's truth-claims concerning hypotheses as probable, fallible, and revisable in details if not in principles (I, pp. 19–21).

In view of Duhem's positivist inclinations, it is at least a little strange that as he surveyed the development of cosmology, he did not see fit to examine the empirical evolution of astronomy in more detail. What few figures Duhem uses in *Le Système* are schematic and far too qualitative to permit any insight into the solution of mathematical-astronomical problems. This observation provokes a number of doubts, but the obvious answer is probably the correct one, namely, that Duhem's history is not the story of the better representations, because these were only approximate and could serve only as hints of later, more adequate representations. Swerdlow's recent commentary, of course, emphasizes the solution of technical problems, and he claims that it was only with the appearance of Regiomontanus's *Epitome* in 1496 that mathematical astronomy in Europe was reborn (I, p. 54).

Experts disagree on this last point, but whatever the difficulties with Swerdlow's interpretation, he has provided at least one reason for Copernicus's hesitation about publication that conflicts with Duhem's account. If Copernicus had concluded that he had indeed demonstrated the truth of the hypothesis of the moving earth, then why would he have expressed doubt and fear about its reception? As Swerdlow so incisively puts it, "Copernicus was no fool". (After that comment we may imagine Swerdlow muttering, "So take that, Luther".) Copernicus was convinced that he was right, claims Swerdlow, but he also knew that his arguments and mathematical proofs were probable at best (I, p. 20). When Copernicus asserted that the observations of the bounded elongations of Mercury and Venus and of the retrogradations of all of the planets followed directly and necessarily from the hypothesis of the orbit of the earth around the sun, he concluded that all of these phenomena proceed from the same cause. But the cause to which he immediately refers is the motion of the earth, not the cause of the motion of the earth nor the cause of any other motion for that matter (*De Revolutionibus* I, 10). Copernicus had no demonstration, because Copernicus could not demonstrate the cause of the motion of the earth. Copernicus's argument, then, rests, first, on the fact that directly observable consequences follow from the hypothesis of an orbiting earth and, second, on assigning priority to the directness of these observations over the observation of terrestrial motions, which he justified by reintroducing standard late medieval doubts about the supposed simplicity of terrestrial motions (*De Revolutionibus* I, 8).

Why did Copernicus get no further? Kepler has provided the definitive answer in my opinion: Copernicus believed that Ptolemy's models were correct and that his task was to preserve their effects (Swerdlow; Hartner). Swerdlow appropriates Kepler's answer and adds that even though Copernicus probably made far more observations than is usually thought (another point on which there is disagreement among the experts), he derived parameters for already invented models that were slightly modified. It was not Copernicus's intention to construct models that were actually appropriate to the motions of the planets, because he believed that Ptolemy's descriptions of phenomena were correct and that the models that represent them were at least theoretically accurate (Swerdlow I, pp. 36–38, 77–85). Finally, we need to consider the import of remarks made by Rheticus in 1551 in which he reports that Copernicus complained about the accuracy of his own observations and that he had come to question the accuracy and veracity of many observations of the ancients. The remark probably refers to the period of the late 1530s when Copernicus was altering his work to make it numerically as internally consistent as possible. Such reservations are not found in *De Revolutionibus*, but if the report is true, it would mean that Copernicus had come to realize that his theory was not accurate. There may well have been other problems that concerned Copernicus, but we need not resort to speculation to confirm Copernicus's doubts about the publication of his book or about the demonstrability of the heliocentric hypothesis. The point is that Copernicus's confidence in the truth of the hypothesis of an orbiting earth was unaffected, even though he apparently realized that the perfection of the planetary theory that his hypothesis required would have to be left to future astronomers (Swerdlow I, p. 20).

What this means is that the realism that Duhem attributed to Copernicus is only partly correct. Copernicus believed that astronomical hypotheses should be true; he believed that the hypothesis of an orbiting earth was true; but we have little persuasive evidence that Copernicus believed that he had demonstrated, in any traditional sense, the truth of his hypothesis. On the contrary, the evidence rather indicates that Copernicus knew that his arguments were at best probable and only more or less persuasive, and for that reason a dialectical-rhetorical strategy (in the tradition of the *Topics*) was necessary (compare with Westman). As for his belief in the absolute truth of his hypothesis, in Duhem's view the consequences might have been disastrous if not for Osiander. Consider the reply that had Copernicus not believed that much, we have good reason to believe that he might not have published his book at all. As to what he knew, however, Copernicus *apparently* held the view that astronomical science could progress by postulating hypotheses that were more rather than less probable and by making ever more accurate observations. On the other hand, Duhem is correct, in my view, that Copernicus saw the *ultimate* goal as the construction of physically causal explanations of observed motions. But back to the first hand again, even if it was only a provisional view in light of his self-perceived failure to prove the heliocentric theory, Copernicus's acquiescence to the probable truth of his theory means that Copernicus himself believed that absolute truth is not a necessary condition of progress. Alas, even this more modest view was rejected by Duhem.

4. MINIMAL CONDITIONS FOR A REALIST INTERPRETATION OF DUHEM

Duhem consistently reserved the judgment of truth or falsity to empirical assertions, preferring to assess hypotheses as to their convenience or inconvenience. None of his assertions about progress, natural classification, and a metaphysical order suggests that physical theory aims or should aim at causal explanation or at causes of observed phenomena. We do think in terms of causes, and although such an inclination suggests some profound natural imperative, Duhem was very reluctant to impart to this activity a role any more concrete than an attraction, drive, or intuition.

According to some standard distinctions between the truth-claims of various versions of scientific realism, if we focus on the theoretical terms of science, we must conclude that Duhem rejected metaphysical realism, semantic realism, and epistemic realism (Merrill):

Metaphysical Realism: The entities postulated by a (good or acceptable) scientific theory really exist. Alternatively: the theoretical terms of science denote actually existing entities. *Semantic Realism*: We must interpret scientific theories realistically – i.e., we must take the theoretical terms of science to function as denoting terms. *Epistemic Realism*: To accept a theory is to believe that it is true, to believe that its terms denote existing entities. Alternatively: to have good reason for holding a theory is to have good reason for holding that the entities postulated by the theory really exist. (p. 229)

Had he supported any of these unambiguously, then surely Duhem would have had no difficulty with assertions about the truth of theories, the existence of causes and theoretical entities, literal interpretations of laws and theories, or that evidence for a theory implies belief that the theory is true or an accurate description of reality.

On the other hand, if we focus on the observation terms of a theory and on all of the commonsense terms of our experience and on what Duhem styled "empirical laws – meaning the laws of ordinary experience which common sense formulates without recourse to scientific theories" (*AS*, p. 283), then Duhem approved realism in metaphysical, semantic, and epistemic senses. Alternatively, we might represent Duhem's view about physical theories as allowing him to accept semantic realism while rejecting metaphysical and epistemic realism (Merrill, p. 232). But Duhem explicitly rejected the suggestion that laws may be regarded as true without commitment to existence-claims about the entities postulated in the theory, because physical laws are always provisional and approximate (*AS*, p. 172). Hence, Duhem seems self-consistent in maintaining a *qualified* instrumentalism as regards physical theories and laws while holding a qualified, if regulative, realism as regards our ordinary experience and deeper metaphysical intuitions. This is, I believe, one of the sources of the confusion in discussions of Duhem's positivism and realism. This distinction, that is, between scientific theories and empirical laws, is the basis of Duhem's view that physical theory and metaphysical doctrines have no common terms; between judgments having no common terms but bearing on the same subjects there can be neither agreement nor disagreement.

Although experimental facts are more refined, more theory-dependent, and more abstract than simple observation statements, physical theory and metaphysical doctrines are connected in some fashion at the level of observation, inasmuch as theories correlate abstract ideas with the really observed facts (*AS*, p. 147). That there is at least some connection at the level of observation and yet disparity in the correspondence between abstract symbol and concrete fact (p. 151) seems to render adequation asymptotic and always incomplete (pp. 154–58). These and other limitations led Duhem to his extraordinary caution. In more general terms, Duhem was very sensitive to the problem that the revisability of physical theories makes truth-claims paradoxical. On the other hand, the inaccessibility of unobservable entities leaves the analogy intuited by metaphysical doctrine unable to satisfy proof-conditions. Whatever the dangers, however, the connections are what Duhem exploits to compensate for the limitations of physical theory and metaphysics. The logical problem concerning crucial experiments, for instance, does not mean that hypotheses are never rejected on rational grounds, but rather that logic and the rules of experimental method do not possess the resources for justifying conclusively the abandonment of a hypothesis (Ariew, pp. 322–23). The analogy between physical theory and cosmology can aid the philosopher in his selection of cosmological doctrine as it seems to correspond to the natural classification towards which he sees the arrangement of experimental laws advancing.

Earlier I characterized Duhem's account of Copernicus as consistent with his philosophical views. But inasmuch as the account of Copernicus is only partly correct and, furthermore, the fact that Duhem's history of fictional hypotheses is questionable, the correction demands a reformulation of Duhem's judgments. The truth-claims or judgments of probable truth made by the astronomer or physical theorist are mistaken only if made *qua* astronomer or physical theorist. But, as Duhem himself recognized, even the astronomer and physical theorist make judgments that can be justified only by appeal to metaphysics. Duhem's history of cosmological doctrines is the history of the correct positivist philosophy of science and the history of its relation with metaphysics (compare with Paul). There were two extremes that Duhem rejected: (1) the complete subordination of science to philosophy and (2) the absolute and total separation of science from philosophy.

As a consequence, Duhem's history unfolds in the following stages: (1) In the most ancient speculations known to us, philosophy was linked inseparably with the science of nature and with the science of number and figure. (2) During that period and certainly by the time of late antiquity, the exact sciences became more detailed and difficult, leading to a distinction, but no separation, between science and metaphysics. (3) During the Renaissance, however, occurred an overreaction to this distinction that made science subordinate to theological and philosophical realism. (4) Subsequently, much of philosophy developed independently and, emptying itself of the content to which it owed its solidity, appeared to fly off with the slightest effort. In other words, the subordination of science to philosophy contributed to the independence of *philosophy*. Hence, by the nineteenth century the picture looked like this: Most of philosophy was unsupported and unnourished by science; science remained for some subordinate to philosophy and for others completely separate from philosophy; and finally there were some bold individuals who were taking up the task of once again linking science with philosophy and mathematics without the subordination of science to philosophy (*AS*, pp. 312–13).

For Duhem, then, the history of the continuity that he had traced and the history of a continuing tradition that he was making constituted the story of the gradual rectification of two extremes and the story of the restoration of the delicate balance between representation and explanation, a balance that Duhem believed was essential to the advance of physical theory, metaphysics, and history itself. The extent of Duhem's positivism and of his fear of premature closure and dogmatism is revealed in his rejection of even probable explanation, but Duhem clearly believed that *his* view *was* the balanced one. The problem and tension recognized and experienced by Duhem remain as the principal obstacles to the reconciliation of empiricism with scientific realism.

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COPERNICUS, THE ORBS, AND THE EQUANT

ABSTRACT: I argue that Copernicus accepted the reality of celestial spheres on the grounds that the equant problem is unintelligible except as a problem about real spheres. The same considerations point to a number of generally unnoticed liabilities of Copernican astronomy, especially gaps between the spheres, and the failure of some spheres to obey the principle that their natural motion is to rotate. These difficulties may be additional reasons for Copernicus's reluctance to publish, and also stand in the way of strict realism as applied to *De Revolutionibus*, although a realistic astronomy may be envisioned as a goal for Copernicus's research program.

In the previous paper André Goddu presents Duhem's views on the progress of science, connecting his views on Copernicus with some recent scholarship and examining the varieties of realism this renders possible. I want to suggest that this recent work on Copernicus misses certain important considerations by unduly emphasizing mathematical astronomy over cosmology and physics. The same issues have consequences for realism, and display valuable features of Duhem's image of science.

When Copernicus explains his motives, in the preface to *De Revolutionibus* addressed to Pope Paul III, he contrasts his views with two other schools of thought but he criticizes these alternatives on different grounds. The homocentric models revived by Amico and Fracastoro fail to show numerical agreement with positional data. On the other hand, although the eccentric models of the Ptolemaic tradition do show numerical agreement, they "contravene the first principle of regularity of motion" (Copernicus 1976, p. 25). As this objection is not raised in the case of the homocentric models, we may conclude that they do not contravene this principle, which concerns motion, the basic subject matter of physics as defined by Aristotle, not just astronomy or the motions found in astronomy.

The objection to the Ptolemaic tradition concerns the nature of the motion required by the equant. There seems to be no difficulty with the equant, regarded as a constraint on the motion of a point in a two-dimensional mathematical construction used for calculating planetary

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longitudes. If there is a difficulty it is because of the connection of the motions with physics, and the "first principle" invoked by Copernicus is an attempt to express the difficulty. Modern commentators have not quite got hold of the problem. Swerdlow for example describes the equant as an offense against "simple mechanical sense" (1972, p. 36). In the work cited by Goddu, Swerdlow and Neugebauer describe the objection to the equant as being on "physical or mechanical rather than on merely philosophical grounds" (1984, p. 290). These remarks must be understood ahistorically. There was no canonical mechanics for early modern scientists to draw upon for such judgments. To understand the equant problem, then, we need to examine the physics of motion employed by Copernicus.

Copernicus evidently expected his audience to be familiar with Aristotle's account of motion.[1] The aspects of the account relevant for understanding Copernicus's objections to Ptolemy are: all bodies move, either because they are subject to their own internal source of motion, or because they are moved by something else, which in its turn moves on account of an internal source. Further, a body may only produce motion in another body when the two are in contact. Copernicus is attempting to give an account of the motion of the planets, which are not themselves endowed with a source of motion.[2] Therefore planets are moved by something else.

In *De Revolutionibus*, Book I, Chapter Four, Copernicus discusses stations and retrogressions as apparent irregularities in the paths of the planets. Planets also appear sometimes nearer and sometimes farther from the earth. "Nevertheless", he goes on, "it must be admitted that their motions are circular, or compounded of a number of circles, because they pass through irregularities of this kind in accordance with a definite law and with fixed returns to their original positions, which would not happen if they were not circular" (1976, p. 39). But planets do not move themselves; they are moved by something else. What other object is capable of moving a planet in indefinitely repeating circles? Chapter Four begins, "The next point is that the motion of the heavenly bodies is circular. *For the movement of a sphere is a revolution in a circle*, expressing its shape by the very action, in the simplest of figures, where neither beginning nor end is to be found, nor can the one be distinguished from the other, as it moves always in the same place" (1976, p. 38, emphasis added). Hence we are to understand that the planets move in circles because they are moved by spheres. Their motion is uniform for the same reason: "Circular motion always goes round evenly, for its cause is unfailing" (1976, p. 45), this cause being for Copernicus the natural tendency of spheres.[3]

To understand Copernicus's position we need to concentrate not on the circles generated by the spheres, or their mathematical properties, but on the nonmathematical concepts of motion and the perfection of the heavens. If we are to locate these concerns according to the dichotomy of mathematical astronomy versus cosmology, then these are clearly cosmological concerns. We are obliged to conclude that Copernicus's main announced criticism of Ptolemy, the inadmissibility of the equant, is a cosmological objection, and in this respect it is identical to the criticisms of the homocentric theorists. The equant is only a problem because planets have to be moved by uniformly rotating spheres. Spheres may rotate uniformly only about axes which are diameters, but the motion required by the equant could be created only by a sphere rotating uniformly about an axis that is not a diameter, a condition impossible to satisfy.

It is a matter of current controversy whether Copernicus believed in the reality of the celestial spheres familiar in the Ptolemaic tradition.[4] There is admittedly no unequivocal statement in *De Revolutionibus*, a point usually counted in favor of those who deny that Copernicus accepted spheres, although the passages I have already quoted (among others) are difficult to interpret in any other way. But the most telling point is that the equant problem is not intelligible except as a problem about real spheres. General historical evidence tends to the same conclusion. Whatever his personal views, Copernicus must have expected his audience to accept the reality of the spheres. Throughout his lifetime the only major astronomical traditions (the Ptolemaic tradition as developed by Puerbach and Regiomontanus, and the less developed alternative tradition of homocentrics) assumed the existence of celestial spheres.[5] Both the text and the context of Copernicus's work suggest that he accepted the reality of the celestial spheres, and this commitment suggests additional reasons for his reluctance to publish, in addition to those adduced by Goddu.

If Copernicus's system is interpreted by means of the physical construction familiar in the Ptolemaic tradition, a major problem appears – the spherical shells are no longer in contact.[6] There are huge gaps between them. There is an even larger gap between the spherical shell of Saturn and the fixed stars. And there is a discontinuity at the center of the system. In the Ptolemaic system as developed in the *Planetary Hypotheses*, or the *Theorica* tradition, or even in the rival homocentric models of Fracastoro and his contemporaries, the innermost sphere of the heavens is continuous with the uppermost sphere of the terrestrial elements. The terrestrial elements form a further series of nesting spheres which is continuous all the way to the center of the system. There is no empty space. Once the center is moved to the Sun,[7] the first of the spheres of the heavens, counting outward from the center, is the sphere of Mercury. The size of the Sun is known to be much smaller than Mercury's sphere. What fills the region between?

Copernicus's cosmological model has gaps. These difficulties are independent of the success of the model in calculating planetary positions, and to that extent are conceptual rather than empirical. They are no different in kind from those that motivated Copernicus to seek an alternative to Ptolemy. And in fact there are other similar difficulties.[8] For Copernicus, the natural motion of a sphere is to rotate, but the Moon is a sphere, and it does not rotate.[9] The fixed stars are also confined to a sphere, and that sphere does not rotate. The same problem arises for the Sun, although whether or not it rotates may be an open question. Adding all these potential difficulties to the problem of the gaps between the spheres provides considerable additional grounds for Copernicus's reluctance to publish.[10]

Copernicus's lack of a detailed (as we would say) physical model for heavenly motion makes his realism hard to appraise. Perhaps we need to see realism not as a global requirement which must be satisfied by any theory, but rather as a goal to be reached if a series of related theories succeeds. The hope is that more and more of these difficulties will be resolved by later work, as Galileo resolved the problem of brightness variation for Venus by demonstrating the phases.[11] Sometimes, of course, the resolution takes the theory in a new direction, as in the case of Kepler's solution to the problem of the gaps between the spheres. We might say that Kepler's work made a realist interpretation of Copernicus's theory untenable.

Contemporary philosophers and historians are too likely to treat science as an activity in which most of the time things go right, as the paradigm of rationality or an activity so successful that its success needs explaining. Much contemporary concern with realism in science is, I think, a consequence of this underlying conception, as is the persistent tendency to ignore or underemphasize the anomalies which are as much a part of the scientist's working environment as the positive evidence for the current theory. It would be as great a mistake to describe science, as a historical phenomenon, without anomalies, as it would be to describe monarchies, as an historical phenomenon, without succession problems, or democracy, as an institution, without faction fights. The assumption of success is fatal as an approach to understanding the history of science.

Duhem saw that the history of science is largely the history of error, of failed theories and abandoned positions. The history of science is like the history of life on earth – extinct theories outnumber the survivors. Duhem accommodated scientific failure by relegating realism to the realm of metaphysics. As Goddu shows, Duhem allowed realism only when science and metaphysics coincide at a theoretical endpoint. Interestingly this coincidence is to be recognized by 'bon sens' a nonlogical faculty of judgment. It is salutary to be reminded by Duhem that there are sources of knowledge that resist logical analysis. I would even locate some of them inside science itself (e.g., tacit knowledge). If we are unable to endorse Duhem's solution to the problem of scientific failure, the response need not be a global realism, but a historically relativized realism – the realism of medium term theoretical success, which may be withdrawn in the long term. But even in locating the problem, Duhem's history and philosophy of science is more sophisticated than much of what has passed for historical and philosophical analysis of science during the twentieth century.

NOTES

[1] He refers, for example, to Aristotle's threefold division of simple motions as if he expects his audience to already understand the doctrine (Copernicus 1976, p. 45).

[2] Although ancient Stoics took the contrary view, and there was new interest in their scientific ideas during the sixteenth century (Barker 1985; Barker and Goldstein 1984, 1988). The possibility that each sphere was endowed with an intelligence capable of self-movement was also debated in the Middle Ages (Weisheiphl 1985, chap. 7).

[3] For a very different reading of these passages, see Jardine (1982).

[4] Swerdlow (1973, 1976) had very much the best of an exchange with Rosen (e.g., 1975), in which he affirmed the reality of Copernicus' spheres against Rosen's denials. Other important points appear in Jardine (1982) and Westman (1980), the latter taking a middle position. I propose modifying Westman's position in the direction of Swerdlow.

[5] For Puerbach see Aiton (1987); on the homocentric theorists see Swerdlow (1972).

[6] For the original Ptolemaic 'nested sphere' model see Goldstein (1967). On later knowledge of the model see Van Helden (1985). On Copernicus's calculations of planetary distances compare Van Helden (1985) with Neugebauer (1968).
[7] Strictly speaking the center is moved to the *mean* Sun.
[8] Other difficulties are more plainly empirical. Copernicus's theory accounts for the observed pattern of variation in brightness for Mars, but also requires a similar variation in brightness for Venus – and this is not apparent to a naked eye observer. This problem was eliminated by Galileo's 'discovery' of the phases of Venus (Ariew 1987). Similarly, although Copernicus avoids the dramatic variation in the apparent size of the Moon predicted by Ptolemy's theory, his own theory employs not one but two epicyclic motions and will not keep the same face of the Moon always turned towards the earth.
[9] This seems to have been the majority position prior to Copernicus. A minority of cosmological commentators recognized that a rotation of the moon might compensate for the kind of epicyclic motion mentioned in the previous note (Gabbey (forthcoming); Grant 1987).
[10] Although these defects of the Copernican theory seem compelling to the modern reader, considerably more argument would be needed to establish that Copernicus himself would have seen either these difficulties or the empirical problems mentioned in note 8 as major defects. In particular, it would be important to consider whether Copernicus saw his astronomy as a system in the modern sense. For more on this problem see Barker and Goldstein (1988).
[11] See note 8 above.

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HOLISM A CENTURY AGO: THE ELABORATION OF DUHEM'S THESIS

ABSTRACT. Duhem first expounds the holistic thesis, according to which an experimental test always involves several hypotheses, in articles dating from the 1890s. Poincaré's analysis of a recent experiment in optics provides the incentive, but Duhem generalizes this analysis and develops a highly original methodological position. He is led to reject inductivism. I will endeavor to show the crucial role history of science comes to play in the development of Duhem's holism.

The claim that our knowledge confronts the tribunal of experience as a whole, which is known as the holistic thesis, has spurred much debate and, in consequence, has received a good deal of attention. Yet it is not at all obvious that the historical origin of this idea has been thoroughly studied. It is generally acknowledged that Pierre Duhem was the first to expound the thesis in 1906 in the first edition of *The Aim and Structure of Physical Theory*. But how he arrived at the idea and why he adopted it are questions which are most often neglected. Tracing the holistic thesis back within Duhem's work, one discovers that it originated in the 1890s, that is, almost a century ago. The initial context shows that the philosophical claim is intimately related to ongoing scientific discussions; it also reveals more clearly Duhem's motives. An analysis of the elaboration of holism and its impact on Duhem's thought yields some noteworthy results. First, a remarkable evolution is brought to light. Duhem came to reject inductivism; this shift occurs after he began to philosophize. Secondly, Duhemian holism does not reduce to a single narrowly interpreted thesis: generalizing the thesis, the French philosopher endeavors to formulate a holistic methodology. Thirdly, new significance is given to some inductivist remarks, which commentators have noted in *The Aim and Structure*: Duhem maintains several ideas developed within his earlier inductivist approach; in this respect, his philosophy is not free from inconsistencies. Finally, a clue is provided for Duhem's conversion to history of science, for the second half of the French physicist's career is devoted almost exclusively to history. External factors and even factors lying within

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the province of history alone do not explain why, from 1903 on, Duhem undertook to rewrite the history of science, especially the pre-Copernican period, an ambitious program, which culminated in his monumental *Système du Monde*.

1.

Some twelve years before synthesizing in *The Aim and Structure* his reflections on the methodology of physics, Duhem announced in 'Quelques réflexions au sujet de la physique expérimentale' the basic idea of holism: "an experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group" (Duhem 1894a, p. 187).[1] In 1894 this claim was truly novel. Not only was it absent in the earlier articles, but it even appears to conflict with the initial conception set forth there. In his first philosophical article, 'Quelques réflexions au sujet des théories physiques', Duhem recommended an inductive method for selecting hypotheses, and he did not point out the shortcomings of such a method (Duhem 1892, pp. 146f, for example). In support of this contention, let us simply note that in an autobiographical passage of 'Physics of a Believer' Duhem acknowedges such an evolution of thought, which implies the rejection of an earlier inductivism (Duhem 1905, pp. 275–78).

A contemporary experiment in physics provided the incentive for philosophical reflections: Otto Wiener's experiment on the direction of vibration of polarized light, whose results were published in 1890. This experiment created a stir at the time, not because it revealed a yet unknown property of light, but because it seemed to make it possible to decide between two competing theories. For a good number of years physicists had been hesitating between Fresnel's theory and F. E. Neumann's and MacCullagh's theory. The customary interpretation of these theories yielded two diametrically opposed predictions: if light, following the classical view, is taken to be a vibration in an ether medium, according to the first theory, the vibration is normal to the plane of polarization; according to the second theory, the vibration is parallel to the same plane. By verifying the first prediction, Wiener's experiment infirmed Neumann's theory and confirmed Fresnel's theory. Some scientists did not fail to take the experiment as an example of a crucial experiment. Thus, for example, Cornu in his appraisal of the experiment:

This beautiful experiment deserves to mark the beginning of a new era in the history of optics: *it decisively overthrows* theories which place the vibration in the plane polarization of light, like those of MacCullagh and Neumann; on the other hand, *it confirms in a spectacular manner* the ideas of Fresnel and his pupils . . . . This experiment reveals by a palpable fact the dynamic character of the vibration of light, which had begun to be considered, by some mathematicians, as an abstract conception, a symbolic entity indifferently reducible to many different kinematic equivalents. In the light of this experiment in which the experimentator directs as he wishes the mechanical action of light vibration like sound vibration, one can no longer assert that optical vibration is a mere geometrical abstraction. (Cornu 1891a, p. 187; italics mine)

An exceptional situation warrants a strong conclusion: because two competing theories which lead to contradictory predictions are involved, Wiener's experiment, indeed, enforces the truth, the reality, of the surviving theory. Cornu seizes the opportunity to attack abstract nonrealist conceptions, like those already presented by Poincaré and Duhem in several branches of physics. His interpretation of Wiener's experiment constitutes a challenge for such conceptions.

Duhem rebuts such an interpretation in 'Les théories de l'optique', citing the intricacy of experimenting: "What Mr. O. Wiener's experiment condemns is not the particular hypothesis that the vibration is parallel to the plane of polarization; what it condemns is the group of hypotheses which constitute MacCullagh's and Neumann's theory; his experiment teaches us to abandon some part of it, but it does not tell us what to change; we can for example give up placing the motion of the ether molecule in the plane of polarization of the ray; but we can also let the ether molecule vibrate in the plane of polarization as long as we change some other hypothesis of the theory, for example the hypothesis which explains the mechanical sense ascribed to light intensity" (Duhem 1894b, p.112). One need only adopt another interpretation of one of the fundamental concepts of optics in order to provide an entirely different situation. Nothing prevents such a move, as the concepts involved admit of several interpretations.

Here Duhem is following Poincaré, this being one instance of the latter's influence. The famous mathematician gave an account of Wiener's experiment in 1891 in front of the Paris Academy of Sciences; he held that this experiment in itself is not crucial (Poincaré 1891a).[2] Poincaré took up again this view the following year, in the second volume of his *Théorie mathématique de la lumière*, in a passage which Duhem did not fail to call attention to in his review of the book (Duhem 1893, p. 257). For both authors the philosophical question is in the foreground. Neither Poincaré nor Duhem are interested in rescuing Neumann's theory; in their scientific research, they both favor Fresnel's theory (Poincaré 1891a, p. 325; Duhem 1896).

Where does Duhem's originality lie? Should Poincaré be given the credit for formulating the holistic thesis? If Poincaré may have been the first to advance a critical interpretation of Wiener's experiment, from his analysis he never inferred a general conclusion concerning the nature of experimental testing. It is true that he writes in 1902: "In optics . . . Fresnel believed the vibration to be perpendicular to the plane of polarization; Neumann considered it to be parallel to this plane. An 'experimentum crucis' which would make it possible to decide between these two theories was sought for some time, but it was not possible to find one" (Poincaré 1968, p. 224; this remark appeared in his 1890, vol. 2, p. xiv). But this passage of *La science et l'hypothèse*, is very ambiguous; in fact, Poincaré is reproducing here a text published in 1890, most likely before he learned of Wiener's experiment. Why does Poincaré not recall here his interpretation of Wiener's experiment? He not only passes over this interpretation in silence, but he even continues to speak of decisive experiments as well as crucial experiments (for example, in Poincaré 1968, pp. 158, 165). Poincaré neglects Duhem's early formulation of holism, which had been noticed right away by Milhaud, another member of the loosely structured critique of science movement, which Le Roy characterizes as a "new positivism".

Unlike Poincaré, Duhem generalizes the critical interpretation of Wiener's experiment into a philosophical thesis: "What we have here is not a particularity of the experiment carried out by Mr. O. Wiener but a general characteristic of experimental method; it is never possible to subject an isolated hypothesis to the test of experiment, but only the group of hypotheses" (Duhem 1894, p. 112). Duhem perceives the importance of this result, and he pursues his analysis in his next article, 'Réflexions au sujet de la physique expérimentale', where he chooses a new example to illustrate his claim, Foucault's experiment. This experiment shows that light travels faster in air than in water, thereby infirming a prediction of the corpuscular theory of light, while confirming a prediction of the wave theory. Duhem demonstrates that this experiment in itself is not, any more than Wiener's, a crucial experiment, that is, an experiment that imposes decisively one theory. Now, this experiment was considered as a classic example of crucial experiment.[3] By giving another view of Foucault's experiment, Duhem challenges the methodology of crucial experiment, one of the dogmas of traditional philosophy of science. What was merely a critical interpretation of a recent experiment becomes a full-fledged thesis. This thesis implies an entirely new conception of experimental method.

2.

Retrospectively, 'Réflexions au sujet de la physique expérimentale' appears to be a complement to the first article, but this should not conceal the novelty of the text. To subject experimental method to an exacting analysis after Claude Bernard, whose aim was to introduce experimental reasoning in physiology, must have seemed like a superfluous endeavor. The opening remark of the article is not a rhetorical device: "What is an experiment in physics? This question will undoubtedly astonish more than one reader . . . ; is there any need to raise it, and is not the answer self-evident?"[4] It is not the importance of experimental method which Duhem questions, but the soundness of the classical conception. He is conscious of contradicting traditional methodology: "By declaring that the interpretation of facts by means of theories is an integral part of a physical experiment . . . , we will perhaps scandalize more than one mind concerned with scientific rigor; more than one will bring up against us the rules framed hundreds of times by philosophers and observers from Bacon to Claude Bernard."[5] Duhem explicitly challenges Bacon's idea of crucial experiment; he rejects Bernard's account when applied to a highly developed theoretical science like physics. In *The Aim and Structure* he will come to condemn the inductive or Newtonian method.

Up to this point Duhem has shown that, because multiple theoretical choices are involved, there are no experiments which are truly decisive in themselves. But so-called crucial experiments were considered exceptional. Duhem goes a step further and gives us a general analysis of physical experiments; he emphasizes here the importance of theoretical interpretation: "An experiment in physics is the precise observation of phenomena accompanied by an *interpretation* of these phenomena; this interpretation substitutes for the concrete data really gathered by observation abstract and symbolic representations which correspond to them by virtue of the theories admitted by the observer" (Duhem 1894a, p. 182 and again in his 1914, p. 221f; 1954, p. 147; Duhem's italics). Theoretical interpretation separates and distinguishes the practical fact, the brute evidence, and the theoretical fact, the evidence incorporated into the theory. One example Duhem chooses for illustrating these remarks is Regnault's series of experiments on the compressibility of gases, which had become a paragon of experimental research. This is an ordinary experiment in the sense that the experimental procedure is straightforward;[6] Regnault's results are not controversial, that is, within a certain degree of approximation and pending some minor corrections, they are definitive.[7] Let us take the simplest measurement involved, the volume occupied by the gas: "In a sighting device Regnault saw the image of a certain surface of mercury become level with a certain line; is that what he recorded in the report of his experiments? No, he recorded that the gas occupied a volume having such and such a value. . . ." The operation involves concepts of several different areas of physics, namely, general mechanics and celestial mechanics.[8] The volume occupied by a gas is not only an abstract idea but also a theoretical idea. An experiment always involves a theory as a whole and even brings in several different chapters of physics. Duhem's interpretation of experimental method is thus intimately connected with the holistic thesis.

It is not necessary to dwell on this point, which has received much attention. Let us simply register that Duhem's conception of experimental testing is acquired, in the main, as early as 1894. In fact, the article is almost identical with the text found in *The Aim and Structure*, chapters four, five, and six of part two. What is striking, however, is the omission in the article of the two paragraphs concerning Newtonian method. Let us follow up this clue. From his analysis of experiment Duhem draws some conclusions; for example, he rejects a particular method of construction or presentation of a theory, according to which "one would like the professor to arrange all the hypotheses of physics in a certain order, to take the first one, enounce it, expound its experimental verifications, and then when the latter has been recognized as sufficient, declare the hypothesis accepted; he would begin this operation again on the second hypothesis, on the third, and so on until all of physics was constituted . . . . This idea is a false idea" (Duhem 1894a, p. 196). Such a method clearly contradicts the holistic thesis: it is not possible to test an isolated hypothesis. *The Aim and Structure* takes up almost word for word this sentence, inserting a highly revealing clause: "One would like [the professor] to formulate the first hypothesis by *inductive generalization* of a purely experimental law" (Duhem 1914, p. 304; 1954, p. 200; italics mine). What is being referred to is the inductive method, which *The Aim and Structure* condemns unambiguously; "The teaching of physics by the *purely inductive method* such as Newton defined it is a chimera" (Duhem 1914, p. 309; 1954, p. 203; italics mine). What is true of teaching is all the more so of theory construction. Apparently, in 1894 Duhem did not attempt to deduce all the consequences of his analysis of experimental method, since the same analysis will lead him, twelve years later, to declare the inductive method impracticable. It is true that only two years before Duhem recommended this method against the excesses of mechanism.

Duhem's criticism of the inductive method raises a question: how to choose the principles on which to build a theory? In the first article this choice appeared in fact to be determined, guided, in some way, by the inductive method. By what to replace this method? The pedagogical hints given in 'Réflexions au sujet de la physique expérimentale' are obviously an insufficient answer. In *The Aim and Structure* the difficulty is no longer avoided; the author sends the reader on to the following chapter on the selection of hypotheses, where recourse is made to history of science. History of science thus appears in Duhem's methodological treatise; it has its place in the construction as well as in the teaching of physical theories: "The legitimate, sure, and fruitful method of preparing a student to receive a physical hypothesis is the historical method" (Duhem 1914, p. 408f; 1954, p. 268). In 1894 Duhem does not yet perceive the role that history of science should play; this is undoubtedly the deep reason why he hesitated to criticize explicitly the inductive method.

3.

How does history of science come to provide a solution? It is by no means accidental that after rejecting the inductivist schema of the transition from Kepler's laws to Newton's principle in part two chapter six of *The Aim and Structure*, Duhem gives a long account of the historical genesis of the principle in the next chapter. This account is clearly intended as an alternative to the inductivist reconstruction. The author places the principle of gravitation in the history of scientific thought. Duhem integrates some material from his erudite *Origines de la statique*, which he is working on simultaneously. Duhem no longer leaves out the Middle Ages; he has an inkling of his famous thesis: many major ideas of modern science have their origin in the thirteenth and fourteenth centuries. Already Duhem criticizes the classic idea of scientific revolution, a drastic and sudden change: "In the course of this long and laborious birth, we can follow the slow and gradual transformations through which the theoretical system evolved; but at no time can we see a sudden and arbitrary creation of new hypotheses" (Duhem 1914, p. 384; 1954, p. 252). The slowness, the gradualness of scientific evolution is a sign of its continuity. Duhem explicitly links continuism with his idea of natural classification: "By virtue of a *continuous tradition*, each theory passes on to the one that follows it a share of the *natural classification* it was able to construct" (Duhem 1914, p. 44; 1954, p. 33; italics mine). History of science then provides the missing link for Duhem's rejection of inductivist methodology.

Let us test this idea by attempting a final comparison between one of the early articles and *The Aim and Structure*. It is not a coincidence if the text in which the holistic thesis first occurs is Duhem's first article pertaining to history of science. 'Les théories de l'optique' offers a rapid overview of optical theories since the seventeenth century. During the second half of the seventeenth century, Huygens formulated a wave theory of light. This theory inspired by Cartesian physics, in turn, was rejected by Newton and his successors in favor of a corpuscular theory based on an attractionist model. Toward the middle of the nineteenth century, in the light of new discoveries, scientists took up again the wave hypothesis. This evolution is quite astonishing: the rehabilitation of a hypothesis which had been rejected and was believed to have been refuted.

Duhem uses the history of optics to illustrate a general thesis concerning the nature of physical theories. Theories are fragile and temporary constructions. Historical distance shows that many of the pretensions of traditional mechanism concerning the value of its hypotheses are unfounded. Duhem foretells the imminent decline of mechanism: "Mechanical hypotheses have disappeared, broken up by experimental contradictions or carried away by the torrent which has, for three centuries, turned over and over metaphysical systems" (Duhem 1894b, p. 124). These critical remarks serve to justify abandoning realistic and mechanistic conceptions. Physical theory is to be conceived as a convenient representation of laws and not an explanation of reality. History serves to justify this view.

Skepticism is not however the final lesson of Duhem's history. Behind the succession of theories and hypotheses, he perceives an evolution, a direction of history. Old theories disappear, while contributing to the evolution of science. First of all, the experimental laws which were discovered with the help of these theories remain. But if theories merely serve to suggest experimental laws, their usefulness would seem slight; one might even propose to do away with them altogether. Yet theories appear to play an essential role. This is how Duhem sees the legacy of hypotheses which have been discarded: "Huygens' hydrodynamic ideas are outmoded; but they have given to mathematical optics the notion of wave surface, the form of this surface for isotropic and uniaxial media. Newton's light particles have disappeared, with their access of easy reflection and easy transmission; but optics has continued to represent light phenomena by means of a magnitude which varies periodically in time and whose period, very short, characterizes color. Young assimilated the ether of the ray of light with a column of vibrating air; this assimilation is no longer accepted, but it led to ascribe a direction to the magnitude represented by light phenomena and to compose these magnitudes with one another like forces or speeds. Fresnel's ether and its motions seem about to disappear; but through them it has become known in optics that the magnitude representing light phenomena is governed by the same equations as the transversal motions of elastic solids" (Duhem 1894b, p. 125). In this series of examples some of the fundamental concepts of modern optics are to be found. The contribution of a theory is, in the final analysis, quite different from what its creator foresaw. The elements of a theory are altered and incorporated in a new context. The scientist is surprised by the strange origin, and the historian by the unexpected evolution. Even when they prove to be false, theories can add something to the language of science. This language is refined and enriched, progressively becoming a more and more efficient instrument.

In *The Aim and Structure* Duhem often calls on the history of optics in order to illustrate his argumentation; he draws heavily on 'Les théories de l'optique'. But the ideas of the article undergo an interesting modification. Duhem relates in the article Arago's experiment so as to explain the acceptance of Fresnel's theory. The same example in *The Aim and Structure* shows that theory can anticipate experiment and serves to introduce the idea of natural classification, which is absent in the article: "[Physical theory] assumes, while being completed, the characteristics of a *natural classification*. The groups it establishes permit hints as to the real affinities of things. This characteristic of *natural* *classification* is marked, above all, by the fruitfulness of the theory which anticipates experimental laws not yet observed, and promotes their discovery" (Duhem 1914, p. 40; 1954, p. 30; italics mine). Duhem thus expands his idea of a progressive development of the languages of science and expounds a continuist philosophy, which has both methodological and historical implications.

An analysis of 'Réflexions au sujet de la physique expérimentale' and 'Les théories de l'optique' shows that the first formulation of the holistic thesis is unaccompanied by an explicit criticism of the inductive method; it also indicates the bearing history of science has on the author's evolution of thought. What I suggest is that methodology and history of science are intimately connected in Duhem's philosophy and that this connection deserves careful attention. Going back a century, returning to the initial context, also helps us to understand better the meaning of holism. Holism is not an abstruse idea framed only by philosophers; it originates in a precise scientific setting and becomes a general philosophical thesis, in an attempt to break with traditional methodology. Holism thus lies at the origin of contemporary philosophy of science.

NOTES

[1] The same remark is taken up again in his (1914), p. 183, translated by P. Wiener as Duhem (1954), p. 278; I give throughout the pagination of the original with that of the translation. In places I modify the English translation.

[2] A. Cornu replied in his (1891b); Poincaré, nevertheless, maintained his position in his (1891b).

[3] As Duhem says, in his (1894a), "An experiment regarded as one of the most decisive ones in optics", p. 190. This remark is taken up in his (1914), p. 282; (1954), p. 186.

[4] Duhem (1894a), p. 179. The same remark occurs in his (1914), p. 218; (1954), p. 144. Duhem foresees the reader's astonishment again in another passage of his (1914), p. 231; (1954), p. 153.

[5] Duhem (1894a), p. 182. This passage does not occur in *The Aim and Structure*, but Bacon and Bernard are mentioned in Part II, chap. 6, section 1.

[6] Duhem (1894a): "What Regnault did is what every experimental physicist necessarily does", p. 182. "Let us take any experiment whatever for example, Regnault's experiment", p. 185. Only the first passage appears in his (1914), p. 221f; (1954), p. 147.

[7] Duhem (1894a): "In his experiment on the compressibility of gases Regnault let exist a cause of systematic error which he did not perceive and which has since been pointed out: he neglected the action of weight on the gas under pressure", p. 206. Also in his (1914), p. 239; (1954), p. 158.

[8] Duhem (1894a), p. 181. Also in his (1914), p. 220f.; (1954), p. 146. In *The Aim and*

Structure, Duhem adds hydrostatics and optics to the list, before concluding: "The knowledge of a good many chapters of physics necessarily precedes the formation of that abstract idea, the volume occupied by a certain gas."

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DUHEM AND THE ORIGINS OF STATICS: RAMIFICATIONS OF THE CRISIS OF 1903–04*

ABSTRACT. Much speculation on the sources of Duhem's historical interests fails to account for the major shifts in these interests: neither his belief in the continuous development of physics nor his Catholicism, when his Church was encouraging the study of generally Aristotelian scholastic thought, led to any interest in mediaeval science before 1904. Equally, his own claim that he was merely testing his views on the nature of physical theory is easily squared only with earlier work with no trace of mediaeval science. Behind this discontinuity lies a major crisis. Though not a positivist, Duhem had based all his work on assumptions acceptable to positivists. One of these, the sterility of the Middle Ages, was refuted by his chance discovery of evidence of genuine mediaeval science in the autumn of 1903, but that left the doctrine of scholastic sterility intact.

1. INTRODUCTION: INTERPRETING DUHEM AND THE 1913 ACADEMY DOCUMENT

Since it covers both the history of science and its philosophy, the work of Pierre Duhem provides a suitable test case on which to focus discussions of the mutual interaction of these two studies.[1] To what extent, we may ask, did Duhem's philosophical writing control his historical investigations? To what extent were the historical investigations a main source for his distinctive philosophical positions? Like Agassi (1963) and Lakatos (1971), Duhem fully expected that the mutual relationship would be close, but the outcome of his labours was not what he or anyone else would have expected. A study of that outcome can tell us a good deal about Duhem's other concerns.

In 1913 Duhem authorized one interpretation of his historical work, in his submission to the Académie des Sciences at the time of his election as a nonresident member.[2] His attempt to present his philosophical and historical studies as ancillary to his physics bears all the marks of the occasion for which it was drafted: it was as a physicist that he was up for election, and it was as a physicist he was going to present himself. Whatever other interests he may have had, they were not relevant on that occasion or, if they were relevant, it was for the light they shed on his career as a physicist.

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The picture he presented then lies behind one school of Duhem interpretation, of which Armand Lowinger with his Columbia thesis (1941) is perhaps the most prominent representative. Duhem the physicist had developed a scientific programme to reduce the whole of physics to the laws of heat via a general thermodynamics or Energetics; in defence of that programme he had developed a philosophy, a critical analysis of scientific knowledge, and set this out in a number of writings up to the *Théorie Physique*. But to the good scientist, theories, including theories of scientific knowledge, require testing, and for the facts to test this theory he turned to history. On this showing, the historical writings are apologetics for the Energetic method.

This interpretation of Duhem's philosophical and historical interests aligns him with the common practice of late positivism, and makes of him a practitioner of the rigidly internalist critical history for which perhaps Mach[3] is best known (and which some latter day positivists are still trying to keep alive in the face of the flood of externalist historiography), a practitioner of a type of 'history' in which the only factors allowed to appear are those which the logic of the philosopher-historian accepts as relevant. This account represents also a highly integrated rigid view of Duhem with no room for development, with all the parts interconnected, and no joins showing. Most obviously, it makes no allowance for Duhem's undoubted and never concealed religious and political concerns.

There is much to be said for this view of Duhem's philosophy and historical interests. Both his love of and dedication to theoretical physics, and his commitment to his Energetic programme are beyond doubt, and apologetics for his Energetic method seems a reasonable description for the brilliant classic short works he wrote before and up to about the turn of the century, works like *Les Théories Électriques de J. Clerk Maxwell* (1900–1901), *Le Mixte et la Combinaison Chimique* (1902) and the *Évolution de la Mécanique* (1903). Nonetheless, it is a nonsense: the detailed discussions in the *Systéme du Monde* (1913–59) of the philosophy and theology of Aquinas and of the condemnations of 1277 have no conceivable connection with Duhem's Energetic programme. The Academy document and the school of Duhem interpretation springing from it would have been a pretty fair description up to 1903. It was not in 1913.

At this point, readers may be tempted to appeal to Duhem's doctrine of continuity.[4] According to this much-discussed doctrine, good physics builds on what came before instead of seeking to destroy it, usually also claiming that the appearance of revolutions in the history of physics conceals an underlying continuity discoverable by closer analysis. It is commonly supposed that the idea of a seventeenth-century scientific revolution offended against this doctrine and so drove Duhem into seeking the mediaeval antecedents of modern science. Duhem's belief in continuity is beyond question, but as Malocchi has shown, it was common among late nineteenth century positivists, and it will not do the work required of it. If his continuism is an adequate explanation of Duhem's work on mediaeval science it is remarkable that it did not have that effect before 1904.

2. MEDIAEVAL SCIENCE AND THE 'NEO-SCHOLASTIC' ALTERNATIVE

There is another theory of the origin of Duhem's philosophical and historical interests which is even more implausible: that it was an outgrowth of late nineteenth century and early twentieth century neo-scholasticism.[5] This programme for reviving scholastic thought is particularly associated with Pope Leo XIII,[6] and as a Catholic Duhem must be supposed to have been aware of it. The ultimate source of this theory is a little unclear, though it seems to have been current in the Vienna Circle,[7] and it also got publicity from Antonio Favaro.[8] In epistemology it seems to rest on superficial resemblances between Duhem's doctrine of the autonomy of physics,[9] its independence of all metaphysical and extra-physical considerations, and scholastic theories of the classification of the sciences. In history of science its basis seems to be Duhem's interest in the Middle Ages, and his many publications in neo-scholastic journals such as the Brussels *Revue des Questions Scientifiques*,[10] and the Paris *Revue de Philosophie*.[11]

It depends on what counts as scholastic. If all that is meant is taking mediaeval thought seriously, Duhem is included; he is also included if it means no more than a genuine attempt on the part of Catholics to grapple with modern science, and he may have seen the *Revue des Questions Scientifiques* in that light.[12] In the eyes of the Catholic authorities, however, it meant very specifically the revival of the thought of Thomas Aquinas, and his supposed reconciliation of Christian thought with Aristotle.[13] It was a definite ecclesiastically sponsored movement with definite ideas, forwarded by particular people. Duhem could, the evidence is, tolerate some of these, like Jean Bulliot, the editor of the *Revue de Philosophie*, but many of them could rouse him to express his disgust in extremely violent terms.[14] They in their turn found him an embarrassment: the lengths Jacques Maritain went to refute him[15] is evidence of that, and equally symptomatic is the way Étienne Gilson avoids citing the passages from Duhem's *Systéme du Monde*[16] this Thomist must have found particularly uncongenial.

But the most obvious difficulty facing any kind of 'neo-scholastic' theory of the origin of Duhem's interests in the history of mediaeval science in particular is the discontinuity, discussed above, in the career of the continuist philosopher of physics. Duhem's mediaeval interests came so very late in his life: all the works on which Duhem's reputation as a historian of mediaeval science rests are later than 1904.[17] The 'neo-scholastic' theory of Duhem's philosophical and historical interests could have had some plausibility in 1913, but it has none for the Duhem of 1903, whose works showed no interest in or knowledge of mediaeval science. In the first part of his career Duhem seems to have been totally immune to all ecclesiastical pressures in favour of mediaeval thought, while in the second part, his researches were increasingly controlled by concerns outside physics. A serious account of the various interests of the historical Duhem has to take account of this discontinuity, and pay attention to just what was happening at the watershed of 1903–1904 and in the years that followed.

3. THE DISCOVERY OF 1903–1904

In 1974, I began examining systematically those of Duhem's works deriving from that crucial period. I found what had not to my knowledge been documented before: Duhem's discovery of mediaeval science took him completely by surprise in the winter of 1903–1904 while he was working on *Les Origines de la Statique*. In a paper published by *Annals of Science*,[18] I documented the surprise and sought to interpret the evidence: I saw the discovery of mediaeval science as provoking a crisis of a far-reaching kind, some of whose ramifications I attempted to indicate. It is now clear to me that I did not see anything like the full picture, but, nevertheless, a very brief summary is in place here. In what follows I do not speak of a crisis in the Kuhnian[19] sense of an interregnum between paradigms, when nobody can understand each other, but in the Biblical sense of a judgement, a time when one's acts and one's doings are in question, under judgement, and it is unclear what will stand or fall.

In the autumn of 1903 Duhem had just completed his *Évolution de la Mécanique*, of which a principal theme had been the dynamics of Lagrange, a theory of moving bodies and of the forces that moved them built on a principle (virtual velocities) having to do with bodies, not in motion, but at rest. This principle of the theory of forces at rest, or statics, had a long history, and Duhem now set out to write a work on the origins of that theory, under the motto title *Les Sources des Théories Physiques*. Like much else that he wrote, he published this work in serial parts in a journal, in this case the quarterly *Revue des Questions Scientifiques*. The first instalment of four chapters, appeared in the October 1903 issue, and in the ordinary course of events, the next would have appeared in the January 1904 issue. It did not: when it did appear in April it was flagrantly inconsistent with the first. The first instalment, unusual though it was for the emphasis it placed on Leonardo's ideas and their influence, otherwise broke little new ground, and in particular flatly announced that the commentaries of the scholastics on Aristotle's Μηχανικὰ προβλήματα (actually there were no such commentaries: the work was unknown in the Middle Ages[20]) had added nothing to our knowledge. The second, however, started with the admission that Duhem had so far given only a crude sketch of the early development of the subject, and now needed to gather up the Alexandrian sources of mediaeval statics. He was also now laying heavy emphasis on the previously unmentioned thirteenth century figure of Jordanus de Nemore.

What brought Duhem to this *volte face* is even now not clear. He seems, in late October or early November 1903, to have become bothered by the inadequacies of his sources and asked Paul Tannery for help in locating a certain Euclid text. Tannery's postcard reply of December 1903,[21] volunteers the following additional information:

> There is also, under the name of Euclid, another piece on statics, *Euclidis de Ponderibus*, sometimes confused with the work *Jordanus Nemorarii de Ponderibus* (see Rose's *Anecdota Graeco-Latina* II, 291). I have seen this treatise in manuscript, but don't really know it – it seems of Arabic origin. Woepcke (*Journal Asiatique* XVIII, 1351, p. 217) has given the translation of a Euclid piece 'on the balance' connected with it. Maurice Gallien, retired artillery commander (Toulouse, 5 rue Traversière) was going to make a special study of these writings, so that I have not concerned myself with them, but I have not heard from him since 1900.

That postcard seems to have set him off: at any rate, in 1905, in his preface to the book form of his work (otherwise a verbatim reprint of the articles, including the inconsistencies), he told the world that the only thing for it was to get hold of and analyse everything relating to mediaeval statics in the Bibiliothèque Nationale and the Bibilothèque Mazarine. Duhem's response, it might be felt, was perhaps a little extreme: did he really need to go that far to fill in the gaps in his sources? For a critical historian, Duhem had always been unusually careful with his sources, making sure he got his facts right before his critical analysis got going, but this is going beyond even ordinary care. It is hard to resist the conclusion that for Duhem important issues were at stake.

4. THE CONSEQUENCES OF THE DISCOVERY

My 1976 paper attempted to say what these issues were. I suggested that in Duhem we had to do with a physicist, with the prejudices physicists imbibed in their training, and among these prejudices, I suggested, was the doctrine, in all his sources from Lagrange[22] to Dühring[23] and Mach, that the mediaeval period was scientifically sterile. In Jordanus, however, he encountered evidence, which his training as a physicist compelled him to recognize as evidence, of worthwhile mediaeval science. I also suggested that the refutation of the doctrine of mediaeval sterility had knock-on effects in other areas of Duhem's thinking: it called in question his historiographical assumptions and the methodological principles that guided these assumptions, particularly the autonomy of physics. As he had explained in the *Théorie Physique*,[24] the kind of history of interest to the physicist, was one bare of all personal details, containing only the occasions in which an important new fact was discovered, a principle clarified or false idea refuted, and in this he was advocating a 'critical' approach to the history of physics rather like the 'rational reconstruction' method advocated by Imre Lakatos.

Duhem was, however, conscious of the limitations of this 'historical' method. In 1903, in an extended essay review of the French translation of Mach's *Mechanik*,[25] he discussed Mach's exclusion of nonscientific ideas from his story, and remarked that this was acceptable for recent physics, since it was now generally agreed that physics was and should be separate from religion and metaphysics. This general agreement, however, did not obtain in earlier periods, and the historian who wanted to bring the past to life had to take account of such ideas. Bringing the past to life again was what Duhem was certainly not doing in 1903. In the following year, however, he was doing it, and because he was doing it, he could no longer assume the autonomy doctrine in his analysis. But equally, he had nothing to replace it with, and the lack of such a principle shows in the chaotic nature of much of his work over the next decade. With *To Save the Phenomena* in 1908,[26] however, he had the outline of a replacement, in which the *problem* of autonomy replaced autonomy: the standing of physics and its degree of independence from other disciplines was now the central issue.

Thus far this is the account of my 1976 paper. With qualifications, it still seems generally correct, though something would need to be added about the positivistic atmosphere in which the physicist had been trained, as has been so admirably done by Roberto Maiocchi, but a great deal more needs to be said about the crisis of those years.

5. WIDER RAMIFICATIONS OF A CRISIS

What I did not see in 1976 was that bringing into question the historiographical applications of Duhem's methodology had much wider effects on the whole orientation of a man in an extremely sensitive political and religious position. I did not then see these wider effects, nor see it for what it was, a crisis imperilling his whole life strategy, I did not see that beyond the time invested in a particular approach to the history of science, founded on a particular set of historiographical assumptions linked to a particular methodology, lay a particular way of coping with the political and religious conflicts all around him. Duhem had developed an integrated strategy for coping with these conflicts, and this strategy was now at risk. I have no time or space here for a detailed account of the pressures Duhem as a Catholic scientist in the Third Republic was exposed to, and in any case the job has been very well done by Harry Paul,[27] but some understanding of these is essential for understanding the religious motifs in his writing, and for making any kind of sense of his later output, and I give a brief summary here.

The France Duhem grew up in and in which he made his career was deeply unstable, its instability marked by a succession of crises that split the country from top to bottom.[28] The most famous of these was the Dreyfus affair of the closing years of the nineteenth century and early years of the twentieth century. This concerned the Jewish army colonel, Albert Dreyfus, who was imprisoned in 1894 on Devil's Island after being falsely found guilty of espionage, and the long political campaign for his rehabilitation that finally succeeded only in 1906. The Third Republic was in practice the constitution imposed by one party on another, imposed by people who were atheists on Catholics who hankered after a restored Bourbon monarchy, imposed by people whose ideological inspiration was positivist[29] on Catholics who had every reason to resist that ideology, imposed by people who appealed to the authority of science as the counterweight and answer to the authority of religion. The educational system was the key to the strategy against the religious enemy: a schoolmaster in every parish was to undermine the authority of the priest. It was an all-embracing system, from the elementary school to the university, unless you opted for mostly underfunded Catholic schools and universities of low prestige. Duhem made his career as a Catholic in the state system, and was not alone in this even if he was perhaps the most visible, and the obvious question is what strategy he adopted to ensure his survival and protect his integrity. That strategy will throw light on the rationale behind the nonsense of the 1913 Academy document discussed in my opening paragraphs.

In a like political situation the founders of the Royal Society of London responded to it by forbidding all discussion of politics and religion at its meetings. The strategy is common in conflict-ridden societies, and was available to Duhem; if, for example, as a Catholic (and therefore drawn to the side of the reactionary opponents of the campaign for Dreyfus's release from the Devil's Island) he wanted to stay on good terms with his friend and colleague, the left-leaning mathematician Jacques Hadamard[30] who just happened to be the brother-in-law of Albert Dreyfus and organiser of the campaign for his release. The correspondence with Hadamard continues right through the Dreyfus affair, and after Duhem's death Hadamard contributed to the memorial volume on his work [31] and spoke at the 1936 memorial meeting in Paris organized by Aldo Mieli.[32] He also referred to him in friendly terms in his *Psychology of Invention* (1945) many years afterwards. It is no surprise that letters from Hadamard at the time of the affair are on purely technical matters (the stability of the solar system[33]) and do not refer to politics. Duhem's strategy is the obvious one: make your stand on your scientific excellence, and let your work speak for itself: never ever appeal to your faith or politics in your scientific work. The French Catholic who made his career as a scientist within the Third Republic at no point appealed to his religion: his work, acceptable to the positivists on their own terms, was to be its own argument. The thing was to seek truth and broadcast what he found to the winds,[34] avoiding unnecessary religious or political polemic.

Duhem was thus involved in a strategy of defeating the enemy from within using the assumptions and methods of the positivists, excelling at the science and using only the methods of the science that was their paradigm of knowledge and rationality. Only by thus presupposing physics as a neutral ground for debate, valid in its own terms, could Duhem attempt to expose the limitations of claims to authority based on that science. Hence the rigorous analysis of the bases of physical theory, underpinning a particularly abstract approach to the problems of theoretical physics, in which all that counted were just those things the positivists said counted: agreement with observation and experiment. Hence also the brilliant historical sketches of the 1890s and early 1900s in which alternative less abstract approaches to physics and physical chemistry based, for example, on atomic hypotheses were shown as leading into contradictions and dead ends while the abstract approaches Duhem favoured triumphantly overcame these obstacles. Hence finally the brilliant attempt to show that within its own terms an account of physical theory relying on nothing but observation and experiment was incomplete, unable to explain its own goals and procedures.[35]

Duhem's strategy had only partial success. Close colleagues like Hadamard often came to make allowances for his combative disposition and respect his integrity, but those who controlled his career and wrote reports on him saw him as something of a Trojan horse who had to be neutralized: they held back his professorial grading as long as they could and made sure that he never got the Paris job that was the normal expectation of an academic of his standing. As Harry Paul has shown, to them he was a dangerous enemy to be dealt with at arm's length. That may be no surprise, but it is more surprising that Duhem's strategy was hardly more acceptable to his fellow Catholics.

6. NEO-SCHOLASTICISM AGAIN

Duhem's neutralizing strategy can be contrasted with the neo-scholastic alternative. This offered an alternative philosophy, based on the work of St. Thomas Aquinas, and an alternative rationality in which the authority of science was to provide an independent rationale for the authority claims of the Catholic hierarchy, its right to command obedience both in the religious sphere and the political. Thomist scholasticism offered a philosophical environment that facilitated the elaboration of a natural theology offering proofs of God's existence to underpin the teaching of the Christian revelation and assist the articulation of that revelation. At the same time it facilitated the erection of a system of natural law underpinning Catholic moral theology, giving it a solidity that it might have lacked if it had to rely on the teachings of the New Testament and its tradition. Such claims to authority were very much at issue when Duhem was writing, as the Catholic community descended worldwide into its so-called modernist crisis,[36] of which France was the principal focus. The principal document of that crisis, the encyclical *Pascendi Dominici Gregis* of 1907,[37] specifically pointed out the dangers 'modernism' posed to natural theology, and went on to identify a distrust of scholasticism as a principal symptom of the 'heresy' so named. Through the 1890s Duhem did not object to the idea that a rationale for the method of physics might be found in a metaphysics of generally scholastic type, but he did not encourage it, and in the meantime ensured, to neo-scholastic protests,[38] that any such metaphysics would have no power whatever to dictate the contents of his autonomous physics.

Duhem did not, moreover, do anything to encourage any suggestion that there was a mediaeval science, scholastic in form, worthy of attention in his own time. Such suggestions could have been very welcome to the ecclesiastical authorities: a constant theme of Catholic apologetics has been to show how the Church supported education and learning in the Middle Ages, and to show the beneficence of the church's control in matters intellectual. Moreover, a plausible scholastic science would have made easier the job of the natural theologian wanting to update the proofs of God's existence. But a science under scholastic auspices was not a likely prospect for Duhem's apologetic purposes: what he needed was one under nonscholastic auspices. There should be no difficulty in seeing why: it was and is a commonplace that the seventeenth century scientific revolution was at heart the replacement of scholastic sterilities by experimentally controlled mathematical precision,[39] and that received wisdom meant that the Church that was supposed to have encouraged these sterilities could make no convincing claim to the attention of scientific intellectuals. There was a high price to pay for taking pride in just that feature of mediaeval thought universally agreed to have been overcome by the rise of modern science. Duhem's strategy had no room for mediaeval science, or at least for what was then known of it, and little room for a synthetic programme of absorbing science into metaphysical framework of scholastic inspiration.

But that strategy had serious consequences: Duhem was now in effect sitting on the fence in the midst of his Church's battle for survival, refusing it the weapons it needed to win the war. His difficulties were now more severe than they had been in the early 1890s. Then, when his early near-Machian assistance to the memory account of physical theory[40] had come under scholastic attack, he had responded with a theory of natural classification, according to which the classifications imposed on the world by his purely mathematical abstract systems approached the truth asymptotically. He was even prepared to hazard the guess that reality might be truly describable by a metaphysics of scholastic type. The concession cost him little, for he had never doubted that there was a real world out there, and he had not conceded metaphysics any right to dictate the contents of his physics. He seems to have thought that such partial concessions to scholasticism, welcomed as they were by Belgian friends associated with the *Revue des Questions Scientifiques*, would be enough to fulfill his obligations as a loyal Catholic, but the events of the following decade proved otherwise.

After the death in 1916 of his student friend, the historian of philosophy Victor Delbos, Duhem, who had himself only a couple of months left to live, remarked to Blondel[41] that he had sometimes heard him accused, particularly by priests, of not being open and forceful enough in his Catholicism: to be insufficiently strident was to leave oneself open to suspicions of treason. Astonishing as it may now seem, Duhem was exposed to just this suspicion, that in his philosophy he had compromised with the enemy. In this he was in a like position to those other Catholics, increasingly accused by their opponents of modernism, who sought intellectual and political dialogue with non-Catholics. Throughout the 1890s and into the beginning of the following decade, Duhem had avoided aligning himself either with such Catholics or against them. He was now to be forced to take sides. In 1905 his student friend

Maurice Blondel invited him to collaborate with the *Annales de Philosophie Chrétienne*,[42] which he had just acquired on the death of its previous owner and editor Charles Denis. Duhem hesitated: he was not prepared to give up his previous collaborations and rather wished that Catholics would unite against the common enemy instead of shooting at each other. He did, however, agree to collaborate with it and it carried both his 'Physique de Croyant' (October–November 1905) and *To Save the Phenomena*. But in due course it also carried Maurice Blondel's long attack (1909–10) on the reactionary politics of Action Française[43] and those Catholics, mainly neo-scholastic, who hoped for an alliance with this atheistic positivist-led monarchist movement. When, as a result of that campaign, it was denounced to Rome and condemned in 1913,[44] Duhem's sympathies were only too clear. He had made his choice.

7. CONCLUSION

The reason for Duhem's mediaeval interests and the discontinuity in his career should now be clear. In October 1903 he stumbled on what he did not know he was looking for: a nonscholastic mediaeval science. It seems that, much as he distrusted many of the scholastics of his acquaintance, he had assumed that the story they told of the Middle Ages was factually correct: the 'science' of the period was indeed scholastic and (therefore) sterile. Loyal to his Church as Duhem wished to be, his loyalty did not go so far as to proclaim the fertility of that sterile nonsense. But now, it seems, impressions had been mistaken and Duhem went in search of more of the newly discovered gold. He discovered that the scholastics could not be relied on to give an account of mediaeval thought, of its science, of its philosophy, or even of its theology. He discovered that when first stated the doctrines now being made normative for Catholics had actually been condemned by the ecclesiastical authorities of their day in the name of Christian orthodoxy, in his view rightly so. He discovered that the famed synthesis of Christianity and Aristotle was no synthesis at all, merely incoherent, for Aristotle's philosophy, like much else from the ancient world, was irrevocably vitiated by the pagan principles on which it was built, and so he was able to satisfy himself that the system of thought needing to be discarded if modern science was to make its appearance was equally inimical to Christian orthodoxy.

Duhem pursued these discoveries with all the persistence and energy of a detective on the trail of a fraud, for fraudulent was certainly how he regarded it, and the massive results of his industry were the *Études sur Léonard de Vinci*, *To Save the Phenomena*, and the monumental incomplete *Système du Monde*. Without the crisis of 1903–1904 nobody would now be remembering Duhem the historian of science, and without it his work would lack its special flavour and interest. The ramifications of this major upset need to be borne in mind by readers of any of his writings of these years. Duhem took a while to establish the new directions in which his thought would go, but while the argument of this paper has concentrated on his historical work, it is a safe bet that no area of his work was left untouched.

NOTES

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[1] Useful general accounts of Duhem are D. M. Miller's (1971) DSB article, and S. L. Jaki's (1984) biography.

[2] See P. Duhem, 'Notice' (1971), a translation of the relevant part of which was circulated with the preparatory papers for this conference.

[3] See Mach, *Geschichte und Wurzel* and *Mechanik*.

[4] Heavily emphasized by Agassi in his *Historiography*. Maiocchi, *Chimica e Filosofia*, gives a very full treatment of this aspect of Duhem's work and its antecedents among his late nineteenth century predecessors.

[5] The only full presentation of this common interpretation I know of is that of Paul, *Contingency* (1979), chapter 5, pp. 136–78. For a detailed discussion of it with full references see my essay review 'Darwin and Duhem' (1982).

[6] See his encyclical *Aeterni Patris*. There are full discussions of the movement in McCool, *Catholic Theology* (1972), and Thibault, *Savoir et Pouvoir* (1972). The relevant articles in the *New Catholic Encyclopedia* ('scholasticism', 'thomism', 'sciences, classification of') give something of its intellectual commitments.

[7] See Philipp Frank's reiterated but unargued claims in his 'Introduction, Historical Background' (pp. 1–51) to his *Modern Science and its Philosophy* (1949).

[8] See his 'Galileo...' (1921).

[9] See *Théorie*, part I, chapter I.

[10] On this journal see Nye, 'Moral Freedom' (1976).

[11] On this journal and Duhem's role in its foundation see Paul, *Contingency*, pp. 177–8, and Hélène Pierre-Duhem, *Savant*, p. 105.

[12] See his letter of 3 July 1897 to Paul Tannery.

[13] Dom David Knowles, *Evolution of Mediaeval Thought*, offers one version of this theme, and Gilson, *Christian Philosophy*, a more sophisticated version.
[14] Solicited by Maurice Blondel to collaborate with the *Annales de Philosophie Chrétienne* of which Charles Denis was then the editor, he replied (12 January 1896) thus (translation mine):

> The Abbé Denis may be a decent man, but as for the Société St. Thomas d'Aquin, whose organ his journal is, it too may well contain some decent people, but it also contains some beings puffed up with vanity – Count Domet de Vorges for example – as well as some dirty poisonous beasts, like the individual who hides in the *Annales de Philosophie Chrétienne* as well as in *Le Monde*, behind the name 'Congressist' – I have no desire to mix my prose with people of that ilk, who think they are authorized to tell lies because they wear a cassock.
>
> Moreover I will confess to you that such people have put me off the Catholic world – not Catholics, which isn't the same thing – more than I can say. The Lille Catholic University had already given me the measure of the degree of sincerity in that world when the Brussels congress completed my education: scribes and Pharisees, hypocrites!

The congress referred to was the third in a series of international meetings of Catholic Academics. The congress report (vol. 1, 1895) prints the text of Duhem's interventions and the Thomist A. Gardeil (1894) gives a highly suggestive account of events.
[15] See his *Distinguer pour Unir* (1932, pp. 84–90, 123–5, 385).
[16] His *Christian Philosophy* (1955) cites the chapter of volume v of the *Système* dealing with Albertus Magnus but not the equally extensive adjacent chapters on Thomas Aquinas and Siger of Brabant.
[17] The *Origines* (1905–6), the *Études* (1906, 1909, 1913), and *Système* (1913–59).
[18] My 'Genesis' (1976), which analyses the chronology of this episode in detail.
[19] According to P. L. Rose, and S. Drake (1971). I owe this reference and much else to the late Dr. Charles Schmitt, then the member of the editorial board of Annals of Science responsible for handling my paper.
[20] See his well-known *Structure* and *Essential Tension*.
[21] So dated by the editors from the postmark.
[22] See Lagrange, *Mechanique Analitique* (*sic*) (1788, 1811–15, 1853–55).
[23] *Kritische Geschichte* . . . (1873, 1877). Dühring actually states explicitly that the mediaeval period was a historical desert.
[24] *Théorie*, Part II, Chapter VI, Section VI.
[25] This twenty-page essay review (1903) of Mach's *Mécanique* (1904) has all the marks of an inside job done on advance proof copy. The translation was only published the following year, and Duhem got the title wrong, reading *Étude* for *Exposé*. See also Hentschel (1988).
[26] This is a natural interpretation of Duhem's preface (pp. 1–2).
[27] See particularly his 'Crucifix and Crucible' (1969), and 'Quest of Kerygma' (1969).
[28] Bury's *France 1814–1940* (1949–1976) gives a useful general history of the period, while Adrien Dansette's *Boulangisme* (1938) gives a fascinating account of one of the crises.
[29] On nineteenth century positivism see Gouhler, *Jeunesse d'Auguste Comte* (1933–41); Hayek, *Counter-Revolution of Science* (1952); and Simon, *European Positivism* (1963).

[30] The *Encyclopedia Judaica* has a useful account of him.
[31] 'L'Oeuvre de Duhem dans son aspect mathématique' (1917).
[32] Hadamard (1937).
[33] *Théorie*, part II, chapter III, sections III and IV, citing an article by Hadamard, are largely the fruit of this correspondence.
[34] With Catholic critics in mind, the letter to Blondel cited above continues:

> My firm intention is never to get mixed up with these people: seek the truth, and when I think I've found a particle of it, throw the news of it to the four winds and let the crows caw.

Cf. this response from Lucien Laberthonnière (9 January 1909):

> The stuff of yours that I've read and the conversations I've had with you have considerably clarified, by confirming it, the idea I had of the intellectual apostolate. I am very grateful. Yes you are right: what matters and counts is to work away believing in the value of the truth

I haven't been able to see the other side of this correspondence.
[35] See *Théorie*, part I, chapter IV, section X and elsewhere.
[36] The essential secondary source on modernism is Émile Poulat's *Histoire Dogme et Critique* (1962, 1979).
[37] On the analysis of this encyclical (1907) and its origins see Vidler, *Variety* (1970), and Daly, *Transcendence and Immanence* (1980).
[38] See e.g., Vicaire, 'Valeur Objective' (1893); Paul, *Contingency*, presents a mass of evidence of neo-scholastic criticism of Duhem.
[39] See e.g., Burtt, *Metaphysical Foundations* (1924, 1932); Butterfield, *Origins of Modern Science* (1949, 1957, 1962); Hall, *Scientific Revolution* (1954, 1962); Koyré, *Études Galiléennes* (1966); Westfall, *Construction of Modern Science* (1971, 1977).
[40] See his 'Réflexions' (1892).
[41] Letter of 8 August 1916.
[42] Blondel's letter seems to have been lost, but Duhem's reply is dated 25 July 1905. Laberthonnière's response to Duhem's offer of the article that became 'Physique de Croyant' is dated 16 August 1905. Both the published Blondel-Wehrlé (1957) and Blondel-Valensin (1969) correspondences contain material relating to this journal implying that Blondel was at least financing or had actually bought it.
[43] The standard work is Eugen Weber's classic study (1962), but see also Paul, *Second Raillement* (1967).
[44] *Index* (1948). Laberthonnière was forbidden to publish anything at all or say anything about this further ban. There are full letters from Duhem to Blondel dated 27 June and 20 July, and from Blondel dated 16 July 1913. I thank Donald G. Miller for copies of letters from Duhem when on loan to him, and G. Mosseray for copies of letters from Blondel. I hope to make a fuller study of this important correspondence in the future, and I thank Duncan McGibbon for discussing its significance with me. Extracts from Duhem's second letter are published in L. Lecanuet (1930), pp. 478–9, and the Blondel-Wehrlé correspondence.

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DUHEM IN DIFFERENT CONTEXTS: COMMENTS ON BRENNER AND MARTIN

ABSTRACT. These comments consist of reflections on the papers Anastasios Brenner and R. N. D. Martin presented at the Conference on Pierre Duhem: Historian and Philosopher of Science. I argue they present nicely complementary accounts of Duhem's turn to history of science: Brenner emphasizes reasons internal to Duhem's philosophical concern with scientific methodology while Martin highlights reasons derived from the broader context of Duhem's engagement with religious controversies of his culture. I go on to suggest that seeing Duhem in this broader perspective can help us cope with the conflicts between science and religion in our own culture.

At first glance the papers by Anastasios Brenner and R. N. D. Martin seem quite diverse in their preoccupations, but a bit of reflection on them reveals a common theme.[1] Both papers raise questions about the role of history of science in Duhem's thought. What larger purpose, if any, did he mean to use the history of science to serve? And, in particular, why was he especially interested in late medieval science? As I shall argue, the two papers return answers to such questions that are in some ways nicely complementary. Moreover, it seems to me that this complementarity is a consequence of the fact that Brenner and Martin situate Duhem's thought in different intellectual contexts. This leads me to wonder about how to specify a context for the study of Duhem that will enable us to learn as much as we can from the legacy of his thought. Anglophone philosophers have, for the most part, focused on the context of Duhem's concerns with science and philosophical reflection on its methodology. But, as I shall suggest, there may be much of philosophical value to be learned from locating Duhem in a broader context that includes his religious concerns and the theological problems of his culture and ours.

Brenner's paper concentrates its attention on the context of Duhem's developing philosophy of science and reveals reasons for Duhem's historical turn internal to his methodological thinking. As Brenner's narrative presents the development of Duhem's holism, it begins in an analysis of Wiener's experiment on the direction of vibration of polarized light and is later generalized into a philosophical account of experimental method. Duhem's general analysis of experiment is meant to

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support holism by showing that, in order to bring experience and scientific theory into contact with one another, observed fact must be transformed into theoretically interpreted fact and that whole theories are presupposed in making such transformations. Consequently, theoretical principles cannot be established seriatim by induction. And this, as Brenner emphasizes, leaves Duhem facing a problem about how to choose among theoretical principles.

Readers of *The Aim and Structure of Physical Theory* will recall that Duhem invokes good sense to solve this problem. We test whole groups of theoretical principles experimentally. When such a group fails to square with experimental results, logic alone does not dictate which of its members are to be rejected. But at this point good sense may intervene to yield a choice. Duhem himself describes good sense rather dramatically in terms of Pascalian reasons of the heart. It might also be characterized less grandly, following Polanyi, in terms of tacit craft-knowledge of communities of scientific practice or, following MacIntyre, in terms of the local rationality of historical traditions of inquiry. The point is that good sense embodies a kind of informal rationality and so its choices are generally rational. If the good sense of scientists can itself be shaped by knowledge of the history of science, there will be a role for history of science to play in the constitution of scientific rationality.

I think it quite plausible to suppose that Duhem envisaged a role of this sort for history of science. After all, he writes history of science with the didactic aim of supporting his instrumentalist philosophy. If history could be made to teach scientists that their enterprise does best when it concentrates on saving the appearances, then it would make good sense for scientists to rally in support of the research program in energetics Duhem himself favors, even if logic alone does not dictate that they should do so. As Brenner points out, the main lesson a history of scientific theories written from a Duhemian point of view teaches is that experimental laws accumulate, the language of science grows ever richer, and science progressively becomes a more and more efficient predictive instrument.

Duhemian history of science is, of course, partisan history. Scientific realists can and do write history of science in support of their philosophical predilections. This is a game both sides in the controversy between instrumentalism and realism can play, and neither side has yet emerged from play a clear winner. Some historical episodes seem to be grist for the instrumentalist mill; others appear to be powder for the realist cannon. So I think it would be wise to remain skeptical about whether the entire history of science or a consensus of its practitioners can be made to speak unambiguously in favor of one side or the other in the great philosophical debate between instrumentalism and realism.

Near the end of his paper Brenner draws attention to the somewhat surprising fact that Duhem did not ultimately rest content with mere instrumentalism. In *The Aim and Structure of Physical Theory*, he claims that the growing predictive success of science serves to indicate that science is making progress toward a natural classification, a classification system that, so to speak, carves nature at the joints. Though my own opinion is that this claim has a status in Duhem's thought closer to a pious hope than to a demonstrated conclusion, I think it is a bit puzzling that Duhem considered it important to advance such a claim. After all, a thoroughgoing instrumentalist does not have to take a stand on the vexed question of whether science is approaching or will converge on a natural classification. Why is Duhem unwilling to engage in skeptical suspense of judgment on this issue? Why, in other words, does Duhem think that successfully saving the appearances is not, in the last analysis, success enough for science? Some interesting but rather speculative answers to such questions emerge from considerations emphasized in Martin's paper.

Martin's account of Duhem's engagement with medieval science invokes a broad intellectual context that includes Duhem's religious concerns and those of his cultural environment. He situates Duhem in the thought-world of the Dreyfus affair in politics, the modernist controversy in theology, and the revival of Thomistic scholasticism in French Catholic philosophical circles. As he sees it, Duhem was not moved to study medieval science by disinterested historical curiosity; Duhem's agenda went beyond a simple desire to test his instrumentalism against another part of the historical record. Nor is his interest in medieval science to be explained by attributing to Duhem as a Catholic intellectual a desire to support the neo-scholastic movement. Prominent in that movement was the ambition to constrain science by insisting that it be grounded in a Thomistic philosophy of nature, and Duhem had ample reasons, both personal and professional, to resist this attempt to subordinate science to philosophy by mounting a defense of the autonomy of science. Since he had an interest in maintaining that science fares badly if it is subordinated to philosophy or theology, it would have suited Duhem quite well to think that the Middle Ages were, scientifically speaking, dark ages.

According to Martin's narrative, Duhem discovered almost by accident that there had been good science done in the Middle Ages, and this discovery precipitated an intellectual crisis. The pressing question was whether the historical record could be used to show that science had done well enough, or even especially well, when subordinated to scholastic theology or philosophy. If it could, the historical case for the claim that science must be autonomous if it is to make progress would be undermined. So Duhem's project in the history of medieval science was, Martin argues, to show that good medieval science was not in fact grounded in or based on Aristotelian or Thomistic theology or philosophy. And there is indeed evidence that such a concern shapes the way Duhem thinks about medieval science. He assigns a pivotal and liberating role in his own narration to the condemnation of various Aristotelian theses by Etienne Tempier in 1277, and he makes much of the connection between philosophical nominalism and scientific progress in the fourteenth century.

No doubt it is in principle possible for a thoroughgoing instrumentalist to mount a philosophically interesting defense of the autonomy of science. One way to proceed is to demarcate the spheres of authority of science and religion by means of an appeal to some version of the Kantian distinction between phenomenal and noumenal realms. Science concedes to religion exclusive cognitive access to the things in themselves of the noumenal realm; in return, religion yields to science exclusive cognitive access to the appearances of the phenomenal realm. The writ of science runs no farther than saving the appearances, but within the phenomenal realm science is the supreme cognitive authority. If this division of labor could be enforced, the protracted warfare between science and religion could be brought to an end.

The trouble with such a defense of the autonomy of science is that, in practice, some of the partisans of religion will not accept peace on these terms. They are not willing to settle for coexistence with science but instead demand that it submit to religious control. Neo-scholastic thinkers of Duhem's day often insisted that it is Thomistic philosophy of nature and not science that tells the real truth about the observable natural order. It is easy to see how such a view can lead to a devaluation of or even contempt for science, and such attitudes have not been unknown in Catholic intellectual circles. These days we hear ominous talk of Islamic science coming from the Islamic fundamentalists of Iran. Closer to home, there is so-called 'creation science', allegedly based on biblical revelation, and many Christian fundamentalists claim that it rather than evolutionary biology is on the right track in accounting for life on earth. If a strong defense of the autonomy of science is to be mounted against such practical threats as those I have mentioned, it seems to me of strategic importance to insist that science in its own right can, at the very least, aspire to become an independent source of cognitive access to the final truth about the natural order. I think it makes sense to suppose that the whiff of realism in Duhem's talk about approach to a natural classification is part and parcel of such a strong defense. Even if there is a religious way of knowing that has exclusive access to truth about supernatural things, Duhem is in a position to maintain that it does not have exclusive access to the truth about nature and so cannot on that account legitimately claim to control or preempt scientific inquiry.

But if it is granted that autonomous scientific inquiry is an independent source of cognitive access to the natural order, there is no guarantee of perpetual peace between science and religion. Even if it does not have exclusive cognitive access to the natural order, traditional Christian theology has been committed to claims that have implications for our understanding of nature. The Augustinian account of Adam's fall, if taken literally in broad outline, makes some sort of historical claim about a catastrophe in the remote human past. The Catholic doctrine of Transubstantiation is framed in terms of concepts drawn from an Aristotelian metaphysics of substance and accident. The most distinctive of Christian doctrines, the Incarnation, places constraints on the ways in which traditional Christians can consistently formulate accounts of human nature. Though it may be hoped that in the long run science and Christian theology will independently converge on a unified account of nature, in the short run conflict between the best science at a given time and the best theology of that time cannot be ruled out in advance. If a realistically interpreted science were to undermine the historical claim, the metaphysical framework or the theories of human nature alluded to above, then traditional Christians would come under increasing pressure to choose between backing off to mere instrumentalism in philosophy of science and making deep and perhaps unwelcome revisions in theology. If the retreat to mere instrumentalism is precluded for the sake of maintaining a strong defense of the autonomy of science, a delicate balancing act may be required in order to negotiate successful resolutions to episodes of conflict between science and theology. In such negotiations, it cannot be assumed *a priori* that science has to be the fixed point, for scientific conceptual schemes and ontologies have changed in the past and are likely to do so in the future. But neither can it be assumed *a priori* that theology has to be the fixed point, since ecclesiastical doctrine has developed over the centuries and will doubtless continue to do so. And, of course, the whole enterprise of conflict resolution is only made more complicated by the fact that Christianity is not the only religion whose theology has implications for our understanding of nature and so can claim to be a source of cognitive access to the natural order.

So when we locate Duhem in the larger context of the theological controversies of his culture, some problems emerge that are not merely of antiquarian interest to those of us who take the cognitive claims of both science and religion seriously. One moral I would draw from considering Duhem in this context is that a strong defense of the autonomy of science may carry with it an unpredictable theological price. But I do not think this exhausts what Duhem has to teach us about how to cope with the conflicts between science and religion that persist in our own culture, and so it seems to me there is much to be learned from studies of Duhem which, like Martin's, situate his thought in its religious context.

NOTE

[1] These comments are based on the versions of their papers that Brenner and Martin presented at the Conference on Pierre Duhem: Historian and Philosopher of Science and do not take into account any subsequent revisions they may have made in those papers.

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EINSTEIN AND DUHEM*

ABSTRACT. Pierre Duhem's often unrecognized influence on twentieth-century philosophy of science is illustrated by an analysis of his significant if also largely unrecognized influence on Albert Einstein. Einstein's first acquaintance with Duhem's *La Théorie physique, son objet et sa structure* around 1909 is strongly suggested by his close personal and professional relationship with Duhem's German translator, Friedrich Adler. The central role of a Duhemian holistic, underdeterminationist variety of conventionalism in Einstein's thought is examined at length, with special emphasis on Einstein's deployment of Duhemian arguments in his debates with neo-Kantian interpreters of relativity and in his critique of the empiricist doctrines of theory testing advanced by Schlick, Reichenbach, and Carnap. Most striking is Einstein's 1949 criticism of the verificationist conception of meaning from a holistic point of view, anticipating by two years the rather similar, but more famous criticism advanced independently by Quine in 'Two Dogmas of Empiricism'.

The typical story of the influence of Pierre Duhem's philosophy of science outside of French philosophical circles begins with Otto Neurath and ends with Willard V. O. Quine. There is more than a little truth to this story. Duhem's influence on Neurath was significant, direct, and generously acknowledged.[1] His influence on Quine was equally significant, though indirect (Neurath was the principal intermediary), with Quine himself having been unaware of the parallel between his and Duhem's views until it was pointed out to him by others.[2] And it is primarily through Quine's writings that Duhem's ideas have retained what currency they have in contemporary debates in the philosophy of science.

But the story is far from complete, and it leaves one with the clear impression that Duhem's holistic variety of conventionalism has been far less influential than the views of other thinkers like Mach, Poincaré, Russell, Wittgenstein, Schlick, and Carnap. I think that this impression is misleading. I think that we have, for years, and for a variety of reasons, underestimated Duhem's influence on twentieth-century philosophy of science.[3] And I will defend this thesis by exhibiting what I regard as the pronounced 'Duhemian' features of Albert Einstein's mature philosophy of science. I do not claim that Einstein's philosophy of science is through and through Duhemian; no such simple description can adequately characterize the views of a thinker like Einstein, who

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aimed, deliberately, not to be a systematic philosopher and who took the philosophical turn only when driven that way by problems arising in his scientific work. Instead, I want to argue (1) that recognition of the 'Duhemian' features is essential for understanding Einstein's philosophy of science, and (2) that the central place of the 'Duhemian' elements in Einstein's thinking is evidence of the profound and pervasive, if not always well-recognized influence of Duhem's ideas within twentieth-century philosophy of science.

1. EINSTEIN'S FIRST ACQUAINTANCE WITH DUHEM'S *LA THÉORIE PHYSIQUE*

Let us begin by exploring the context of Einstein's first acquaintance with Duhem's major work in the philosophy of science, *La Théorie physique, son objet et sa structure* (1906), which appeared in a German edition in 1908 under the title *Ziel und Struktur der physikalischen Theorien*. It was translated by Friedrich Adler, an ardent follower of Ernst Mach, who himself contributed a sympathetic foreword. In at least two respects, the involvement of Adler and Mach is significant for understanding Einstein's acquaintance with the text.

From the point of view of intellectual history, the involvement of Adler and Mach tells us something about the contemporary reception of Duhem's views at least among German-speaking philosophers of science, namely, that many thinkers saw no serious conflict between the views of Mach and Duhem, that both were seen as part of a larger anti-metaphysical movement in the philosophy of science emphasizing the economical and descriptive side of scientific theorizing. Mach's own characterization of Duhem's main thesis is instructive:

> The author shows how physical theory gradually transforms itself from a presumptive explanation on the basis of a vulgar or more or less scientific metaphysics into a system resting on a few principles, a system of mathematical propositions that economically describe and classify our experiences. In this process the explanatory picture changes many times, until finally it falls away entirely, while the descriptive part passes over into the new, more complete theory almost unchanged . . . Duhem regards the model, like the picture, as a parasitic growth. (Mach 1908, pp. iii–iv)

And Adler's characterization echoes Mach's: "The elimination of all metaphysics constitutes the fundamental tendency of the work, and the principle of the economy of thought, which Mach first formulated, is consistently maintained" (Adler 1908b, p. vi). There were, of course, some issues about which Mach and Duhem disagreed, as Adler dutifully and carefully indicated. Duhem, for example, did not consider any foundation for scientific concept formation like Mach's "elements of sensation" (Adler 1908, p. vi); such crude 'atomistic' reductionism, vesting not only each admissible proposition but each admissible concept with its own, individual empirical content, would be incompatible with Duhem's holism. Nor did Duhem require, as Mach would, that 'hypotheses' have an empirical basis and serve merely as tools for organizing experience (Adler 1908, pp. vi–vii); Duhem rejected such constraints also on the basis of his holistic conception of theories, arguing that entire theories must have empirical content, but that individual hypotheses could not (Duhem 1906, pp. 215–16).[4] With hindsight, *we* might regard these as serious and fundamental disagreements, as they indeed turned out to be, but the point is that *Mach* and contemporary Machians like Adler did not; they minimized the differences and stressed the broad areas of agreement.

Further evidence of Duhem's friendly reception by Mach and his followers is easily assembled. Thus, for example, the just-published *La Théorie physique* receives high praise from Mach in the foreword to the second edition (1906) of *Erkenntnis und Irrtum*, Mach's last systematic work on epistemology and the philosophy of science,[5] and it is cited numerous times in the annotations. Especially noteworthy are Mach's seemingly approving mentions of the characteristically 'Duhemian' theses of the theory-ladenness of observation,[6] and the holistic character of all hypothesis testing.[7] There is also Mach's gracious acknowledgment of Duhem's role in "the epistemological discussions" and of Duhem's "valuable" critical remarks in the foreword to the seventh edition of his *Mechanik* (1912, p. ix).[8] And there is, finally, the fact that another one of Duhem's major works, his *L'Évolution de la mécanique* (1903a), was translated in 1912 by Philipp Frank, also an enthusiastic follower of Mach's.[9] As Frank explained some years later, he and many of his contemporaries regarded the views of Duhem and Mach as being quite compatible, both examples of what the philosopher Abel Rey dubbed the "new positivism".[10] Like Adler and Mach himself, Frank stressed the common anti-metaphysical tendency of Mach's and Duhem's theories of scientific method, but he also emphasized theoretical holism as one of the principal features of Duhem's position of interest to him and his contemporaries (Frank 1949, pp. 25–28).[11]

The sympathetic reception by Mach and his followers may not reflect a wholly accurate reading of Duhem. The one thesis that has proven to be Duhem's major contribution to discussions of scientific method – his theoretical holism – is simply incompatible with the reductionistic and atomistic empiricism in Mach's epistemology. But that does not change the fact crucial for the story I want to tell here, which is that the Machians' public sympathy for Duhem would have prepared the ground for a similarly sympathetic reading by other thinkers, like Einstein, already favorably inclined toward Mach.[12]

Perhaps even more important for my story, however, is specifically the involvement of Friedrich Adler as the translator of *La Théorie physique*. It is important because of the personal relationship between Adler and Einstein.

Einstein and Adler were both students of physics in Zurich around the turn of the century, Einstein at the ETH, Adler at the University of Zurich.[13] Both completed dissertations under Alfred Kleiner at the University of Zurich,[14] and, finally, both were candidates for the Extraordinary Professorship in physics at the University of Zurich to which Einstein was appointed in 1909.[15] They had been acquainted with one another from their student days, Einstein apparently having first sought out Adler upon hearing that he was working on a dissertation on specific heats. They reportedly met often to discuss questions of physics, and, together with their future wives, who were also physics students, they audited lectures at the ETH.[16]

When Einstein returned to Zurich in the fall of 1909 to assume his new position at the university, he and his wife happened to rent an apartment immediately upstairs from the Adlers at Moussonstraße 12. The old relationship between the families, both now with children, was quickly resumed. But more importantly, the renewed personal relationship led to a renewal of the intellectual relationship between Einstein and Adler. In order to escape the noise of the children, they would retire to the attic, where they could work and carry on their discussions in relative quiet.[17] Adler described their relationship in a letter to his parents of 28 October 1909:

We stand on very good terms with Einstein, who lives above us, and indeed as it happens, among all of the academics, we are on the most intimate terms precisely with him. They have a bohemian household similar to ours, one boy of Assinka's age, who is very often at our place . . . The more I speak with Einstein – and that happens fairly often – the more I see that my favorable opinion of him was justified. Among contemporary physicists he is not only one of the clearest, but also one of the most independent minds, and we are of one mind about questions whose place is generally not understood by the majority of other physicists. (Ardelt 1984, p. 166)

The Adlers and Einsteins lived as neighbors until the spring of 1911.[18]

Einstein and Adler shared more than an address and a profession. They shared also the interest in the philosophy of science in general, and in Mach's work in particular, that was typical of many students in Zurich at that time, Zurich being as it were the Swiss second home of positivism, owing to the fact of Richard Avenarius's having taught at the University of Zurich from 1877 to his death in 1896. Einstein was introduced to Mach's writings by his friend Michele Besso during their student days at the ETH and quickly developed an appreciation for Mach's style of conceptual criticism.[19] Adler also first read Mach during his student days (Ardelt 1984, p. 98). Having started out as a follower of Friedrich Engels's brand of materialism, he was slow to warm to the anti-metaphysical Mach, but by 1905, when he published a paper criticizing the epistemological assumptions underlying Ostwald's energeticist program from the point of view of Mach and Avenarius (Adler 1905), he had been won over to the cause (Ardelt 1984, pp. 98–99, 125–37).[20] Adler and Mach began corresponding in 1903 (see Blackmore and Hentschel 1985, pp. 30–31), meeting personally for the first time in May 1905 (Ardelt 1984, p. 136), after which Adler went on to become a frequent public spokesman for Mach.[21]

So by the time when Einstein became the fellow bohemian Adler's upstairs neighbor and companion in attic conversation in the fall of 1909, Adler was a close colleague and prominent supporter of Mach's. It was that very autumn when Adler wrote his spirited defense of Mach against Planck's widely-discussed criticisms.[22] Just a few months earlier, in July or August, Einstein's own correspondence with Mach had begun, apparently with the help of Adler.[23]

And it had been just one year since the publication of Adler's translation of Duhem's *La Théorie physique* (1908).[24] Under these circumstances, it seems to me highly likely that Einstein's first acquaintance with Duhem's work can be dated to no later than the fall of 1909. There is no documentation of his having read *La Théorie physique* at this time, but given the nature of his relationship with Adler, given their mutual interests in Mach and the philosophy of science, and given Adler's role in translating *La Théorie physique*, it is hard to imagine that Einstein would not at least have learned about the work through discussion with Adler. And my guess is that he probably also then read it for the first time. But whether he read *La Théorie physique* at this time or merely discussed it with Adler, his acquaintance with Duhem would have been conditioned by the context, a context in which the community of interest between Mach and Duhem as proponents of the 'new positivism' would have been featured, a context that would have predisposed Einstein to sympathy with Duhem.

When exactly Einstein did read *La Théorie physique* for the first time is not clear. That he did read it and had a favorable opinion of it is evident, however, from the one and only apparent reference to the book that I have found in his writings and correspondence. It is in a letter of September 1918 from Einstein to the Bonn mathematician Eduard Study. Einstein had written to Study on 17 September (EA 22–301) praising Study's book, *Die realistische Weltansicht und die Lehre vom Raume* (1914), but suggesting that he did not agree with all of Study's views. In his reply of 23 September (EA 22–304), Study asked Einstein to elaborate on his criticism, and Einstein answered on 25 September (EA 22–307) with a three-page letter setting out his reaction in detail.

The main thrust of Einstein's criticism concerns Study's defense of scientific realism, the principal aim of the book. Einstein says that the proposition "The physical world is real" appears to him "meaningless" (*sinnlos*), as if one were to say "The physical world is cock-a-doodle-doo" (*Die Körperwelt ist kikeriki*); he adds that to him the "real" is "an in itself empty, meaningless category". And he concludes, "I concede that the natural sciences concern the 'real', but I am still not a 'realist'".

But then, as if to balance his criticism of the realist, Einstein offers this criticism of the positivist:

> The positivist or pragmatist is strong as long as he battles against the opinion that there [are] concepts that are anchored in the "A priori." When, in his enthusiasm, [he] forgets that all knowledge consists [in] concepts and judgments, then that is a weakness that lies not in the nature of things but in his personal disposition /just as with the senseless battle against hypotheses, cf. the clear book by Duhem/. In any case, the railing against atoms rests upon this weakness. Oh, how hard things are for man in this world; the path to originality leads through unreason (in the sciences), through ugliness (in the arts) – at least the path that many find passable. (EA 22–307)

That the interlineated remark about Duhem is joined to a criticism of the positivist attack on hypotheses makes it clear that the book intended is *La Théorie physique*, and that Einstein had in mind specifically sections 8 and 9 of chapter 6, along with the whole of chapter 7.[25] This is where Duhem defends the role of hypotheses against overly restrictive empiricist demands that every admissible scientific proposition possess its own empirical content, arguing, again, that while whole theories must have empirical content, the individual hypotheses constituting those theories cannot.

Given Einstein's relationship with Adler, and my conjecture that he first learned about Duhem through Adler, it is striking that he emphasizes here as a principal virtue of *La Théorie physique* vis à vis the positivism of Mach one of the two differences between Mach and Duhem that Adler had pointed to in his preface to the translation of *La Théorie physique*. But whereas Adler downplayed the significance of such differences between Mach and Duhem, Einstein stresses them. As we shall see later, it was precisely this thesis about the lack of empirical content of individual hypotheses that proved to be Duhem's main legacy to Einstein and that constituted the crucial difference between Einstein's empiricism and that of the Vienna and Berlin empiricists.

2. UNDERDETERMINATION, HOLISM, AND CONVENTIONALISM IN EINSTEIN'S PHILOSOPHY OF SCIENCE

Conventionalism first emerges as an explicit theme in Einstein's thinking in the mid- to late 1910s, partly in response to certain conceptual problems encountered in the development of general relativity, and under the significant influence of Moritz Schlick, himself then still the realist and conventionalist of his pre-Vienna days.[26] Einstein and Schlick most often brought out their conventionalist arguments in reply to attempts by various neo-Kantians to defend Kant against general relativity's threat to the claimed *a priori* character of Euclidean geometry.[27]

In a series of essays and reviews during the early 1920s,[28] Einstein and Schlick agreed with the neo-Kantians that empirical evidence underdetermines theory choice, especially the choice of deep theoretical principles like the axioms of geometry; but whereas the neo-Kantians exploited the fact of underdetermination to insulate cherished principles from empirical refutation, and insisted that our choice among the alternative theories equally compatible with experience is determined by *a priori* considerations, Einstein and Schlick argued that no principle is immune to rejection or revision in the light of experience, and insisted that the choice among alternative theories is a matter of convention, guided at most by considerations of simplicity. Indeed, this is one of the roots of Einstein's frequent talk of theories being "free creations of the human intellect" (for example, Einstein 1921, p. 5).

The kind of conventionalism that Schlick and Einstein deployed in response to the neo-Kantians owed at least as much to Poincaré (1902, 1905) as to Duhem. But the distinctively Duhemian themes of holism and underdetermination often came to the fore, especially in Einstein's writings of the period.

Consider, first, the theme of underdetermination. In a remarkable letter to Schlick of 21 May 1917, Einstein wrote:

> If two different peoples pursue physics independently of one another, they will create systems that certainly agree as regards the impressions ('elements' in Mach's sense). The mental constructions that the two devise for connecting these 'elements' can be vastly different. And the two constructions need not agree as regards the 'events'; for these surely belong to the conceptual constructions. (EA 21–618)

(By "events" Einstein means the points of the space-time manifold constituting a theory's fundamental ontology.) And in his address on the occasion of Planck's sixtieth birthday (26 April 1918), Einstein wrote:

> The supreme task of the physicist is . . . the search for those most general, elementary laws from which the world picture is to be obtained through pure deduction. No logical path leads to these elementary laws; it is instead just the intuition that rests on an empathic understanding of experience. In this state of methodological uncertainty one can think that arbitrarily many, in themselves equally justified systems of theoretical principles were possible; and this opinion is, *in principle*, certainly correct. But the development of physics has shown that of all the conceivable theoretical constructions a single one has, at any given time, proved itself unconditionally superior to all the others. No one who has really gone deeply into the subject will deny that, in practice, the world of perceptions determines the theoretical system unambiguously, even though no logical path leads from the perceptions to the basic principles of the theory. (Einstein 1918, p. 31)

This passage is especially interesting for the way it contrasts the logical fact of underdetermination with the practical fact of unambiguous determination, exactly the same ironic contrast having been stressed by Duhem.[29]

Perhaps the most definitive statement of the underdeterminationist thesis is found in a little-known newspaper article of Einstein's, entitled 'Induktion und Deduktion in der Physik', that appeared on Christmas day 1919 in the *Berliner Tageblatt.* Einstein wrote:

A theory can thus be recognized as erroneous if there is a logical error in its deductions, or as incorrect if a fact is not in agreement with its consequences. But the *truth* of a theory can never be proven. For one never knows that even in the future no experience will be encountered that contradicts its consequences; and still other systems of thought are always conceivable that are capable of joining together the same given facts. If two theories are available, both of which are compatible with the given factual material, then there is no other criterion for preferring the one or the other than the intuitive view of the researcher. Thus we may understand how sharp-witted researchers, who have command of theories and facts, can still be passionate supporters of contradictory theories. (Einstein 1919, p. 1)

Notice here Einstein's careful distinction between normal Humean inductive uncertainty – the standing possibility that new facts will arise that are incompatible with accepted theories – and Duhemian underdetermination – the necessary existence of alternative theories equally capable of explaining the same facts.

Holism is implicitly assumed at every mention of underdetermination – there are empirically equivalent alternative theories precisely because it is theories as a whole, not individual hypotheses, that stand the test of experience. But explicitly holistic arguments are not common in Einstein's writings from the 1910s and 1920s. The only example I have found is in one of Einstein's reviews of a neo-Kantian work on relativity, Alfred Elsbach's *Kant und Einstein* (Elsbach 1924), where, after asserting that relativity theory is incompatible with the Kantian doctrine of the *a priori*, Einstein wrote:

This does not, at first, preclude one's holding at least to the Kantian *problematic*, as, e.g., Cassirer has done. I am even of the opinion that this standpoint can be rigorously refuted by no development of natural science. For one will always be able to say that critical philosophers have until now erred in the establishment of the a priori elements, and one will always be able to establish a system of a priori elements that does not contradict a given physical system. Let me briefly indicate why I do not find this standpoint natural. A physical theory consists of the parts (elements) A, B, C, D, that together constitute a logical whole which correctly connects the pertinent experiments (sense experiences). Then it tends to be the case that the aggregate of fewer than all four elements, e.g., A, B, D, *without* C, no longer says anything about these experiences, and just as well A, B, C without D. One is then free to regard the aggregate of three of these elements, e.g., A, B, C as a priori, and only D as empirically conditioned. But what remains unsatisfactory in this is always the *arbitrariness in the choice* of those elements that one designates as a priori, entirely apart from the fact that the theory could one day be replaced by another that replaces certain of these elements (or all four) by others. (Einstein 1924b, pp. 1688–89)

This passage deserves careful attention. For one thing, it anticipates the still more sophisticated holistic arguments we will find Einstein advancing in the late 1940s in opposition to the empiricist theories of meaning. More immediately, however, it points up the reasons for the parting of the ways that was shortly to occur between Einstein and Schlick in their respective understandings of the role of conventions in science.

By 1925, when the second edition of the *Allgemeine Erkenntnislehre* appeared, Schlick had adopted the more refined interpretation of the conventionalist thesis we now associate with the members of the Vienna Circle and its allies like Hans Reichenbach. More clearly than in the first edition of the *Erkenntnislehre*, Schlick now insisted on a fundamental distinction between two types of propositions constituting a theory: analytic *coordinating definitions* and synthetic *empirical propositions*. He argued that only the former are conventional and that once they are fixed by convention the truth or falsity of the individual empirical propositions is unambiguously determined by experience – quite the contrary of Duhem's position (Schlick 1925, pp. 89–101).[30] Schlick and the other defenders of this position, like Reichenbach,[31] seem to have been driven to it by the logic of their argument with the neo-Kantians. As Einstein noted in his review of Elsbach, merely asserting that a theory as a whole possesses empirical import is logically not sufficient to force the hand of the neo-Kantian, who can always then protect cherished, allegedly *a priori* principles from empirical refutation by electing to abandon other elements of the theory when confronted with empirical evidence incompatible with the theory's predictions. Presumably not satisfied with Einstein's subtle criticism that the choice of which propositions to protect is entirely arbitrary, Schlick and Reichenbach seem to have wanted a more decisive reply to the neo-Kantian, one that would logically imply the empirical corrigibility of each individual synthetic proposition the neo-Kantian might want to defend, such as Euclid's fifth postulate. Hence the distinction between coordinating definitions and empirical propositions, and the claim that only the definitions are conventional.

The Schlick–Reichenbach conception of conventionalism stands or falls with the analytic-synthetic distinction, which provides the only basis for distinguishing between coordinating definitions and empirical propositions. Consistent Duhemians do not endorse the analytic-synthetic distinction, but that was not how Einstein criticized Schlick and Reichenbach. Instead, he used the same argument he used against the neo-Kantians, namely, that such distinctions among the propositions constituting a theory – whether between *a priori* and *a posteriori* propositions or between coordinating definitions and empirical hypotheses – is arbitrary, and hence, presumably, of no fundamental epistemological significance. One finds this argument in Einstein's classic 1936 essay, 'Physik und Realität', where Einstein ever so gently qualifies what might appear to be an endorsement of the Schlick–Reichenbach position:

> We shall call 'primary concepts' such concepts as are directly and intuitively connected with typical complexes of sense experiences. All other concepts are – from the physical point of view – meaningful only insofar as they are brought into connection with the 'primary concepts' through statements. These statements are partly definitions of the concepts (and of the statements logically derivable from them) and partly statements that are not derivable from the definitions, and that express at least indirect relations between the 'primary concepts' and thereby between sense experiences. Statements of the latter kind are 'statements about reality' or 'laws of nature', i.e., statements that have to prove themselves on the sense experiences that are comprehended in the primary concepts. *Which of the statements are to be regarded as definitions and which as laws of nature depends largely upon the chosen representation*; *in general it is only necessary to carry through such a distinction when one wants to investigate to what extent the whole conceptual system under consideration really possesses content from a physical standpoint.* (Einstein 1936, p. 316; emphasis mine)

See how the holistic viewpoint is insinuated at the end: We need only make a distinction between definitions and empirical propositions when we desire to determine the empirical content of the "whole conceptual system", and even then, where we draw the line "depends largely upon the chosen representation".

3. EINSTEIN'S 'DUHEMIAN' CRITIQUE OF EMPIRICIST CONCEPTIONS OF MEANING AND THEORY TESTING

Over the years, Einstein grew ever more impatient with the failure of Schlick, Reichenbach, Carnap, and their allies to understand his reservations about their view of the structure of theories and the relation between theory and evidence. His summary opinion was stated clearly, and with acid sarcasm, in a letter to Paul Schilpp of 19 May 1953, declining Schilpp's invitation to contribute a paper to the Carnap volume of the Library of Living Philosophers:

It is a good idea to devote a volume of your collection to Carnap's life's work. But I cannot comply with your request. That is to say, I have come to terms with this slippery material from time to time only when my own problems made it urgently necessary. But even then I have studied only a little literature, so that I cannot do justice to the swarm of incessantly twittering positivistic little birds.... Between you and me, I think that the old positivistic horse, which originally appeared so fresh and frisky, has become a pitiful skeleton following the refinements that it has perforce gone through, and that it has dedicated itself to a rather arid hair-splitting. In its youthful days it nourished itself on the weaknesses of its opponents. Now it has grown respectable and is in the difficult position of having to prolong its existence under its own power, poor thing. (EA 42–534)

Some of the reasons for Einstein's growing disenchantment with positivism emerge in his reply to Reichenbach's contribution to the Library of Living Philosophers volume on Einstein himself. And it is clear from this exchange that the problem concerned precisely the failure of Reichenbach and his colleagues to appreciate the implications of Duhemian holism.

Reichenbach had defended a view of the empirical character of geometry not unlike that which Einstein himself had defended years earlier in his influential *Geometrie und Erfahrung* (Einstein 1921),[32] with the exception that Reichenbach invoked explicitly the distinction between coordinating definitions and empirical hypotheses, interpreting Einstein's identification of the geometer's 'rigid body' with the physicist's 'practically rigid rod' as an instance of a coordinating definition (the definition of 'congruence'):

The choice of a geometry is arbitrary only so long as no definition of congruence is specified. Once this definition is set up, it becomes an empirical question *which* geometry holds for physical space.... The conventionalist overlooks the fact that only the incomplete statement of a geometry, in which a reference to the definition of congruence is omitted, is arbitrary. (Reichenbach 1949, p. 297)

As one might expect, Einstein did not agree. But instead of just saying so, he couched his criticism in the amusing form of an imaginary dialogue between 'Reichenbach' and 'Poincaré'.

A crucial step in the dialogue has 'Reichenbach' grudgingly agreeing with 'Poincaré' that, since there are no perfectly rigid bodies in nature, and since we must therefore employ our physics to correct for deformations resulting from things like changing temperature, we really wind up testing the whole body of theory consisting of geometry plus physics, and not just geometry alone. At this point, Einstein has an 'anonymous nonpositivist' takeover for Poincaré, out of respect, he says, "for Poincaré's superiority as thinker and author" (Einstein may also have realized that the view attributed to Poincaré was more Duhem's than Poincaré's). The nonpositivist observes that, in agreeing that geometry and physics are tested together, Reichenbach has contravened one of his own fundamental positivist principles – the equation of meaning with verifiability:

Non-Positivist: If, under the stated circumstances, you hold distance to be a legitimate concept, how then is it with your basic principle (meaning = verifiability)? Must you not come to the point where you deny the meaning of geometrical statements and concede meaning only to the completely developed theory of relativity (which still does not exist at all as a finished product)? Must you not grant that no 'meaning' whatsoever, in your sense, belongs to the individual concepts and statements of a physical theory, such meaning belonging instead to the whole system insofar as it makes 'intelligible' what is given in experience? Why do the individual concepts that occur in a theory require any separate justification after all, if they are indispensable only within the framework of the logical structure of the theory, and if it is the theory as a whole that stands the test? (Einstein 1949, p. 678)[33]

Not only is this a strikingly clear statement of the implications of Duhemian holism for our understanding of the empirical content of scientific concepts and theories, it is also a remarkable anticipation of the more famous criticism of the verificationist theory of meaning that Quine advanced independently two years later in his well-known essay, 'Two Dogmas of Empiricism' (Quine 1951).[34]

4. CONCLUSION

A sympathy for Duhemian conventionalism, with its emphasis on underdetermination and theoretical holism, was an abiding and central feature of Einstein's mature philosophy of science. It is one of the keys to an understanding of his attitude toward neo-Kantianism as well as his attitude toward logical empiricism. And it is a measure of the significant (if sometimes almost subterranean) influence that Duhem's philosophy of science has exerted throughout our century.

Let me offer now a final anecdote showing clearly where Einstein's sympathies lay. It concerns not Duhem directly, but the wonderful image that Otto Neurath introduced for representing the Duhemian ideas of holism and underdetermination, where he compares theory choice to our having to reconstruct a ship not on firm footing in a dry dock, but at sea, one plank at a time (Neurath 1932, p. 206). The story is found in Rudolf Carnap's diary. On the 16th of November 1952, Einstein's longtime friend Paul Oppenheim brought him to visit Carnap, who was then staying in Princeton. The conversation touched upon several topics, turning eventually to the subject of reality. Carnap records this exchange:

(2) On reality. I say that only Mach advanced such formulations according to which the sense data are the only reality. He says that the positivists nevertheless want to start from something securely given, and that there is no such starting point. I agree: there is no rock bottom, Neurath's reconstruction of the ship afloat. With that he emphatically agreed. (RC 025-80-01)[35]

NOTES

* I wish to thank the Hebrew University of Jerusalem, which holds the copyright, for permission to quote from the unpublished letters of Einstein. Items in the Einstein Archive are cited by giving their number in the control index after the following format: EA nn-nnn. Similar formats are employed for citing other archival material. Thus 'AA' refers to material in the Adler Archive at the Verein für Geschichte der Arbeiterbewegung, Vienna; and 'RC' refers to material in the Rudolf Carnap collection at the Archive for Scientific Philosophy, Department of Special Collections, Hillman Library, University of Pittsburgh. The research for this paper was supported in part by a grant from the National Science Foundation, No. SES-8420140, as well as by grants from the Deutscher akademischer Austauschdienst, the American Philosophical Society, and the University of Kentucky Research Foundation.

[1] See Neurath 1916, p. 27, 1932, pp. 213–14, and the numerous references to Duhem in Neurath's collected philosophical papers, Neurath (1983).

[2] "When I wrote 'Two Dogmas of Empiricism', I had not read Einstein's reply to Reichenbach, nor did I know of Duhem. My holism there was just my own common sense, plus perhaps some influence from Neurath's congenial figure of the boat. After 'Two Dogmas' appeared, January 1951, both Hempel and Philipp Frank told me about the kinship of my view to Duhem's; so I added the footnote citation of Duhem when 'Two Dogmas' was reprinted in *From a Logical Point of View*, 1953." (Private communication, 9 October 1986.) On the connection to Einstein, see below, note 34.

[3] There are many reasons for the neglect. Foremost among them must be the fact that Neurath, whose thinking most clearly reflected the influence of Duhem and who would have been Duhem's foremost advocate, died immediately after the second world war (1945), and never had the same opportunity as Reichenbach or Carnap to represent Viennese philosophy of science to an English-speaking public. As it turned out, Neurath's views were not often fairly represented, being interpreted to us primarily by the born-again physicalist, Carnap, who never really appreciated how different were his and Neurath's views. English-speaking philosophers – most of whom did not read Poincaré or Duhem in the original – came to know conventionalism only in the form in which it was presented by Schlick, Reichenbach, and Carnap. But as is explained below in Section 2, this version of conventionalism differs significantly from that of Duhem.

[4] Mach also worried that the Catholic Duhem had an ulterior motive: "Given the power and influence that scholasticism and Catholicism still have in France, it is nevertheless possible that Duhem is nurturing some kind of devil in the background; he is after all an admirer of Thomas Aquinas, something of which he makes no secret at all. But of what consequence is that, as long as he does not turn the devil loose? Perhaps he wants to free physics of metaphysics only in order to win elbow-room for the latter over against physics. Philosophers and theologians can do what they will with metaphysics. If by that the physicists, physiologists, and psychologists accustom themselves to making do without metaphysics, then all is won". He concluded: "For the time being I am quite content with the degree of agreement with Duhem". (Mach to Adler, 22 April 1908, in Blackmore and Hentschel 1985, p. 50.)

[5] Mach writes: "I was very pleased by Duhem's work, 'La Théorie physique, son objet et sa structure' (1906). I had not yet hoped to find such thoroughgoing agreement on the part of physicists. Duhem repudiates any metaphysical conception of questions in physics; he views the conceptually-economical determination of the factual as the aim of physics . . . . The agreement between us is all the more precious to me, since Duhem arrived at the same results wholly independently". (1906, p. x.)

[6] "Claude Bernard advises us to disregard all theory in experimental investigations, to leave theory at the door. Duhem rightly objects that this is impossible in physics, where experiment without theory is incomprehensible . . . . In fact, one can only recommend that attention be given to whether or not the experimental result is on the whole compatible with the assumed theory. Cf. Duhem (La Théorie physique, pp. 297f)". (Mach 1906, p. 202, n. 3.)

[7] "Duhem (La Théorie physique, pp. 364f) explains that hypotheses are not so much *chosen* by the researcher, arbitrarily and at will, but rather *force* themselves *upon* the researcher in the course of historical development, under the impress of facts that are gradually becoming known. Such a hypothesis usually consists of a whole complex of ideas. If a result then arises, e.g., through an 'experimentum crucis', that is incompatible with a hypothesis, then for the time being one can only regard it as contradicting the *entire complex of ideas*. On this latter point cf. Duhem, l.c., pp. 311f". (Mach 1906, p. 244, n. 1.)

[8] What form these 'epistemological discussions' took is not clear. There was a modest correspondence between Duhem and Mach, lasting from 1903 to at least 1909, but as Stanley Jaki puts it, the letters 'contain only generalities' (1984, p. 380). The 'valuable' critical remarks were presumably those contained in Duhem's review (1903b) of the French edition (1904) of the *Mechanik*, remarks for which Mach thanked Duhem in a letter of 15 May 1904 (published, along with the rest of the Mach–Duhem correspondence, in Hentschel 1988, p. 78).

[9] See Frank (1917) for a summary of Frank's views on Mach. For more on the relationship between Mach and Frank, see Frank (1941), pp. 18–30, Blackmore (1972), and Wolters (1987) (which corrects a number of errors in Blackmore).

[10] Rey (1907), pp. 392ff; cited in Frank (1949), p. 21. In one essay Frank called Duhem the "most important representative of the Machian line of thinking in France" (Frank 1917, p. 66).

[11] For more on the relationship between Mach and Duhem, see Paty (1986), Jaki (1984), pp. 319–73, and Blackmore (1972), pp. 196–7. For different reasons, the latter two

discussions should be read with care. Blackmore exaggerates the extent of Mach's influence on Duhem, whereas Jaki too quickly dismisses the "stereotype classification of Duhem as representative of positivism" (p. 358), arguing that it ignores the significant realistic and metaphysical strain in Duhem's thinking. Jaki does not stress sufficiently the distinction – absolutely necessary for understanding the contemporary reception of Duhem's views – between Duhem's broader philosophical, metaphysical, and theological commitments (clearly recognized by Mach, as shown by the letter to Adler quoted above in note 4) and his more restricted views on the methodology of physics. It was the latter that excited the interest of contemporary philosophers of science, and it would be seriously misleading to describe Duhem's views on scientific methodology as a version of scientific realism; the Duhemian thesis of underdetermination is inherently antithetical to a realistic conception of scientific method. Some of the most insightful comments on the relationship between Duhem and Mach and on Duhem's influence on the members of the Vienna Circle are found in the work of Rudolf Haller; see especially Haller (1982, 1985, 1988).

Another implication of Mach's sympathy for Duhem should not be overlooked. The Mach who is so enamored of Duhem cannot be the niggardly positivist often presented to us in the secondary literature. In particular, the Mach who reads *La Théorie physique* with enthusiasm must have a more liberal attitude toward the role of hypotheses in physics than many of his critics grant him. One scholar who has already argued for this more liberal interpretation of Mach on other grounds is Gereon Wolters; see Wolters (1987), pp. 101–20, and (1988).

[12] For more on Einstein's attitude toward Mach, see Blackmore (1972), pp. 247–85 and Wolters (1987), pp. 11–171, which corrects some errors in Blackmore's treatment of these topics.

[13] Einstein studied at the ETH from 1896 to 1900; for documentation on Einstein's years at the ETH, see Einstein (1987). Adler was at the University of Zurich from 1897 to 1901; see Ardelt (1984), pp. 71–111.

[14] Adler's dissertation was completed in 1902 (Adler 1902); for background, see Ardelt 1984, pp. 101–11. Einstein's was completed in 1905 (Einstein 1905) after an abortive earlier attempt at about the same time Adler finished his dissertation; for background on both the 1905 dissertation and the earlier attempt, see Einstein (1989), pp. 170–82.

[15] Einstein, of course, had been working since 1902 as a clerk in the Swiss Federal Patent Office in Bern (see Seelig 1960, pp. 89–160). Adler had been a Privatdozent in physics at the University of Zurich for the previous two and one half years, having received on 13 December 1906 the *Venia legendi* for "experimental and theoretical physics, as well as their history and epistemological foundations" (Ardelt 1984, p. 157–66).

[16] For background on the relationship see Seelig (1960), pp. 162–4. The lectures they attended were either Minkowski's lectures on 'Analytische Mechanik', winter semester 1898/1899, or his lectures on 'Anwendungen der analytischen Mechanik', summer semester 1900 (Einstein 1987, pp. 367, 369). See also Adler to Heinrich Braun, 22 January 1919, EA 6-013: "I have been well acquainted with Einstein from our time together as students in Zurich".

[17] See Seelig (1960), p. 165. Adler himself recalled these conversations in the first letter he wrote to Einstein after his imprisonment for assassinating the Austrian Minister-President, Count Stürgkh; see Adler to Einstein, 9 March 1917, EA 6-001.

[18] In March 1911, Einstein moved to Prague to take up the chair in physics at the Charles

University (Seelig 1960, p. 203). At the end of May, Adler moved to Vienna to take up a position as secretary to the Austrian social democratic party, one of whose founders was Adler's father, Viktor (Ardelt 1984, p. 215).
[19] For more on Einstein's first acquaintance with Mach, see Wolters (1987) and the introduction to Einstein (1989). Einstein frequently expressed his debt to Mach; see for example Einstein (1916).
[20] Ardelt (1984) is also a good source to consult on the interesting role played by Mach's philosophy of science in debates over the interpretation of Marxism that pitted Adler and many of his Austrian colleagues, who were influenced by Mach's anti-metaphysical arguments, against doctrinaire materialist Marxists like Lenin. Adler is one of the targets of criticism in Lenin's *Materialism and Empirio-Criticism* (see, for example, Lenin 1909, p. 46).
[21] See, for example, Adler (1908a). For more on the relationship between Adler and Mach, see Ardelt (1984), Blackmore (1972), and Wolters (1987).
[22] Adler (1909), Planck (1909). Adler's paper appeared on 26 December 1909. Mach himself replied the following year (Mach 1910), eliciting a final rejoinder from Planck, in which Adler's reply is cited (Plank 1910, p. 1188).
[23] Near the end of a letter to Adler of 26 July 1909, Mach asks: "Is Einstein still in Bern? I want to send him a copy also". (Mach was referring to a copy of the new second edition of his *Erhaltung der Arbeit*, 1909.) Einstein's first letter to Mach of 9 August 1909 (EA 17-410) indicates that he received the book sometime during the intervening fourteen days. In that letter, Einstein too expressed sympathy for Mach in the debate with Planck (Einstein to Mach, 9 August 1909, EA 17-410). The mentioned letters are reprinted in Blackmore and Hentschel 1985, pp. 58–59.
[24] It was Adler who first suggested the project to Mach in the fall of 1906 after reading the second edition of Mach's *Erkenntnis und Irrtum* (1906), in which, as noted above, Mach had praised the book. As it turned out, Mach had already recommended translation of the work to the publisher Barth, who then recruited Adler for the task. See Adler to Mach, 19 October 1906 (AA 130), Mach to Adler, 20 October 1906 (AA 130), and Adler to Mach, 10 November 1906 (AA 130). These details are provided in Ardelt (1984), p. 293, n. 16.
[25] The only other serious candidate for 'the clear book by Duhem' is *L'Évolution de la mécanique* (Duhem 1903a), which appeared in the German translation by Philipp Frank in (1912). But not much in this book would bear directly on the positivist critique of hypotheses, whereas the latter is an important theme in *La Théorie physique*.
[26] See Schlick (1915, 1917, 1918). For further discussion of these issues, see Howard (1982, 1984, 1987, 1988).
[27] Between 1919 and 1925 there was a floor of books and articles of this kind coming both from critical realists in the tradition of Oswald Külpe and Alois Riehl and from critical idealists in the Marburg tradition of Hermann Cohen and Paul Natorp. See, for example, Sellien (1919), Cassirer (1921), Schneider (1921), Winternitz (1923), and Elsbach (1924). For a helpful survey of the neo-Kantian reaction to relativity theory, see Hentschel (1987).
[28] See especially Schlick (1921, 1922), and Einstein (1924a, 1924b).
[29] Duhem wrote: "Contemplation of a set of experimental laws does not, therefore, suffice to suggest to the physicist what hypotheses he should choose in order to give a theoretical representation of these laws; it is also necessary that the thoughts habitual

with those among whom he lives and the tendencies impressed on his own mind by his previous studies come and guide him, and restrict the excessively great latitude left to his choice by the rules of logic. . . . On the other hand, when the processes of universal science have prepared minds sufficiently to receive a theory, it arises in a nearly inevitable manner and, very often, physicists not knowing each other and pursuing their reflections at a great distance from each other generate the theory at the same time. One would say that the idea is in the air, carried from one country to another by a gust of wind, and is ready to fertilize any genius who is disposed to welcome it and develop it, as with pollen giving birth to a fruit wherever it meets a ripe calyx. . . . Logic leaves the physicist who would like to make a choice of a hypothesis with a freedom that is almost absolute; but this absence of any guide or rule cannot embarrass him, for, in fact, the physicist does not choose the hypothesis on which he will base a theory; he does not choose it any more than a flower chooses the grain of pollen which will fertilize it; the flower contents itself with keeping its corolla wide open to the breeze or to the insect carrying the generative dust of the fruit; in like manner, the physicist is limited to opening his thought through attention and reflection to the idea which is to take seed in him without him". (Duhem 1906, pp. 255–56)

[30] See also Schlick (1936).

[31] Reichenbach's books on relativity are even better known sources for essentially the same conception of the role of conventions; see especially Reichenbach (1924, 1928).

[32] This essay is often misread as a repudiation of conventionalism, since Einstein's principal aim was to criticize Poincaré's conventionalist defense of Euclidean geometry, arguing that when geometrical primitives ('rigid body') are given physical interpretations ('practically rigid rod') geometry becomes an empirical science. But all Einstein denies is that one would always choose to save Euclidean geometry owing to its simplicity relative to alternative geometries. He still asserts that our choice of a total theory – geometry plus physics – is conventional, determined primarily by considerations of simplicity, and he concludes: "In my opinion, Poincaré is correct, *sub specie aeterni*, in this conception" (Einstein 1921, p. 8).

[33] I have corrected the translation on the basis of Einstein's original German text, which was published in Einstein (1954), p. 503, the German edition of Schilpp (1949).

[34] Quine was unaware of Einstein's criticism when he wrote 'Two Dogmas': 'I never met Einstein, and I saw him only once – fifty years ago [1936], when he addressed the Harvard tercentenary. . . . When I wrote 'Two dogmas of empiricism'. I had not read Einstein's reply to Reichenbach". (Private communication, 9 October 1986). Quine does acknowledge his possibly having been influenced by Neurath (see above, note 2), who may also have been a source for Einstein's ideas, though I have found no reference to Neurath by Einstein. It is more likely that Paul Oppenheim discussed these questions with Einstein during the 1940s. See below, section 4.

[35] Quoted here from the transcription made by Richard Nollan from Carnap's original, which is in Stolze-Schrey shorthand. Quoted by permission of the University of Pittsburgh. All rights reserved.

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PIERRE DUHEM'S *THE AIM AND STRUCTURE OF PHYSICAL THEORY*: A BOOK AGAINST CONVENTIONALISM

ABSTRACT. I reject the widely held view that Duhem's 1906 book *La Théorie physique* is a statement of instrumentalistic conventionalism, motivated by the scientific crisis at the end of the nineteenth century. By considering Duhem's historical context I show that his epistemological views were already formed before the crisis occured; that he consistently supported general thermodynamics against the new atomism; and that he rejected the epistemological views of the latter's philosophical supporters. In particular I show that Duhem rejected Poincaré's account of scientific language, Le Roy's view that laws are definitions, and the conventionalist's use of simplicity as the criterion of theory choice. Duhem regarded most theory choices as decidable on empirical grounds, but made historical context the main determining factor in scientific change.

Duhem's famous book *La Théorie physique* is almost universally considered one of the most significant documents of that cultural movement addressed against positivist optimism. Reflecting on the crisis of nineteenth-century mechanism, at the beginning of our century, this movement generated an instrumentalistic conception of scientific knowledge. Duhem's text has always been considered one of the most brilliant and vital – perhaps the most vital – of the conventionalist movement, the skeptical, philosophical answer to the difficulties of classical science.

The study of Duhem's intellectual biography (Maiocchi 1985) has led me to reach conclusions in many ways diametrically opposed to traditional judgments. These may be synthesized in a formula which is only apparently paradoxical: the main intent of the *Théorie physique* was to oppose instrumentalism, subjectivism, and the devaluation of the cognitive power of science.

The first observation to be made, apparently a point of chronology, but of decisive importance, is that the epistemological theses contained in the *Théorie physique* of 1906 were clearly and fully expressed by Duhem in a series of articles written between 1892 and 1894 (Duhem 1892a, 1892b, 1892c, 1893a, 1893b, 1893c, and 1894a). Thus Duhem's epistemology predates the discovery of radioactive phenomena, Gouy's experiments on Brownian motion, and Kaufmann's experiments on the variable mass of the electron, as well as the introduction of quantum

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hypothesis and the first relativistic hypotheses. In short, Duhem presented his epistemological theses before the "undoing of all principles", to use Poincaré's ill-timed expression, and before the explosion of the 'crisis' of the sciences.

In fact, the thematics of crisis are totally absent in Duhem. On the contrary, all of Duhem's historical and epistemological reflections, all of his scientific work, as a researcher reveals the conviction that science was not only undergoing a period of great splendor during the late nineteenth century, but was getting rid of the errors that had accompanied it through the last three centuries! Duhem's criticism of mechanism never attacks the trust in mechanics as a theory of mathematical physics, but always and only rejects attempts to extend mechanics into a nonscientific, metaphysical sphere. Above all, his criticism of mechanism is based upon the fundamental assumption that there is a better theory than rational mechanics, i.e., generalized thermodynamics. It is the success of thermodynamics that imposes the necessity of constructing a new mechanics, not the failure of the old one. Thanks to the new generalized mechanics, the dreams of the boldest mechanists, such as Berthollet, seemed to be on the verge of coming true.

The new mechanics did not reject classical mechanics, but enlarged and generalized it. Classical mechanics became the model for rigor as well as for method (Newton's, obviously, not Descarte's), and for the form given to one's own principles, which were required to maintain the closest possible analogy with the classical ones. The new mechanics stayed close to its classical model, rather than opposing it, following the original program of energetics formulated by Rankine and carried on by the mechanist William Thomson, before the latter became what Duhem called a "modelist" (see Duhem 1893d).

In Duhem's opinion, the developments in nineteenth century science confirm the positivist belief in a continuous progress of scientific knowledge from other methodological bases: "In our days, many are being swept by a wave of skepticism", but those who force themselves to find in science "the continuation of a tradition of a slow but steady progress", will see "that a theory that disappears, never disappears completely" (Duhem 1894b, p. 124). It is not the crisis of science, but its successes which impose upon Duhem the necessity of epistemological reflection.

Duhem's interpretation of scientific theories as simple instruments of classification does not appear as an answer to a supposed crisis of mechanism. Not only is his epistemology free of such a 'crisis', but such instrumentalistic conceptions had already been present in the French milieu for several decades. Diffidence toward hypotheses, a phenomenalist view of science, and an instrumentalistic, manipulative interpretation of theories were widely diffused ideas, and dominant among the French scientists of the positivist age. The French scientists' ideal was personified by the likes of Regnault, Bertin, Berthelot, Sainte-Claire Deville, Jamin, Cornu, Violle, and Le Chatelier – all fundamentally experimenters. They sporadically showed an ideological belief in the supreme value of mechanics, although in fact they produced 'anti-modelist' physics. In many cases, their work was mathematically poor, deaf to the calls of theoretical physics and insensitive to nuances of experiment. Even more clearly than contemporary physics, the milieu of chemistry, in which Duhem was trained, showed a general mistrust of the idea that scientific theories might yield explanations, in the sense of revealing hidden truths behind phenomena. These objections appeared most clearly in the case of the atomic theory. Atomism was interpreted in the first place as a classifying tool, even by its supporters like Wurtz. Duhem's understanding of science and epistemology was fundamentally influenced by Henri Sainte-Claire Deville. In the work of this French chemist-physicist, Duhem found, even before Mach, the very clearly stated idea that every scientific theory is simply a classifying tool (see Sainte-Claire Deville 1866).

Unlike Mach in Germany, Duhem did not have to fight against dogmatic belief in the nascent cognitive power of mechanics or against a tendency to objectify models. The cognitive devaluation of theories and models was already extensively employed as a criticism by French positivism. As criticism it was not particularly discerning, but certainly historically effective. Duhem had to fight a battle in exactly the opposite direction: contrary to the flattening out imposed by the empiricist method of his predecessors, he had to avenge the rights of theory, showing how the ineliminable theoretical components present in every observation gave meaning to the scientist's experimental work. Duhem's epistemology was a defense of theories against positivist pretenses to eliminate them by strictly reducing science to pure experience. The positivists considered theories as secondary tools when compared to experience, even as superfluous and therefore, eliminable. Duhem endeavored to show that theories are the heart of a scientific venture.

His was a radically anti-inductivist, antiempiricistic epistemology, a 'praise of theories', and in that sense it was opposed to Mach's.

Even Duhem's most renowned battle against Anglo-Saxon physics cannot be understood as a fight against the identification of models and reality, denying cognitive value to the models. Duhem never identified, but rather, he always underscored the distinction between Cartesian mechanism and that of Kelvin and Maxwell: in British mechanism, Duhem immediately recognized the model had only heuristic functions; it was a working tool for the physicist who needed to 'illustrate', satisfying the need of concrete interpretation (see Duhem 1893b). The fundamental charge against the British was not the fact that they used models, but their use of models in an incoherent way, conceiving them precisely as simple instruments. Even Laplacian physics was 'modelist', and yet, on repeated occasions, Duhem referred to it as one of the great examples of a theory in physics. Anglo-Saxon 'modelism', propagated in France by Poincaré, seemed to Duhem the most dangerous variant of instrumentalism, and he fought it by taking an explicitly anti-instrumentalistic position: if theories were simple classifying tools, it would be perfectly normal to adopt various criteria for different classifications, introducing incoherence in physics by using different models to represent the same object, as the British did. Incoherence, (i.e., British physics as supported by Poincaré) can be fought only by admitting that theories are classifying tools, being neither arbitrary nor subjective, but leaning rather not toward the construction of a 'natural classification', namely, one having objective significance.

Theoretical coherence obsessed Duhem's research. He sought the rigorous structuring of scientific terms in a deductive, hypothetical system which conceded nothing, in matters of rigor, to intuition or common sense. Generalized thermodynamics was the perfect answer to these requirements (and where it fell short, Duhem took great pains to make the necessary corrections). Anglo-Saxon 'modelism' instead proposed an uncoordinated physics, a gallery of images that, due to lack of coherence, could not be judged a theory. But coherence was sustainable and justifiable only by admitting that theories, inasmuch as they are constructed to organize mathematically the world of phenomena, are also capable of reflecting an ever-perfectible and always 'more perfect' real arrangement, rather than a subjective one. We know with certainty – according to Duhem in 1893 – that relations among material substances are "neither undetermined nor contradictory". Therefore, when faced with physics proposing two irreconcilable theories:

> We are certain that the classification proposed by such physics is not in conformity with the natural order of 'the laws . . . making the incoherence disappear, we will have some probability of bringing it closer to that order to make it more natural, thus, more perfect . . . . (Duhem 1893c, pp. 369–70)

The idea of 'natural classification' was judged an extrinsic ideological addition, and even contradictory with Duhem's epistemology. Yet this idea was enunciated from 1893 as the methodological axis carrying the fight against British physics and in favor of generalized thermodynamics. Without it, all of Duhem's scientific work would be meaningless. Not only that, but the whole of his epistemology and his historical work was an effort to sustain this notion. The pivotal problem around which all of the *Théorie physique* hinges is just that: how to reconcile an unprejudiced, pitiless and extremely acute critique of the scientist's work with the idea of a science that has cognitive value. How does one criticize the dogmatic empiricism of positivism without falling into the subjectivity of instrumentalism? In order to understand why this problem had become so important in Duhem's eyes during the first few years of the new century, we should remember the genesis of the *Théorie*, and the framework within which it was generated.

During the years 1892–94, Duhem took up the fight against the basic positivist empirical notion of science and against Anglo-Saxon 'modelism', which was still encountering noticeable diffidence among the French. From those years to the year 1906, the year of publication of the *Théorie*, a number of riotous overlapping events considerably changed the French scientific and cultural scene. A series of upsetting experimental discoveries and an equally surprising sequence of theoretical elaborations (especially tied to Lorentz's theories) imposed a realistically interpreted atomic theory, together with Maxwell's electromagnetism, upon the younger French scientists such as Perrin and Langevin. These events diffused the theme of scientific crisis.

The victory of atomism and electromagnetism meant victory – or at least seemed to – for that modelism Duhem thought already defeated. These scientific events accompanied and even favored changes of great importance in the French philosophical panorama, which was characterized by the ever-increasing success of instrumentalistic, anti-intellectual, and subjective concepts. Beginning in the 1890s, with the explosion of the celebrated debate on the 'bankruptcy of science', French philosophy was deeply marked by an impetuous blossoming of anti-intellectual currents, such as Bergsonianism and modernism, radical conventionalism, and varied forms of spiritualism. To use Fouillée's famous words, this period saw the "revolt of the heart against the intellect". Modelism and instrumentalism, English physics, atomism and exasperated conventionalism, the crisis of science, anti-materialism and spiritualistic skepticism seemed to form a thick web destined to surround and suffocate the model of scientific rationalism elaborated by Duhem in the 1890s.

To fight these foes, Duhem published a series of works in the early 1900s. The *Théorie* represents the ultimate battle of this campaign. In 1902, he criticized electromagnetic theory very harshly in *Les Théories électriques de J. Maxwell.* During the same year he attacked atomism with *Le mixte et la combinaison chimique.* In 1903, with *L'évolution de la mécanique*, he confronted the more generalized critique of the 'modelist' approach in its diverse historical variants. And finally, in 1904, Duhem started publishing for the *Revue de Philosophie* a series of articles which were eventually collected (with some additions) in 1906 to form *La Théorie physique.* Here he fought the conventionalism then in style on the epistemological level. The *Théorie physique* was, therefore, not at all a book opposing the positivism of the 1800s, in the name of the new century's revolution in physics. It was a work against the emerging novelties intended to demonstrate that the criticisms brought a decade earlier against the positivist conception of science need not give way to the skeptical conclusions that seemed to follow directly from these novelties.

An analysis of the text of the *Théorie* confirms the interpretation which has led me to give the history of this work's genesis. For reasons of space, it is impossible to carry out a detailed analysis here, but some indications may be given: on all the key problems of epistemology (what is a scientific fact? what is a law? how does one choose theories?), Duhem clearly takes a position critical of the main conventionalists, primarily Poincaré and Le Roy, and fights against their supposed solutions. I will briefly consider some examples.

Le Roy had given a rather strong subjectivist interpretation of the 'scientific fact', starting with the analyses made by Duhem in the 1890's and maintaining that, due to the ineliminable theoretical components present in every experience, the 'scientific fact' is to a certain extent 'created' by the subject (Le Roy 1899, p. 516). Poincaré had retorted by trying to subdue this radical subjectivism, maintaining that what the scientist creates is the language with which we ask nature questions, and it is then nature's task to give the answers (Poincaré 1905, p. 266 et seq.).

Duhem argues at length even against Poincaré's mitigated version of conventionalism. The theme of science understood as a well-made language is certainly not new: from Condillac and Lavoisier, through the *Ideologues*, it had gone through positivism and had almost become commonplace. It was directly connected to a depreciation of theories, reducing them to the role of dictionaries which, through obviously conventional rules, allowed the scientist to translate experience into language. The view had, in fact, been emphasized by radical conventionalists like D'Adhémar (D'Adhémar 1904). Against these general positions Duhem emphasizes (Duhem 1906, p. 266) that science differs from other languages as to its terms, just because they are defined within a theoretical context, stabilizing multiple interconnections in a network of relationships between term and term, concept and concept, not to mention relationships among some terms and groups of phenomena. A scientific fact is not differentiated from a nonscientific fact only because it is expressed in a language resulting from customs known only by a small group of people (Poincaré's thesis). Its main characteristic is that of belonging, by virtue of the theories that we use to express it, to an intricate network of relationships with theoretical terms and with a multitude of other scientific facts. When we translate a raw fact into a scientific fact, we do not simply construct a proposition using the expressions of a language provided with conventional rules known by a small group of people (the scientists), we do much more. We insert that fact in a sequential scheme, including other facts, and we recognize relationships among phenomena. However, the linguistic translation of the raw fact to the scientific fact is not simply made by choosing the rules of translation freely and conventionally. It is guided by the theories allowed at a given historical moment, and the result of the translation work is, therefore, not invented by the scientist; it is the result of history. It depends upon the level that science has reached at a given historical moment. Science as a means of human expression is, in fact, a language, but a language radically different from all others.

Duhem contributed more than anybody else to the criticism of positivism's dogmatic concept of science's empirical basis. His analysis of the impossibility of crucial experiments is famous, but his criticism is not limited to the denial of the notion of the empirical basis of science. The distinction between the theoretical and the observational is unsustainable for Duhem from the logical point of view, because, in the mature sciences, every observation is impregnated by theories. Positivism taught (just as neopositivism would in the future) that such a distinction was logical, and therefore absolute. For an absolute distinction between theoretical physics and experimental physics, Duhem substituted an historical distinction: there exists at every historical moment the heritage of previous history; a body of 'trusted' theories which guarantees the experimental physicist the possibility of making 'observations' without having to doubt every concept used. Thus science may progress, constantly increasing the theories it trusts (the 'background knowledge' in modern terms). It makes statements which, from the logical point of view, are unavoidably 'theoretical'. They are theoretical at the moment they are proposed, but they become increasingly more 'experimental' as they are provided with increasingly more guarantees of their validity.

Even on the notion of scientific law Duhem's views are opposed to Poincaré, Milhaud, and especially Le Roy. His critique centered upon the typically conventionalist affirmation that laws are used as definitions: in the presence of an experiment which seems to falsify the law, we do not reject it, but we say that the present case does not fit those for which the law was defined, that there are upsetting causes that the conditions of the applicability of the law did not foresee. Experiments, therefore, will never be able to force us to reject the laws; they are not falsifiable (Le Roy 1899, p. 523). Evidently, this approach empties scientific laws of any empirical content. It makes the rigorously required comparison with experience useless for the development of science, since any problem may be resolved by exercising an inventive activity which saves a law by placing it beyond experience. For Duhem, the stern comparison, the refutation of experience, has a result which is only apparently identical to Le Roy's rescue of law thanks to the addition of new perturbing causes, but actually has diametrically opposed objectives to that of instrumentalism. For Duhem, in fact, when confronted with a denial of experience we save a law by specifying its conditions of validity, and that constitutes cognitive progress. It is true, we use the law as a criterion to establish whether or not the conditions under which it is considered valid are respected; but it is experience which allows us to establish the conditions of validity. Not every refutation falsifies the law. Some give us information about its limits of validity, and this constitutes progress. Difficulties are not resolved in a clever, intellectual game of rescue. They are resolved in a symbolic representation by adhering more closely to reality. Each one of our laws is necessarily a poorer scheme than the reality it wants to represent. The failures, the falsifications, force us to refine the theoretical scheme and to complete it in order to "make it more suitable to represent reality" in an unending process of perfection (Duhem 1906, p. 285; 1954, p. 174). In this process it is the falsifying experience that teaches us the conditions of the law's validity and the restrictions to which the primitive terms of law are submitted. Experience is not an enemy from which, with more or less astute devices, one must seek protection, it is the source of the perfecting process of the theoretical scheme: "The necessity of these restrictions didn't appear at all in the beginning, it was imposed by experience." (Duhem 1906, p. 287; 1954, p. 176). The work of continuous minor repairs, through which the laws of physics avoid the denials of experience, does not have the function of saving a law by petrifying it into the limbo of conventionalism, but it plays an "essential role in the development of science" (Duhem 1906, p. 288; 1954, p. 176). What is important from Duhem's point of view is not the rescue of the law, but the progress of the theoretical scheme, which is realized in the attempt to resolve the issues raised by a falsifying experience:

> It is through the unending struggle of this work which continually completes the laws to the end of including exceptions, that physics is able to progress . . . it progresses because without interruption, experience is forever causing the explosion of new contradictions among the laws and the facts and without interruption, physicists rectify the laws so that they may represent facts more accurately. (Duhem 1906, pp. 289–90; 1954, p. 177)

Even the problem of choosing the basic hypotheses of a theory, which Poincaré and Le Roy had resolved in terms of a conventional choice, receives in the *Théorie* a solution that decidedly finds fault with the main lines of subjectivism. Moreover, it is just in this respect that the most important and significant variation between the articles of the 1890s and the book of 1906 should be considered. The problem of the choice of hypotheses had been dealt with in *Quelques réflexions au suiet* *des théories physiques* (1892), in criticisms of the positivist dogmatism extolling the creative freedom of the researcher. In this article, Duhem had maintained that hypotheses are chosen freely and that choices are guided by subjective criteria, mostly that of simplicity. The *Théorie physique* repeats almost to the letter all of that article's criticisms of positivism, but the paragraph dedicated to the choice of hypotheses is completely ignored, together with all those passages containing rather excessive conventionalism!

In the *Théorie*, Duhem shifts the problem from the field of logic, of metahistorical methodological criteria, to the field of history. The subjective criteria of choice so dear to instrumentalistic conventionalism are no longer given space because Duhem is convinced of the fact that even if they are possible from the abstract point of view, the scientist, in reality, does not use them to make his own choices. He does not use them because in the concrete cases of historical evolution, the scientist does not make choices of any kind. The theory or hypothesis germinates within him without his concurrence. This means, in a less paradoxical form, that logical criteria are altogether insufficient to guide theory choice and, relying only upon them, the scientist would remain paralyzed in his progress by excessive freedom. This had been the objection which, in a ferocious attack, the neo-Thomist Vicaire had addressed to Duhem's article of 1892 (Vicaire 1893, p. 79). Duhem, in the following year had rectified his own position, maintaining in *L'école anglaise et les théories physiques* that in the choice of hypotheses, a scientist is never guided by logic alone:

> The particular inclination of his spirit, his prevailing faculties, the diffused doctrines in his environment, the tradition of his predecessors, the habits he has adopted, the education he has received, will be his guide, and all of these influences will be found again in the form of the theory he will conceive. (Duhem 1893b, p. 377)

In the *Théorie*, this idea is extensively developed and represents one of the basic theses of the whole work: the historical context, in which every scientist moves, guides the choice of hypotheses; these are the concrete influences that every stage of development of the historically determined scientific thought exerts upon the researcher, resulting in the generation of new ideas. These ideas are the product of all of the foregoing evolution, without which they could not be created; they are 'the last stage of a long development'. Since whoever contributes to scientific progress is so immersed in his contemporary historical context that he cannot move freely, every new hypothesis can only be a modification of already-stated hypotheses. The history of science must be a continuous development (Duhem 1906, pp. 364 *et seq*. and 416 *et seq*.).

The thesis of historical continuity is one part of his epistemology with which Duhem attempts to resolve the problem of the choice of hypotheses. And it is a thesis which has a very important result: if in 1892, relying upon logical criteria of choice, such as simplicity, Duhem had not been able to avoid the acceptance of an instrumentalistic vision of science, now, leaving the problem of choice to the thesis of historical continuity he can sustain a realistic and cognitive vision of the scientific enterprise.

> Showing the physicist the continuing tradition through which science of every age is nourished by the previous century's systems, through which it is pregnant with the physics of the future, citing to the physicist the prophecies that theory has formulated and that experience has confirmed; it creates and strengthens in him the conviction that a physical theory is not at all a purely artificial system, useful today and useless tomorrow, that it is an always more natural classification; an always clearer reflection of reality that the experimental method could never bear in a face-to-face contemplation. (Duhem 1906, p. 445; 1954, p. 270)

What can be said then about the historiographical scheme, that makes Duhem (and conventionalism in general) the advocate of a vision of science proceeding on the basis of choice and decided by a criterion of simplicity? In my opinion, the only possible answer is that the scheme is completely wrong. Except for the paper of 1892, Duhem never admitted that simplicity could be a sufficient criterion for choice. Obviously, like every reasonable person, he considered the simplicity of a theory part of its merit, but he certainly didn't use it as a guideline.

Duhem's vast historiographical work clearly shows that the case of empirically equivalent rival theories is in his opinion extremely rare, and that for the vast majority of cases it is empirical factors that supply a clear criterion of nonsubjective choice. Great theoretical disputes were resolved, according to Duhem, by the superiority of one theory over a rival one in 'saving the phenomena', without any necessity at all to resort to criteria such as simplicity. For example, this is the case of the clash between Ptolemy and Copernicus. It is a very wretched historiographical thesis that, according to conventionalism, the heliocentric system won over the geocentric system because it was simpler. In *To Save the Phenomena*, where Duhem confronts the problem, the sixteenth-century victory of the Copernican system, intended as a calculating instrument, is always explained on the basis of the greater precision that the system allowed in the construction of astronomical tables. There are times (although few) that simplicity is also mentioned along with precision, but this attribute never appears by itself. It is always an additional quality which certainly does no harm to the Copernican system, but that certainly can not explain its victory just for its sake. When commenting on sixteenth-century astronomers following Copernicus, from Reinhold to Peucer, from Schreckenfuchs and Piccolomini to Giuntini, Duhem uses the term of simplicity along with 'precision' only once, when referring to Reinhold. In all other cases, what is always and only considered is the calculating precision obtainable starting from the Copernican hypotheses. Duhem concludes his analysis of the victory of Copernicus's theory with the astronomers thus:

> The spirit of the greater part of the astronomers, during the 20 or 30 years following the publication of Copernicus' book is very clear. The work of the astronomer from Thorn attracts their attention very strongly because it appears to be suited for the construction of *precise* astronomical tables. (Duhem 1908, p. 509; 1969, p. 8)

In the first place, therefore, the criterion of simplicity turns out to be irrelevant and superfluous for the vast majority of theoretical disputes. But even when facing those cases where we are in the presence of equivalent empirical ranges in two rival theories, Duhem never considers simplicity a decisive element, capable of generating a choice endowed with any solidity. The only example of empirically equivalent theories contained in *Le système du monde* is made up of two different astronomical representations. Appolonius of Perga had proved epicycles and eccentrics to be equivalent with respect to observational effects; in 244 B.C., Hipparchus, when faced with this surprising discovery, refused to make a decision in favor of either representation. Now, in Duhem's opinion, this attitude was not due to the astronomer's uncertainties or inability but, on the contrary, it was the attitude of one who follows the correct scientific method; when confronted with theories equivalent from the observational point of view and in the absence of other references capable of guiding the choice (which could be other already accepted theories), he refuses to choose. Although using one theory because it is judged simpler, the astronomer does not condemn the other and does not discard it as a possible alternate tool (Duhem 1913–59, Vol. I, pp. 455–60). It is clear in this case that Duhem does not consider simplicity the only criterion sufficient in founding a definitive theoretical choice. In other cases of empirically equivalent theories examined in detail by Duhem in his various works, the notion of simplicity is always relegated to the background. That is the case, for example, with the contrast between Lagrange's *Mécanique analytique* and Poisson's *Mécanique physique* where the fundamental criterion for evaluating the superiority of one over the other is seen in the relationship between the type of mathematics used and the model upon which such mathematics are applied (Duhem 1905, p. 83 *et seq.*).

The fact that the criterion of simplicity takes on a completely secondary historical role in Duhem should not surprise us, if we remember its previously stated position in the historical dynamics of the relationship between abstract and concrete, which is apparent in scientific laws. According to Duhem, the symbolic schemes produced by the scientist are always impoverished when compared to the reality they are supposed to symbolize. The modification of the theoretical scheme always happens through its increasing complication as it attempts to represent all the richness of the experience. This is also the case for theories: in Duhem's opinion historical progress generally creates an increasing complexity of theoretical physics and only in some particular cases (for example with Copernicus's theory) do we have a simplification which, in any case, cannot by itself justify the acceptance of a theory. At this point, consider once more *Le système du monde*. Here all of the ancient and medieval history of astronomy is recounted as a progressive increase of complexity in function, to create a better adaptation to observational data (Duhem 1913–59, vol. I, pp. 129 and 201). And in *To Save the Phenomena* Duhem advises that the complication of a theory cannot be considered the sole motive for rejecting it:

> The exact representation of celestial movements may force the astronomer to gradually complicate his hypotheses, but the complexity of the system where he will have stopped cannot be a reason to reject such system if it is in full accord with the observations. (Duhem 1908, p. 129; 1969, p. 17)

The most conclusive demonstration of how much Duhem considered scientific progress to be substantially characterized by an increase of complexity is found in considerations regarding the relationship between mechanics and general thermodynamics. Just because it is a more limited theory, capable of covering a lesser number of observational data compared to general thermodynamics, mechanics is much simpler.

Conversely, because it is a better-suited to phenomena, general thermodynamics or energetics is even more complex than the theory it wants to replace.

> The new mechanics founded on thermodynamics has not at all imposed upon its essential hypotheses the exaggerated simplicity required by the old mechanics: it has tolerated their being more numerous and more varied allowing them to express themselves with more complex formula. This greater amplitude left to the choice of principles proved to be a happy and fruitful one. (Duhem 1905, p. 343)

Here then, is a rather difficult historiographical problem for the supporters of Duhemiam simplicity: the new mechanics, that to which Duhem dedicated all of his work as a researcher, is considered more complex than the old mechanics. The revolution brought about to thermodynamics in the chemical and physical sciences proceeds from the simpler to the more complex!

In the *Théorie* Duhem intended to show how to avoid skepticism without abandoning any of the criticisms of dogmatic positivist empiricism he had made over a decade before. The attempt is rather risky: it is a question of constantly maintaining a balance on a metaphorical thread with the recurring risk of falling on the one side into dogmatism, on the other side into skepticism. It is clear that it is this second danger that Duhem fears most because, as a matter of fact, his juvenile theses had been interpreted as skeptical. And it is here that Duhem develops a constant and tight polemic against Mach, against Poincaré and against Le Roy. It is astounding how the critics have not taken into account these extremely clear Duhemian passages which represent the most lucid, articulate and effective polemic against conventionalism with a skeptical note. On all fundamental epistemological questions (what is experience? what is a law? what is a theory? what is the nature of science?), Duhem both reproposes and confirms his own juvenile theses; moreover, he is concerned to show how from those theses one doesn't necessarily have to reach the depreciative conclusions of his false friends. If all its pages are to be taken seriously, and not just those on the critique on positivist dogmatism which in 1906 were the most worn-out for Duhem's readers, the total vision of science that emerges from the *Théorie* is evidently a vision that is rather far from any trust in science void of criticism. But it is also a realistic conception of a science in constant movement, a science made by human beings, and as such always revisable. It proceeds by continuously retouching its own conceptual schemes, modifying them, and generally complicating them, in view of an ever better adjustment between the scientific image of reality and reality itself, a reality that has certainly not been completely reached but that becomes always more approachable.

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MAIOCCHI ON DUHEM, HOWARD ON DUHEM AND EINSTEIN: HISTORIOGRAPHICAL COMMENTS

ABSTRACT. These comments center on the methodological stance that Howard and Maiocchi recommend to us when we are doing history of philosophy. If Howard and Maiocchi are right, both Duhem and Einstein developed closely related versions of conventionalism and realism, and in both of their philosophies the conventionalist and realist moments were mutually compatible. Duhem's holism and, arguably, Einstein's as well, denies the need for across-the-board literalism, and both of them had important reasons for denying that convergence was required or even desirable for realism. Thus, for those who are caught up in the current disputes, serious consideration of the discrepancies between the standard current versions of realism and conventionalism and the positions that contextualist analyses reveal to have been advocated by Duhem and Einstein may uncover some of the tacit assumptions that impede the resolution or advancement of our disputes.

It is some fifteen years since I read both Duhem and Einstein seriously, the latter with particular attention to his arguments regarding the conventional character of spatio-temporal metrics. Since then, my professional preoccupations have been directed elswhere. The texts of the masters are, therefore, not freshly in my mind. These comments, accordingly, center on the methodological stance that Howard and Maiocchi recommend to us when we are doing history of philosophy rather than the interpretation of the particular texts they discuss. I shall point out some of the virtues of their historiographical styles and provide some extensions and corroborations of the general approach to the texts that they both support.

To begin, let me characterize the similarities in the historical methodologies manifested in the papers of Maiocchi and Howard. They both maintain that the proper understanding of philosophical texts and controversies requires a rather full understanding of the intellectual situation and cultural setting of the protagonists in question, most especially of the specific content and character of the positions which they inherited, debated, and/or opposed. Thus the position of Duhem is not that of Quine, and it will not be properly understood through the eyes of those of us who are familiar with Quine but not with Deville, Rankine, Mach, Poincaré, and Le Roy. Similarly, although Einstein's response to

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turn-of-the-century positivism and the neo-Kantianism of the Marburg School blends components of what we would call conventionalism and what we would call realism, precisely because it was directed to specific issues raised in that setting, the resultant doctrine cannot be mapped onto any current version of conventionalism or realism.

Howard and Maiocchi, in short, insist on a sort of historicity that is seldom found in standard histories of philosophy and relatively infrequent even in specialized treatments by historians of philosophy of the major figures in the grand tradition. Their attention to the historical setting greatly enriches our understanding of Duhem, of Einstein, and of the devious pathways by means of which the former's influence spread, particularly into corners of the German-speaking world during a period when many have thought it relatively uninfluential. Both papers produce some surprising findings – e.g., about the special importance of theoretical coherence in the thought of Duhem and about Duhem's influence on Einstein. These findings rest on close attention to the scientific and the philosophical preoccupations of Duhem and Einstein. They demonstrate the value of the contextualist historiographical approach.

Let us explore some advantages of the historiographical stance I am attributing to Howard and Maiocchi. It seems obvious, and hardly needs to be said in the context of this symposium, that it offers better hope than the usual methods of historians of philosophy for understanding the work of our philosophical ancestors. I shall argue that, in addition, it can sometimes shed useful, if indirect, light on current disputes when the usual methods are not much help. But I shall also complicate matters a bit by trying to refine the historiography of our speakers a bit, going slightly beyond what can be safely justified by a literal reading of their texts.

For these purposes, it is expedient to articulate the historical methodology in question more clearly than I have as yet. To a first approximation, it distinguishes at least two ways in which one might interpret a figure whose philosophical views are of interest. The first is very natural to us, living as we do in an ahistorical culture, especially if, like many philosophers and contemporary historians of philosophy, we believe in some form of perennial philosophy. From such a starting point, it is all too easy to trust our own formulations of a standard philosophical problem or position and ask of a great (or not so great) figure's texts what light they shed on the issues as thus conceived. This is what I call the standard method of history of philosophy. It is by such a route that we often come to quarrel about, for example, how best to understand Einstein's realism or conventionalism, or that we come to see Duhem's holism as so allied to Quine's that their two rather different problematics are melted together under the misleading label of 'the Quine–Duhem problem'.

The standard approach mines the work of the ages for positions and for insights bearing on *our* problems – surely a useful and constructive task, but surely also a way to fall prey to easy misunderstandings of the positions of our predecessors and the issues that preoccupied them. Real history, according to our speakers (and they are surely right) is far more interesting and far more revealing philosophically – though it often speaks less directly than we might wish to the philosophical difficulties that tie us and our contemporaries in knots.

The second mode of doing the history of philosophy, in contrast, looks to the local context in which a thinker was working – i.e., to the positions taken by those with whom he or she was engaged, the distinctions employed at the time, the specifics of the disputes in which the thinker was embroiled. It is this context that gives slippery technical terms and concepts their proper meaning. Knowledge of this context helps us to understand what has gone wrong when, employing current labels in their current acceptation, the thought of the individuals being studied seems, at least occasionally, to run skew or even counter to our expectations. Thus it is that, if we employ such terminology with current meanings, Duhem and Einstein are both misdescribed as 'realists' or 'conventionalists'. The subtle shifts since their day in the content of the doctrines and the meanings of the labels involved account for the fact that it was possible, perhaps even easy, for them to be *both* realists and conventionalists, a constellation that is very difficult, if not conceptually incoherent, today. If Duhem and Einstein combined realism and conventionalism in their philosophies, and our speakers are very persuasive in arguing that they did, the likelihood of arriving at a satisfactory understanding of their positions by means of the standard method of the history of philosophy is very small indeed. What is needed in order to properly grasp the philosophies here investigated is a sound understanding of what Duhem and Einstein opposed, of the issues they had to solve in developing their views. And those issues are not the issues or our day.

Lest you think I am misascribing a foreign method to our speakers, let me quote from a related paper by Professor Howard on 'Einstein's Conventionalism'.

> We cannot do justice to the philosophical opinions of a thinker like Einstein if [we] go to him looking for answers to our questions. What is required, instead, is a genuinely historical approach, that takes account of all available resources, and that subjects those sources to the kind of critical scrutiny practised in other areas in the history of ideas. (Howard 1987, p. 44)

It is obvious that Professor Maiocchi, who insists that a proper understanding of Duhem rests on setting his views into the context of his disagreements with Mach, Poincaré, Le Roy, D'Adhemar, Milhaud, and others, shares this stance with Professor Howard.

The time has come to complicate matters a bit. Both Duhem and Einstein were engaged in scientific as well as philosophical controversies. While it would be a mistake to draw a hard-and-fast line between science and philosophy – indeed, contextualism requires that we recognize that any such line changes with time and place – it is important to recognize that the philosophical positions that our protagonists took up were colored at least as much by their scientific as by their philosophical concerns. Professor Howard claims, in the reading version of his paper, that the case of Einstein shows that "the philosophy of science is essential to good science, but only if it places the problems of the scientist in the forefront, not if it attempts to impose its own agenda, not if it pretends to instruct the scientist". It seems clear that this portrays Einstein's own view and practice correctly, for reasons that Howard develops in that paper. Like Howard, I would like to believe that this claim is true of philosophy of science quite generally. But it is not clear how widely such a position has been held by those who have written what we would count as the philosophy of science from the nineteenth century on, and it is not clear whether a parallel claim about Duhem will withstand serious scrutiny.

Einstein's conventionalism was won, in part, by hard wrestling with the interpretation of coordinates assigned to empty space, i.e., to places occupied by no objects and in which no light rays or objects traversing geodesic paths actually interested – and it solved the problem of interpreting the seemingly conflicting curvatures of space that result from different coordinatizations of such empty regions. Duhem's conventionalism (reinforced by Maiocchi, if I understand him rightly), played a less internal role in his science. Rather, it provides external philosophical arguments opposing the initially discredited, but later ascendant, atomism against which he sought to secure his own brand of nonpositivist energeticism. To quote Duhem's *Titres et Travaux*, "it would be irrational to work towards the progress or physical theory [in light of the difficulty of directing experimental refutations against isolated theoretical claims] if that theory were not the increasingly clear and precise reflection of a metaphysics. The belief in an order transcending physics is the sole reason for the existence of physical theory" (Duhem 1917, p. 156, as translated in this volume). Thus metaphysics served Duhem as an external constraint on the proper outcome of scientific reasoning as, I believe, it did not for Einstein.

Whether this interpretation can be sustained in detail or not, it illustrates the point that as long as there is a useful working distinction in the relevant context between science and philosophy, it is necessary for contextualist historians of the philosophy of science to work out the interplay of the scientific and the philosophical influences on the positions in which they are interested – for the aims and the content of an individual's philosophy will, at least sometimes, be crucially affected by whether it is directed in the first instance to a scientific or to a philosophical question. Indeed, in at least some cases (perhaps, for one, in Einstein's) there will be nothing like a coherent closed philosophy to be uncovered precisely because philosophical considerations were pursued only as far as was needed to deal with the scientific problems in hand, without great concern for the coherence of the resulting philosophical fragments. There need not be anything wrong with such an eclectic use of philosophy in spite of the fact that it often results in a misuse of philosophy as a rhetorical club employed in special pleading in favor of whichever theory it is that one prefers. In any case, the fact that such eclectic uses and misuses of philosophy are quite common makes the contextualist's task of reconstructing philosophical views of many particular figures extremely difficult.

A particularly interesting issue posed by Duhem's and Einstein's philosophies of science, especially as they were presented by our symposiasts, is what to make of the notion of a natural classification, of the notion that even though "two different peoples" would come up with quite different descriptions of the events underlying the surface of some domain of phenomena, nonetheless for the working scientist "the world of perceptions determines the theoretical system unambiguously" (Einstein's Festrede for Planck, Einstein 1918, p. 31, as quoted by Howard). The problem in question is quite general in the sciences and by no means restricted to physics. Thus in my own work in history and philosophy of biology, I encounter parallel issues and intuitions regarding the conflicts between Darwinian and anti-Darwinian interpretations of evolution around the turn of the century and regarding instrumentalistic versus realistic interpretations of Mendelian genetics from 1900 clear through to the 1950s. What is of particular interest is the importance of coherence of theories, which Maiocchi describes as a central Duhemian obsession, crucial to his argument against the English-style use of models and to his argument that physics sought and might reasonably expect to approach something like a natural classification.

To illustrate the point that parallel considerations play a crucial role in the evaluation of work in other sciences than those with which Duhem and Einstein were concerned, consider the gulf between embryology and genetics from the twenties through (at least) the fifties of this century: all higher organisms have the same genes in virtually all of their cells. Yet some system of hereditary controls causes the cells to differentiate systematically (in the right places and at the right times) into nerve, muscle, bone, liver, and kidney cells. Mendelian models could not explain how the same cells could yield such different results and embryological models and descriptions could not provide a serious ous account of an extra-Mendelian system of heredity. Within fairly circumscribed limits, both Mendelian genetics and descriptive/experimental embryology were in pretty good shape. But as soon as one posed Duhemian questions about theoretical coherence, about the natural classification for the hereditary controls governing what an embryo would become, the situation looked unsatisfactory indeed. As it happens, this complex of issues was taken particularly seriously in France, where the debate over the status of Mendelism on these grounds was particularly lively (Burian, Gayon, and Zallen, 1988). I have no idea at this point whether there was any indirect influence of Duhem on these debates, but it surely is a matter worth further exploration.

The complications that I have introduced can be summarized rather neatly. There are at least four perspectives which a contextualist historian of philosophy of science may employ in working out the views of a particular figure. These concern

1. the philosophical setting and disputes in which she or he was engaged,
2. the scientific issues to which the philosophical considerations were primarily addressed and the interpretation of those considerations

within the scientific as well as the philosophical context of the day,

3. the application of those philosophical considerations in novel settings, scientific as well as philosophical, including the pathways by which they became influential, and
4. the application of those philosophical considerations in later philosophical contexts.

Let us turn, to close these comments, to the last perspective on my list. One particularly valuable philosophical use of the products of contextualist studies of first-rank figures is to be found here. If Howard and Maiocchi are right, both Duhem and Einstein developed closely related versions of conventionalism and realism, and in both of their philosophies the conventionalist and realist moments were mutually compatible. If I am right, current versions of conventionalism and realism are mutually incompatible. Most contemporary realisms have been saddled with one or both of two commitments foreign to Duhem and Einstein. These concern the need for science to converge on the one true theory or, perhaps, the one true account of the phenomena in a particular domain, and the need to be able to provide a literal semantics across the board for the individual terms and concepts of a true theory. Duhem's holism and, arguably, Einstein's as well, denies the need for across-the-board literalism, and both of them had important reasons for denying that convergence was required or even desirable for realism. Thus, for those who are caught up in the current disputes, serious consideration of the discrepancies between the standard current versions of realism and conventionalism and the positions that contextualist analyses reveal to have been advocated by Duhem and Einstein may uncover some of the tacit assumptions that impede the resolution or advancement of our disputes.

Contextual studies of the sort that Maiocchi and Howard have executed here may not provide an Archimedean fulcrum for resolving philosophical disputes, but they certainly offer a rich panoply of alternatives. In so doing, they provide us not only with vastly improved understanding of our philosophical predecessors, but also with significant leverage for making progress in our own disputes as well. For these reasons, we should be grateful to them and, indeed, to many of the other contributors to this conference.

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PIERRE DUHEM'S CONCEPTION OF NATURAL CLASSIFICATION*

ABSTRACT. Duhem's discussion of physical theories as natural classifications is neither antithetical nor incidental to the main thrust of his philosophy of science. Contrary to what is often supposed, Duhem does not argue that theories are better thought of as economically organizing empirical laws than as providing information concerning the nature of the world. What he is primarily concerned with is the character and justification of the scientific method, not the logical status of theoretical entities. The crucial point to notice is that he took the principle of the autonomy of physics to be of paramount importance and he developed the conception of natural classification in opposition to accounts of physical theories that contravened it.

Pierre Duhem's view that physics aims to establish a 'natural classification' of phenomena is generally treated as something of an embarrassment, so much so in fact that it is frequently dismissed as an aberration or passed over in silence. Taking his official view to have been that the sole purpose of theorizing in physics is to facilitate discussion, commentators have tended to think that he must have regarded theories as 'artificial classifications'. Duhem could not, they suppose, reasonably have taken the theoretical physicist's aim to be both one of summarizing empirical laws in a compendious fashion and one of providing insight into the realities behind the appearances. Indeed some commentators have gone so far as to argue that he introduced the idea of natural classification because he could not bring himself to deny what physicists instinctively believe and some have even argued that he meant something quite different by the idea from what he seems to have meant.[1]

This line of argument is tempting if only because Duhem devoted considerable effort to arguing that theories should be regarded as economical classifications. However, it also labours under the difficulty that Duhem frequently stresses that physical theories provide information about the nature of the world and it strains the imagination to suppose that he did not appreciate the difference between artificial and natural classification. While Duhem certainly thought that theories summarize empirical laws, this did not prevent him from arguing throughout his career for the view that they are converging on natural

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classifications and he seems never to have doubted the consistency of his position.[2] In fact we would seem far better advised to take Duhem's remarks about physics converging on a natural classification at face value and to attempt to figure out how they can be reconciled with the rest of his philosophy.

Undoubtedly much of what Duhem says in *La Théorie physique* needs careful interpretation, but what he says about natural classification seems clear enough. Consider for instance his view that "physical theory is not merely an artificial system, suitable today and useless tomorrow, but . . . an increasingly more natural classification".[3] Better still, consider his explicit contention that "the aim of physical theory is to become a natural classification, to establish among diverse experimental laws a logical coordination serving as a sort of image and reflection of the true order according to which the realities escaping us are organized".[4] In these and similar remarks Duhem distinguishes natural classifications for artificial ones, rejects the view that physical theorizing is restricted to the logical classification of experimental laws, and suggests that the classifications that physicists provide are becoming increasingly natural.

In fact Duhem's picture of science is the familiar one of a self-contained evolutionary enterprise in which less good theoretical classifications are replaced by better ones. In his view physicists make progress by replacing classifications that are partly 'representative' and partly 'explanatory' with ones that are more 'representative' and less 'explanatory'.[5] More specifically, he holds that clashes between theory and experiment result in "the purely representational part" of the theory (i.e., the part obtained using the methods of theoretical physics) being taken up "nearly whole" by the new theory and "the explanatory part" (i.e., the part not so obtained) giving way to "another explanation". We are to think of each theory as passing on to its successor "by virtue of a continuous tradition . . . a share of the natural classification that it was able to construct".

True, Duhem takes consistency, unity and agreement with experimental laws to be the only 'logical conditions' on physical theories.[6] He does not however take these conditions to be the only ones that theories should satisfy, still less regard theories that satisfy them as equally acceptable. In his view 'logically acceptable' theories are all too easy to come by and it is essential that new theories also be transformations of those already in place. To be acceptable a theory must, he argues, be "the slow and progressive result of an evolution"; it must not be "the sudden product of a creation".[7] In other words we should prefer theories that naturally extend flourishing traditions to ones involving new concepts all other things being equal. It is only when a tradition falls short on experimental or logical grounds that it is appropriate to contemplate the introduction of new theoretical principles.

Here it is important to keep in mind that Duhem's view of theories as converging on natural classifications is closely allied with his insistence on the autonomy of physics and its historical continuity. Given his conception of physics as an essentially autonomous enterprise, it is entirely unsurprising that he should appeal to the history of physics (rather than something external to it) to supplement the logical constraints on classifications. And given his commitment to the principle of historical continuity, there is nothing particularly remarkable about his rejection of the view that physical theories are artificial classifications in favour of the view that they are becoming increasingly natural. One can well imagine him thinking that physical theories can be reasonably regarded as natural classifications just to the extent that they are obtained by methods that are strictly autonomous and are appropriately continuous with what went before.[8]

It might be thought that such an interpretation runs foul of Duhem's conception of 'explanation' as radically at variance with 'logical classification' of his criticism of arguments to the effect that physical theories 'explain' experimentally established laws. But this is far less clear than it might appear. For what Duhem's attack on explanation is actually directed against is not explanation as such but 'metaphysical explanation'. Even in the first chapter of *La Théorie physique*, which is often taken to clinch the issue, Duhem does not deny that theories tell us something about the world. Here his main point is the negative one that it is neither desirable nor possible to derive physics from metaphysics. In the terminology of a later chapter, the butt of his criticism is the view that physics provides "definitive explanations"; he was not against thinking of physics as directed toward the discovery of "provisional representations".[9]

Far from wishing to show that theories should be thought of as artificial classifications, Duhem was mainly concerned to combat attempts on the part of some of his contemporaries to subordinate physics to metaphysics. What he rejected was not the usual conception of physical theorizing as culminating in 'nonmetaphysical' explanations but rather the view of it as resting in one way or another on metaphysical assumptions, as subject to *a priori* restrictions. The main point that he wanted to emphasize was that the methods of physics are our sole means of investigating the world, that they are all that we can reasonably rely on, that they alone provide us with 'representations'. It is, he tells us, central to his conception of physical science that it "proceeds by an autonomous method absolutely independent of any metaphysical opinion".[10]

Similarly it is a mistake to trace Duhem's hostility to atomism and mechanism to his rejection of the conception of physical theories as explanations. The reason that he regarded Energetism as deserving special consideration was not that it happened to have a particular form but that he was of the opinion that "the means of knowledge available to Physics justifies the course it takes", his view being that physicists were – "the gossip of the moment" notwithstanding – in the process of securing the "complete realization of [the] ideal [of an abstract theory]".[11] For him the problem with scientific inquiry based on atomistic and mechanistic assumptions was that it violated the requirements of the autonomy and continuity of physics. He did not think that atomism and mechanism could not possibly be made good, only that they have not been (and that there was a better alternative available).[12]

Undoubtedly part of the explanation of why commentators misunderstand Duhem's position is that they fail to appreciate that he held his primary task to be one of delineating how the scientific method is deployed in practice. It is a mistake to assume – as is usually done – that he aimed to show that the methods of theoretical science are less robust than normally thought or that he believed that progress in physics occurs exclusively at the observational level or that thought of physical truth and theological truth as having to do with different 'ontological orders'. As he himself put it at the beginning of *La Théorie physique* his object was to provide "a simple logical analysis of the method by which physical science makes progress".[13] The question of the theoretical progress of physics was never an issue for him; even at his most philosophical he remained true to his scientific and historical convictions.

To appreciate Duhem's stance it is helpful to remember that he took the positivism of his day (its important merits notwithstanding) to be no less flawed than the metaphysical approaches against which it was then being pitted. He agreed with Ernst Mach and like-minded thinkers concerning the autonomy of physics and he took attempts to ground physics in metaphysics (including contemporary neo-scholastic attempts to integrate it into a general theological cosmology) to be subject to positivist criticism. Yet he was also convinced that the positivists were wrong to relegate the aim of physical theorizing to the development of an economical summary or artificial classification of empirical laws. Indeed he can perhaps best be regarded as attempting to appropriate the positivist's criticisms without embracing positivism itself.

Duhem virtually states as much when he observes that "the history of science alone can keep the physicist from the mad ambitions of dogmatism as well as the despair of Pyrrhonian skepticism".[14] A sensitivity to history and the method by which physical science progresses is, he insists, an effective antidote both to dogmatic claims about the subservience of physical theory to metaphysical speculation and to skeptical counterclaims to the effect that such theory tells us nothing whatsoever about the world. In his view a study of the history of physics highlights both the "vicissitudes of cosmological schools" and the inadequacy of picturing "physical theory [as] an artificial system [rather than as] an increasingly clearer reflection of realities". Whenever physicists are "on the point of going to [either] extreme", such a study can, he avers, be relied on to provide the 'appropriate correction'.[15]

What is less clear – and with this we come to the nub of the issue – is how Duhem can hold that physical theories gradually approach natural classifications. It would seem that all that he should believe in this regard is that theories are artificial classifications, this being all that is warranted by considerations of logic and history. To Duhem's way of thinking however such a conclusion would be premature. For while he certainly believes that nothing stronger can be justified on the basis of the logical and historical arguments appropriate to physical inquiry, he is also of the view that there are additional nonlogical, nonhistorical arguments that need to be considered. As he states the matter, "no scientific method carries in itself its full and entire justification [and] we should . . . not be astonished that theoretic physics rests on postulates which can be authorized only by reasons foreign to physics".[16]

Once again Duhem accepts some of the positivists' principles and rejects others. He agrees with them concerning the character of the methods of physics and the particular theories that can be obtained using these methods. But he disagrees concerning the nature of the theories and what these tell us about the world. In particular he takes the positivists to err in supposing that scientific justification is the only sort of justification that counts. The fact that physics is an autonomous enterprise in which an attempt is made to devise economical classifications of phenomena is, he insists, no reason to believe that all that a proper application of its methods can yield are economical classifications. As he sees the matter, physics is capable of establishing far more than it itself can establish that it has established.[17]

In support of this central point Duhem argues that it is taken for granted – albeit surreptitiously – even by positivists. One cannot, he argues, accept the positivist's assumption that physical theorizing results in unified classifications without admitting that such theorizing goes well beyond what can be justified in its own terms, hodgepodges of empirical laws being perfectly compatible with pure logic and economy of thought. Indeed Duhem explicitly states that "neither the principle of contradiction nor the law of economy of thought permits us to prove in an irrefutable manner that a physical theory should be logically coordinated".[18] If one takes physics to be a well-founded enterprise one must, he concludes, admit the existence of another "source [from which] an argument [can be drawn] in support of this opinion".

To avoid misunderstanding I should stress that I am not arguing that Duhem took the positivists to be right about physics and wrong about what lies beyond it. He did not hold that physicists stray beyond the confines of physics strictly understood when they assert that their theories are becoming increasingly natural, nor did he believe that they are – in their capacity as physicists – obliged to think of their theories as artificial classifications. To the contrary Duhem held that physicists are even in this capacity entirely justified in taking their aim to be the establishment of natural classifications and their theories to be even now natural (at least to a certain extent). In his view what lies beyond physics are only the arguments that warrant physicists believing what they happen to believe.[19]

Duhem himself devotes considerable effort to spelling out arguments for these beliefs; he does not simply state that arguments are required. Thus he points out that "it is impossible for us to believe that [the order and organization revealed by our theories] are not the reflected image of a real order and organization" and he argues in some detail that physicists are best thought of as "yielding to an intuition which Pascal would have recognized as one of those reasons of the heart "that reason does not know".[20] In his view we are entirely justified in believing that theories tell us something about the world since it is entirely natural to do so. We should not be misled by the fact that such belief requires an intuitive judgement involving a 'reason of the heart'; this does not mean that it is unimportant or unreliable.

In addition Duhem would have us believe that physicists are justified in thinking of theories as natural classifications to the extent that they are able to use them to obtain novel predictions. The possibility of obtaining information about new situations would in his view be exceedingly remote were physical theories not at least roughly in accord with how the world actually is. If a "theory [were] a purely artificial system", its confirmation by a "hitherto unknown law" would, he stresses, "be a marvelous feat of chance" and we should be quite unwilling to "bet fearlessly in its favour".[21] Of course this is not a deductively valid argument, it being impossible to derive a conclusion having to do with the nature of theories (namely that they are natural classifications) from a premise having to do with how theories have actually been used (namely that the best of them successfully anticipate the future). But for Duhem it is none the worse for that; he has no objection to this type of nondeductive ('transcendental') argument.[22]

In this connection it is helpful to recall that Duhem takes theoretical physics and "spiritualistic metaphysics" to be both "radically heterogeneous" and "approach[ing] each other in their perfect form".[23] It is, he reminds us, one thing to deny that metaphysical conclusions can be derived solely from physical premises, quite another to hold that they cannot be derived from physical premises in conjunction with other reasonable (nonphysical) assumptions. What Duhem wishes to stress is not that theoretical physics has no bearing on theological cosmology, only that its bearing is never direct. One can, he insists, see that such heterogeneous viewpoints are (or are not) analogous given "reasons foreign to physics"; what is totally divorced from metaphysics is physical theory "in itself and by its essence".[24]

Thus I would dispute the widely-held view that Duhem contradicted himself when he took general thermodynamics to approximate a natural classification analogous to Aristotelian cosmology. In arguing this way he was not compromising the principle of the autonomy of physics nor was he going back on his views about the separability of physical findings from metaphysics. While general thermodynamics "in itself and by its essence" neither accords nor conflicts with Artistotelian cosmology, we have every reason to hold that the two views are in substantial agreement given the direction in which physics is tending and certain extrascientific considerations. "Aristotle's cosmology reduced to its essential affirmations" is indeed plausibly taken to be analogous to "the teachings of thermodynamics" and it is not difficult to appreciate Duhem's insistence that this is "all the more striking" for not having been planned in advance.[25]

Be this as it may, it should be clear that Duhem stands foursquare against instrumentalistic conceptions of physical theories.[26] He meant what he said when he spoke of theories as "permit[ting] hints as to the real affinities of things" and stated that natural classification serves as "a sort of image and reflection of the true order according to which the realities escaping us are organized".[27] For him it would have been quite implausible to maintain that physical theories tell us nothing except that certain experimental laws are logically coordinated. While physics is (and always will remain) incomplete, we are well within our rights to think of theories associated with traditions tending in definite directions as reliable indicators of the realities behind the appearances.

In particular I should emphasize that it is not only those who regard Duhem's philosophy as instrumentalist through and through who are at fault. If the present interpretation is correct, it is just as wrong to describe his view as 'mitigated' or 'qualified' instrumentalism or to classify it as 'semantic realism' or to think of it as a species of 'commonsense realism'. Duhem was not attempting to straddle the fence, nor did he merely believe that physical theories should be regarded as true or false, nor was he any less committed to the reality of scientific objects than to the reality of everyday ones. Quite the reverse. If anything he espoused a version of what is nowadays called convergent realism. As we have seen he held that physics – left to its own devices – yields information about the nature of the world and that we are entirely justified in believing that its ontological claims are for the most part close to the truth.[28]

Admittedly it is not difficult to cobble together quotations from Duhem's writing that make him sound as though he was uncompromisingly opposed to realism of any kind.[29] It is also true, however, that we omit an important part of his story when we read him this way. His general strategy is to argue first that theories are classifications (as opposed to metaphysical explanations) and then to provide considerations for the view that they are more or less natural (rather than artificial). Far from regarding physics as aiming at logical classification, he takes this view of the matter to be the unsatisfactory outcome of an overly narrow conception of what physics can achieve. For him "physical theory confers on us a certain knowledge of the external world which is irreducible to merely empirical knowledge" and there is no avoiding the fact that a purely instrumentalistic physics would be of "meager importance".[30]

NOTES

* I have benefited from discussions with Howard Duncan concerning Duhem's philosophy, and from Roger Ariew's and Michel Stack's criticisms of an earlier version of the paper. Also I would like to thank Ernan McMullin for his comments at the Blacksburg Conference.

[1] Compare L. de Broglie's suggestion that Duhem "mitigate[d] the rigor" of his view because he felt that physical theory must be accorded "a deeper bearing than that of mere methodical classification of facts already known" (1962, p. x) and R. Poirier's claim that "[Duhem's] expression 'natural classification' is roughly equivalent to 'language'" (1967, p. 403).

[2] Duhem invokes the conception of natural classification not only in *La théorie physique* of 1906 but also in 'L'école anglaise et les théories physiques' of 1893 and in 'Notice sur les titres et travaux scientifiques' of 1913.

[3] Duhem (1914/1962), p. 270.

[4] Duhem (1914/1962), p. 31. Also compare Duhem's view that "we can and we must . . . attempt to make [our] classifications as little artificial and as natural as possible" (1893b/1987, p. 137) and his claim that "physical theory may attain a certain knowledge of the nature of things . . . [as] the goal of [its] progress, the limit it constantly approaches without ever reaching" (1917/1987, p. 338).

[5] Duhem (1914/1962), p. 32; see also pp. 204–5 and p. 221.

[6] Duhem (1914/1962), p. 220.

[7] Duhem (1914/1962), p. 221; see also p. 295. A good example is Duhem's criticism of Maxwell's introduction of the notion of displacement current (see pp. 78–79 and Ariew and Barker 1986, pp. 140–50). Also it should be remembered that Duhem argues that "the physicist does not choose the hypothesis on which he will base a theory" but rather draws on "the thoughts habitual with those among he lives and tendencies impressed on his own mind by his previous studies" (1914/1962, pp. 255–56).

[8] The importance of the autonomy of physics for Duhem can hardly be overstated, it being one of his "constant" concerns (1914/1962, p. 274). Compare Martin (1976), p. 127, where Duhem is said to have regarded the autonomy of physics as "an essential regulative principle". In emphasizing this point I do not of course mean to belittle the importance of Duhem's critique of the "Newtonian method of induction from observation" (see also Ariew 1984, pp. 319–20).

[9] Duhem (1914/1962), p. 270; see also Duhem (1893a/1987). In addition note that the title of the first chapter of the *La Théorie physique* is "Physical Theory and Metaphysical Explanation" and that explanation and classification are introduced as the two main answers that "logicians" have given to the question of the aim of physical theory (p. 7). Moreover Duhem is not in the least averse to speaking of other possible aims (see, e.g.,

p. 81). It is, I suggest, wrong to suppose that his discussion rests on a "dogmatic and unsupported presupposition about the nature of explanation" (compare Alexander 1967, p. 423).

[10] Duhem (1914/1962), p. 274. Contrast Karl Popper's suggestion (1969, p. 104) that Duhem "seems to think that . . . there are essences but they are undiscoverable by human science (although we may, somehow, move towards them)". As I read Duhem, his view was not that "essences . . . are undiscoverable by human science" but that they are only discoverable this way. Incidentally there are many striking parallels between Popper's own philosophy and Duhem's (see e.g., Duhem (1914/1962), pp. 23, 27, 53, 177, and 277).

[11] Duhem (1917/1987), p. 334 and Duhem (1914/1962), p. 304.

[12] Significantly Duhem argues that atomism and Cartesianism are plagued by faulty reasoning and at variance with important experimental laws; he does not challenge them on philosophical grounds (compare his (1914/1962), pp. 80–86, 280, and 304). While Duhem agrees with Hertz that "Maxwell's theory is the system of Maxwell's equations" (p. 80), he no more takes the theory to be an artificial classification than does Hertz, his main point being that it should not be encumbered with mechanical models. Also compare Duhem's criticism of the atomist and the Cartesian for placing "hypothetical knowledge of the nature of things at the starting point of physical theory" (1917/1987, p. 338).

[13] Duhem (1914/1962), p. 3. Also note that Duhem believed that "to give the history of a physical principal is at the same time to make a logical analysis of it" (p. 269).

[14] Duhem (1914/1962), p. 270.

[15] Also compare Duhem's rejection of the complaint that his view "opens the door to skepticism" and "makes a concession to positivism" (1893a/1987, p. 97). In this paper I do not consider Duhem's historical work but it is worth noting that here too one of his major themes is that physical theories are neither subordinate to metaphysics nor merely 'artificial'.

[16] Duhem (1914/1962), p. 293; see also p. 298. In arguing this point I part company with R. N. D. Martin, who holds that Duhem was concerned with the possibility of logically justifying the requirement that physical theories be "logical and coherent" (see his 1987, p. 306). As I read Duhem his point was that certain scientific procedures cannot be established given logic (and history); he did not think of logic (or history) as being in need of justification.

[17] See also Duhem (1893a/1987), p. 99.

[18] Duhem (1914/1962), p. 102; see also pp. 293–4 and 334, and Duhem (1893b/1987), p. 134. The same point can be made about the common positivist demand – defended, e.g., by Mach – that later theories connect up with earlier ones.

[19] If this is right, Duhem did not hold that "the scientist qua scientist must work with theories as if they are only instruments [i.e. mere classifications]" (Joy 1975, p. 197). In the terminology of Bas van Fraassen (see his 1980) Duhem maintained that physicists are justified in believing their theories as well as in accepting them as empirically adequate. As Michael Stack has pointed out to me there is an interesting analogy between physical theories (as I take Duhem to conceive them) and perceptual beliefs in that both provide information the reliability of which can be vouchsafed only by invoking 'external considerations'.

[20] Duhem (1914/1962), pp. 26 and 27. Significantly Duhem also speaks of the physicists's concern with unity as being "a legitimate one because it results from an innate feeling"

(p. 102). "The aspiration towards a theory whose parts all agree logically with one another is", he insists, "the inseparable companion of [the] aspiration . . . towards a theory which is a natural classification of physical laws" (pp. 103–4).

21 Duhem (1914/1962), pp. 28; see also pp. 195 and 297. In the same context Duhem states that "the wonderful order [that] classification . . . brings about in the tremendous arsenal of chemistry already assures us that the classification is not a purely artificial system" (pp. 28–29; see also p. 300).

22 Thus I reject N. Cartwright's contention (1982, p. 112) that Duhem is antipathetic to "theoretical laws because he does not countenance inference to the best explanation". On my reading, Duhem espoused a version of what has come to be called "the miracle argument" and he was opposed neither to theoretical laws nor to inference to the best explanation.

23 Duhem (1914/1962), pp. 301 and 299.

24 Duhem (1914/1962), p. 285. It is irrelevant that there are normally disanalogies as well as analogies between physics and metaphysics (p. 303). The crucial point is that we can on occasion legitimately step beyond physics and "recognize in [physics and metaphysics] two pictures of the same ontological order, distinct [only] because they are each taken from a different point of view" (p. 310). When Duhem speaks of his view as being positivist in both "its origins" (p. 275) and "its conclusions" (p. 279), what he means is that he developed his ideas without appealing to metaphysics and without having had any specific conclusion in mind.

25 Duhem (1914/1962), pp. 310 and 307. I might note in passing that Duhem's discussion of refutation rests on considerations similar to those just outlined. Duhem's basic idea is that it often makes "good sense" to reject a theory rather than an auxiliary assumption and that while decisions based on good sense cannot be justified scientifically they are nonetheless perfectly reasonable (see p. 217).

26 Compare, e.g., Alexander (1967), p. 425, Popper (1959), p. 78, and van Fraassen (1980), p. 86.

27 Duhem (1914/1962), pp. 30 and 31. It should not be forgotten that Duhem took his preference for Energetism over atomism to have important consequences for our understanding of the nature of the world and that he recognized the existence of "microscopic nuclei" (see p. 221).

28 For the views criticized in this paragraph see Joy (1975), p. 199 and Martin (1987), p. 309, Giedymin (1976), p. 184, and Jaki (1984), p. 320.

29 Compare Duhem (1914/1962), pp. 8, 19, 21, 115, 124, 144, and 180.

30 Duhem (1914/1962), p. 334.

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COMMENT: DUHEM'S MIDDLE WAY

ABSTRACT. Duhem attempted to find a middle way between two positions he regarded as extremes, the conventionalism of Poincaré and the scientific realism of the majority of his scientific colleagues. He argued that conventionalism exaggerated the arbitrariness of scientific formulations, but that belief in atoms and electrons erred in the opposite direction by attributing too much logical force to explanatory theories. The instrumentalist sympathies so apparent in Duhem's writings on the history of astronomy are only partially counterbalanced by his view that science is progressing toward a 'natural classification' of the world.

In Duhem's writings about the nature of science, there is an ambivalence that even the most casual reader can scarcely miss. His account, in consequence, does not fit into the usual categories of the philosopher of science. He was, it seems, quite consciously trying to thread a middle way between two positions he regarded as extremes. One was what would today be called scientific realism, in the most usual sense of that much-distinguished phrase, that is, the view that the explanatory success of a scientific theory gives one valid (even though rarely conclusive) reason to believe in the existence of the underlying entities postulated by the theory. Duhem strongly rejected what has come, by a clumsy phrase, to be called 'inference to best explanation', holding on both historical and logical grounds that the explanatory power of a structural theory cannot serve as a testimony of its truth.

On the other hand, he was equally unhappy with the conventionalism of Poincaré and the inductivism he found in the physics textbooks of his day, because he thought that they unduly limited the truth-claims of science either by exaggerating the arbitrariness of the scientific formulations, as in the case of conventionalism, or by undervaluing the symbolic character of physical theory and the holistic character of its associated warrant, as he held inductivism to do. His distinctive notion of natural classification expresses his attempt to separate himself from the skepticism he saw as inherent in the two dominant fashions of the day in French philosophy of science, without at the same time embracing the model-realism he liked to associate with the 'broad but weak' English mind.

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By intention at least, then, he was neither a realist nor a skeptic about physical theory, in the most usual senses of those two elastic terms. In a move reminiscent of Arthur Fine's recent attempt to find a middle way between realism and instrumentalism, he proposed a definition of physical theory that allowed him conclude that:

What is lasting and fruitful [in physical doctrines] is the logical work through which they have succeeded in classifying naturally a great number of laws by deducing them from a few principles; what is perishable and sterile is the labor undertaken to explain these principles in order to attach them to assumptions concerning the realities hiding underneath sensible appearances. (1914, p. 38)

The 'natural ontological attitude' he advocated, to appropriate a phrase coined by Fine, is to suppose that even though physical theory is incapable of itself of discovering entities that do not belong among the sensible appearances:

The more complete it becomes, the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things, and the more we feel that theory tends to be a natural classification. (1914, pp. 26–27)

Notice the language Duhem uses when he describes how we come to this belief: "the more we apprehend . . . the more we suspect . . . the more we feel . . .". Even though the convergence of physical theory on the relational structure of the world cannot be demonstrated by the methods of physical science itself, it is the natural attitude for scientists to adopt, and is supported by philosophic consideration of the history of specific theories.

Duhem walked a tightrope, to be sure. On the one hand, he found himself in disagreement with the most illustrious French exponents of the new discipline of philosophy of science, Henri Poincaré, Eduard LeRoy, and Abel Rey, who seemed to want to reduce science to a set of practical prescriptions for action, depriving it of its status of objective knowledge, as Duhem understood that phrase. On the other hand, he was scornful of the attempts of the most distinguished physicists of the day to construct theories involving unobservable entities like molecules, atoms, and electrons. Despite the fact that such theories "would undoubtedly be regarded as prophetic forerunners of the theory destined to triumph in the future", despite the 'almost universal assent' favoring them among working scientists themselves, he urges his readers to set them aside and to learn from the study of "the errors of past centuries" to be on guard against those "efforts of the mind that wishes to imagine what ought to be merely conceived" (1905, p. 304).

But Duhem did not want to appear entirely out of step. He tried to put the best face on his disagreement with Poincaré and Mach, claiming to find a tension, amounting at times indeed to logical contradiction, in their work. Besides their usual stress on scientific law as convention or as convenient summary, he reminds us that they also on occasion allow, indeed insist, that physical theory leads to the discovery of the real relations of things with one another (1908b, pp. 327–35). He traces this tension in their thinking to the complex character of the question itself. A logical analysis of the experimental method can never of itself warrant anything more than the claim that physical theory serves an instrumental function as a predictive device. It could not, for example, forbid the simultaneous use of incompatible theories, provided they served the purposes of prediction. Working scientists could never be content with this; yet their conviction that such incoherence must be eliminated cannot be justified merely by an appeal to convention or to instrumental convenience.

It derives, he argues, from an intuition which clearly transcends the limits of science. Those who subscribe to a generally positivist ideal of knowledge face a dilemma, then. If they allow progressive unification of laws as a requirement of good science, they seem to violate the positivist canon; if they do not, they are likely to "shock most of those working for the advance of physics" (1905, p. 294). This is how Duhem excuses those whom he criticizes so gently: their heart is in the right place, he tells us. When they err, it is by understatement, it is only because they fail to realize that they do not need to take positivism quite so literally. He is not nearly so tolerant of the other extreme, of the 'atomists', as he calls them generically, those who believe that the explanatory power of theory allows us to penetrate beneath the level of sensible appearance. Their imaginative excess is more dangerous than positivist defect because of its allure, to scientists and nonscientists alike.

In his essay, 'Duhem's Conception of Natural Classification', Andrew Lugg focuses on Duhem's doctrine of natural classification in order to argue two theses, first, that despite appearances to the contrary, Duhem was not an instrumentalist, and second, that equally despite appearances to the contrary, he was a realist, advocating a version of convergent realism. I would agree with him on the first of these theses, with some reservations, but would deny the second.

The instrumentalist thrust of *To Save the Phenomena* is well known. Duhem traces the debates in astronomy between the 'mathematicians' whose aim was merely to find a formalism that would fit the phenomena and the 'physicists' who wanted to explain the planetary motions in causal terms.[1] His sympathies are clearly with the former in the period prior to Galileo, when the explanatory schemes of the 'physicists' drew their warrant mainly from metaphysical principles in regard to causal action and hardly at all from their ability to save the phenomena. The concluding sentences of the book are worth pondering:

> Despite Kepler and Galileo, we believe today, with Osiander and Bellarmine, that the hypotheses of physics are mere mathematical contrivances devised for the purpose of saving the phenomena. But thanks to Kepler and Galileo, we now require that they save all of the phenomena of the inanimate universe together. (1908a, p. 117)

Duhem leaves us in no doubt that he believes Copernicus and Galileo were wrong, in principle, to suppose that their astronomy could allow them to claim that the earth is in motion. Their contention that their hypotheses bear on 'real movements' was 'false and harmful', and Osiander, Bellarmine and Urban VIII were right in viewing it as 'contrary to logic' (p. 116). There is no suggestion here (as there is elsewhere in his work) of a 'higher logic' that could later reverse this judgment.[2] Insofar as Kepler and Galileo are given credit, it is for their unification of earth and heavens in a single mathematical scheme, a unification that Newton carried to completion. This overcame the sharp dichotomy between the instrumentalism and the realism of earlier astronomy, and was thus the first major step to a natural classification. So despite Duhem's insistence that it was the 'mathematics' and not at all the 'physics' of that earlier period that would bear fruit, he still manages to find a precarious middle way, one that leans, however, rather more in one direction than the other.[3]

Duhem criticized the overly instrumentalist tendencies he detected in the conventionalism of Poincaré and the positivism of Mach, because they fail to account for the progressive unification to which the history of science gives unequivocal testimony. Though there are discontinuities in the history of physical theory, Duhem insists that these are on the side of *explanation*; the metaphysical fashions controlling such explanation come and go, like the ebb and flow of the tide (1914, p. 39). On the side of *representation*, however: "each theory passes on to the one that follows it a share of the natural classification that it was able to construct" (pp. 32–33).

One is reminded here of Kuhn, who stresses the discontinuity that characterizes the development of explanatory theories (and is led, like Duhem, to reject scientific realism on that account) but still wants to insist on the overall progress in puzzle-solving and the cumulative character (in one sense at least) of experimental laws. Like Duhem, he insists also on the objectivity of scientific knowledge, despite his rejection of explanatory ontologies. Kuhn bases this claim on the fact that theory-appraisal is guided by values, over and above predictive accuracy, that are themselves relatively "permanent attributes" of science; again, the resemblance to Duhem's argument is striking. Kuhn would no more want to be thought an instrumentalist than did Duhem, but his defenses against the charge might be thought less secure, since he could not call on so ontological a ground as Duhem's natural classification.

Lugg notes Duhem's criticisms of the two doctrines from which the instrumentalism he questioned might derive. Yet Duhem's differences with the two seem at times more of the nature of family quarrels. He argues that Poincaré's conventionalism makes him unable to account for the part played by theoretical interpretation in the statement of experimental facts. Yet he also describes the principles of his own energetics as "pure postulates or arbitrary decrees of reason" (Duhem, 1917, p. 1), validated only by the conformity of their consequences with experimental law. The conventionalist emphasis on the arbitrariness of symbolic formulations is not without virtue in his eyes, it would seem, though it is in the end qualified, of course, by the doctrine of natural classification.

Duhem is even gentler with positivism. He opens section 2 of 'Physics of a Believer' with the words:

> We should like to prove that the system of physics which we propose is subjected in all its parts to the most rigorous requirements of positive method, and that it is positivist in its conclusions as well as its origins. (1905, p. 275)

It is true that this essay, preoccupied with showing that he did not, as his critics charged, make his physics subservient to a metaphysics, is more emphatic about the virtues of positivism than anything else he wrote. Nevertheless, his insistence on the "essentially positivistic" character of his account of physics (p. 279) was genuine. He could applaud positivism for its exclusion from science proper of metaphysics in any form. He could identify with the positivist denial of "any ability [on the part of physical theories] to penetrate beyond the teachings of experiment or any capacity to surmise realities hidden under data observable by the senses" (p. 274). By arguing that physics was both autonomous and yet in a fundamental sense incomplete, he could retain positivism in one area while flatly contradicting it in another. Because, of course, his insistence on the legitimacy of a metaphysics violated the fundamental principle of Comte in regard to positive knowledge. Duhem plays down this disagreement; the alliance with positivism was crucially important to him. Though Lugg is clearly right to maintain that in the end Duhem was in the strict sense neither a positivist nor a conventionalist, it is important to stress how strong his affinity was with both doctrines.

Lugg's second thesis, cannot, I think, be sustained. He takes Duhem to be a realist, in the sense of holding that "the furniture of the world is more or less what our latest theories pronounce it to be". Further, he takes Duhem's opposition to atomism and mechanism not to be one of epistemological principle; it is only, he thinks, a matter of the inadequacy of the evidence as yet available in their favor. The trouble with them "is not that they cannot (in principle) be made good but that they have (as a matter of fact) never been made so".

Here I find myself in strong disagreement, though I realize that the matter is not cut and dried. Duhem's objections to the use of retroduction within physics itself to infer to the existence and nature of entities that lie beneath the level of sensible appearance, are assuredly a matter of principle for him. Lugg argues that Duhem's often-repeated view that physical theory cannot penetrate beyond the sensible appearances has to be read in the light of his distinction between the physicist's viewpoint, taken narrowly, and the larger perspective afforded by intuition and philosophical argument. But all that the distinction warrants in this case is the claim about natural classification: that physical theory, considered as a set of abstract laws, mirrors the underlying relations between things more and more exactly. Does Duhem envisage that molecules, atoms, electrons, and the like may one day become part of the natural classification? Quite clearly not, it seems to me.[4] If this were to be even a possibility, his arguments against mechanism would fail, and his strictures on explanatory models would have to be discounted as referring only to their use up to the time at which he wrote. I can find no basis in his text for such a construal.

The only kind of realism that we can claim for him (and it is, of course, a crucial one for him) is that of the relationships he found in the laws of mechanics or, more generally, in what he called "energetics". It is not a realism of explanatory theory. The distinction between law and theory which is common today he did not make. For him, the explanatory aspects of the physical theories of his day, those involving unobserved entities causally responsible for the data of experiment, were excess baggage, illegitimate indulgences of the imagination. Would he have allowed retroduction in areas other than the microworld, in astrophysics or geology, for example? It is not clear. Is he, to speak very loosely, in the early lineage of van Fraassen or of Laudan? Is he prompted by an empiricism that would disallow any attempt to postulate entities that are in principle unobservable? Or is he motivated by a distrust of the ontological significance of explanatory models in any domain of science, whether micro or macro?

Lugg notices that the arguments Duhem employs for the ontological significance of the classifications found in physical theory are remarkably similar to one set of arguments used by contemporary defenders of scientific realism. Duhem notes the fertility and the unifying power of the abstract laws of physics, and urges that these cannot possibly be an "accident"; they are best explained by supposing these laws to reflect "realities whose essence cannot be grasped by [the] methods [of science], . . . arranged in a certain order which physical science cannot directly contemplate" (1905, p. 297). Here (as Lugg remarks) is inference to best explanation at the meta-level, more problematic in Duhem's use of it than in that of the contemporary realists who do not (as he does) implicitly question its validity at the object level. Why did it not occur to him that the sort of argument he uses for his realism of relations could just as easily be used for a realism of micro-entities?

Perhaps it was because these hidden entities seemed to him so remote from human modes of perception and conception; they could be reached only in imagination, and he distrusted imagination. But as he scrutinized the historical record, the role played by metaphysics seems to have bothered him even more. Only by drawing upon a cosmology that legislated the acceptable sorts of entity and the permissible modes of interaction could the theorist (it seemed) construct a causal account of what supposedly goes on beneath the accessible surface of appearance. Not only are the facts of science theory-dependent, but explanatory theories have always been metaphysics-dependent. And this second sort of dependence has not been beneficial; atomists, Cartesians, and others have imposed their own notions of mechanism, and none have found any lasting success. A theme to which he returns again and again, one to which he clearly gave emotional as well as intellectual weight, is the importance of recognizing the basic autonomy of physics. The progressive unification which has gone on since Galileo's day has proved, he asserts, to be in no need of the imaginative dress of cosmology.

Duhem clearly thinks of metaphysics as a contaminant in the earlier story of physical theory. Because the physicist wrongly aspires to penetrate to bodies beyond the level of perception:

> He no longer has the right to shut his ears to what metaphysics wishes to tell him about the real nature of matter; hence as a consequence, through dependence on metaphysical cosmology, his physics suffers from all the uncertainties and from all the vicissitudes of that doctrine. (1917, p. 1)

His theories are thus "condemned to perpetual reformulation", and cannot assure the consensus and progress of which science is capable.

Duhem's attitude to metaphysics is puzzling.[5] On the one hand, in passages such as this one, he appears skeptical of the insights it claims into the true nature of physical things. On the other, he is careful not to deny its legitimacy as an autonomous mode of inquiry: "Our inquiry concerning physics has not led us either to affirm or deny the existence and legitimacy of methods of investigation foreign to this science" (1905, p. 280). Indeed, he argues that metaphysics and physics ought ultimately converge on the same natural classification, and suggests that the cosmology towards which his favored science of energetics is tending is the Aristotelian one, rid of its "fossilized doctrines" (1954, p. 308). What might give cosmology access to the structure of the physical world independently of scientific inquiry he never discusses. Are we to suppose he is speaking here as a Catholic apologist? And if we are, must this be thought to be merely a strategy on his part or a founded belief? Difficult but important issues, ones that cannot be addressed in short compass.

From the standpoint of contemporary scientific realism, Duhem appears to have seriously undervalued the resources of retroduction.

Imagination is not as dependent on prior cosmological commitment as he supposed, and the criteria of fertility and unification that he valued can direct imagination more effectively than he allowed. It is true that in the historical cases he studied, the warrant for ethers and atoms lay not so much in their contribution to a predictive model as in a prior philosophy of nature. It is also true that his focus was on mechanics where the purchase of realism has for quite specific reasons always been precarious. Had he looked more closely at the structural sciences of his own day, he might not have been quite so pessimistic about the ability of the theorist to divine the shapes of entities that escape the contingent modes of human perception.

NOTES

[1] Historians have been critical of Duhem's instrumentalist reading of the Platonic tradition of 'saving the phenomena'. The commitment to circular motions and uniform speeds of rotation would of itself suggest that the mathematical formalism was not chosen on merely pragmatic grounds; the circular motions were in some appropriately qualified sense regarded as real. And Duhem too easily ascribed straightforwardly instrumentalist views to writers such as Ptolemy whose real views were undoubtedly much more mixed. See G. E. R. Lloyd, 'Saving the Appearances', *Classical Quarterly* **28** (1979), 202–22.

[2] R. N. D. Martin is at pains to argue that Duhem's apparent support for Bellarmine and Urban and his criticism of Galileo must not be taken as an attempt on Duhem's part to vindicate the action of the ecclesiastical authorities in 1616 ('Saving Duhem and Galileo', *History of Science* **25** (1987), 301–19). Martin recalls Duhem's dictum that "pure logic is not the sole guide of our judgements", and suggests that the fact that Galileo is criticized here for his faulty logic ought alert us that "reasons of the heart" might (in Duhem's view) have been operating under the surface, and that it was Galileo in consequence who was on the right track after all.

Martin may well be right about Duhem's relation to the ecclesiastical authorities of his own day. But it is important to note that the insights Duhem finds hidden in Galileo's work are (as Martin himself goes on to point out) cosmological ones about the unity of earth and sky. Indeed, Duhem is explicit in saying that the truth Galileo was, all unknowingly, introducing was that "one form of dynamics, by means of a single set of mathematical formulae, must represent the movements of the stars . . . (and) the fall of heavy bodies" (1908, pp. 116–7). But this was not the issue between Galileo and Bellarmine. On *that* issue, the Copernican claim about the reality of the earth's motion, Duhem never qualifies his original claim that Galileo was wrong and that his critics were right. They had, in this respect, understood the limitations of the experimental method better than Galileo did (p. 13); see also 1914, p. 43. They realized (he alleges) that the hypotheses of the astronomer are not, in fact, "judgements about the nature of heavenly things and their real movements" (1908, p. 116). On this point, Duhem was entirely in agreement with them, and not with Galileo. The limited realism of the 'natural classification' later disclosed in Newtonian dynamics would undermine the simple instrumentalism they imposed on mathematical astronomy. But it would not validate the realism of Galileo's original position.

[3] Instrumentalists might not find the theme of unification as congenial as Duhem assumes; nor would the 'natural classification' imposed by a mechanics that prescinds from causal explanation appear all that oppressive to them. It is in the end not clear what the notion of classification amounts to in the domain of mechanics (by contrast with biology or chemistry). What is being classified, if inference to unperceived entities is forbidden?

[4] He never did (so far as we know) relax his opposition to a realist construal of atoms, even in the light of the new arguments from Brownian motion which convinced Poincaré and Ostwald, and even perhaps Mach. In his last published writing, *La Science Allemande* (1915), he criticized the physics of electrons as a typical product of the *esprit géometrique* so characteristic of the German mind (pp. 131–4). He rejected the theory of relativity on the same grounds, deploring its disdain for common sense (pp. 134–9).

[5] It is worth noting that metaphysics plays a dual role for him, first as cosmology or philosophy of nature, and second, as reflection on the aims and limitations of science, i.e., as philosophy of science. The argument for the convergence of physics on a natural classification transcends the unaided resources of the physicist; it requires a 'metaphysical assertion', but one whose validity is nevertheless crucially important to Duhem's entire position. He is obviously much more comfortable with 'metaphysics' in this second sense than in the first.

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DUHEM AND HISTORY AND PHILOSOPHY OF MATHEMATICS*

ABSTRACT. The first part of this paper consists of an exposition of the views expressed by Pierre Duhem in his *Aim and Structure of Physical Theory* concerning the philosophy and historiography of mathematics. The second part provides a critique of these views, pointing to the conclusion that they are in need of reformulation. In the concluding third part, it is suggested that a number of the most important claims made by Duhem concerning physical theory, e.g., those relating to the 'Newtonian method', the limited falsifiability of theories, and the restricted role of logic, can be meaningfully applied to mathematics.

It is an interesting but rarely noted fact that Pierre Duhem included a number of claims concerning the history and philosophy of mathematics in his *Aim and Structure of Physical Theory* as well as in his other writings (Duhem 1954; 1909; 1915).[1] Although these claims may at times appear to be digressions, careful examination shows that they function in a significant manner in Duhem's exposition of his philosophy; in particular, Duhem in many cases formulated his main positions regarding physical theory by contrasting it with mathematics. In the three parts of the present paper, I shall suggest answers to the following three questions:

(1) What views did Duhem express in his *Aim and Structure of Physical Theory* concerning the nature and development of mathematics?

(2) Are these views correct?

(3) Can any of Duhem's ideas concerning the nature and development of physical theory be applied to mathematics?

The surprising result that has emerged from my efforts to answer these questions is the recommendation that the second question be answered negatively, but the third affirmatively. It is hoped that the analysis in this paper will simultaneously contribute to a deeper understanding of Duhem's thought and also shed light from a Duhemian direction on the search by historians and philosophers of mathematics for patterns of conceptual change in mathematics.[2]

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PART ONE

What were Duhem's views about the history and philosophy of mathematics? The following three claims are probably among the most important:

(1) The *method* of mathematics is 'profoundly different' (Duhem 1954, p. 265) from that of physics. In support of this claim, Duhem asserted that mathematicians begin with axioms, which are universally accepted, whereas physicists repeatedly alter their theories in response to new empirical information. Moreover, whereas mathematicians must follow logic, physicists in the process of formulating theories have the freedom at times to set logic aside.

(2) The *development* of mathematics has been very different from that of physics. For example, mathematics grows in a linear and cumulative fashion and has avoided the controversies that have beset physics.

(3) A knowledge of the history of physics is vitally important to physicists, whereas mathematicians need have no knowledge of the history of their discipline.

Allied to these claims are some less central points, for example, that mathematicians make extensive use of the reduction to absurdity method, whereas physicists are barred from employing this powerful technique (Duhem 1954, p. 188).

Let us now examine some passages in Duhem's *Aim and Structure* where he articulated these claims. The first claim is embodied in Duhem's warning that:

> The plan to obtain from common-sense knowledge the demonstration of hypotheses on which physical theories rest is motivated by the desire to construct physics in imitation of geometry; in fact, the axioms from which geometry is derived with such perfect rigor, the 'demands' that Euclid formulated at the beginning of his *Elements* are propositions whose self-evident truth is affirmed by common sense. But we have seen on several occasions how dangerous it is to establish an alliance between mathematical method and the method that physical theories follow; how, underneath their entirely external resemblance, . . . these two methods reveal themselves to be profoundly different. (Duhem 1954, p. 265)

Shortly thereafter, Duhem contrasted the clarity and simplicity of mathematical ideas with the confusion and complexity of concepts in physics:

> [T]he mathematical sciences are very exceptional sciences; they are fortunate enough to deal with ideas which emerge from our daily perceptions through the spontaneous work of abstraction and generalization, and which still appear afterwards as clear, pure, and simple.

This good fortune is refused in physics. The notions provided by the perceptions with which it has to deal are infinitely confused and complex notions, the study of which requires long and painful work of analysis. (Duhem 1954, p. 266)

In describing the methodology of physics, Duhem also warned against excessive reliance on logic and, moreover, stressed the limitations of what Duhem, following Pascal, called the 'geometrical mind'.

Pure logic is not the only rule for our judgments; certain opinions [in theoretical physics] which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic yet direct our choices, these 'reasons which reason does not know' and which speak to the ample 'mind of finesse' but not to the 'geometrical mind', constitute what is appropriately called good sense. (Duhem 1954, p. 217)

Duhem's stress on the dissimilarities between the methods of mathematics and of physics was no doubt linked to his conviction that the patterns of development characteristic of these two disciplines have also been very different. Regarding the pattern of development of mathematics, Duhem remarked:

The propositions that make up purely mathematical sciences are, to the highest degree, universally accepted truths. The precision of language and the rigor of the methods of demonstration leave no room for any permanent divergences among the views of different mathematicians: over the centuries doctrines are developed by continuous progress without new conquests causing the loss of any previously acquired domains.

There is no thinker who does not wish for the science he cultivates a growth as calm and as regular as that of mathematics. But if there is a science for which this wish seems particularly legitimate, it is indeed theoretical physics, for of all the well-established branches of knowledge it surely is the one which least departs from algebra and geometry. (Duhem 1954, p. 10)

Nonetheless, theoretical physics, according to Duhem, has enjoyed no such "calm" and "regular" development. In fact, he described it as having been beset throughout most of its history by "perpetual, sterile disputes" (Duhem 1954, 107). Duhem attributed many such disputes to the tendency of physicists, when formulating their theories, to have recourse to metaphysics; as he stated: "to make physical theories depend on metaphysics is surely not the way to let them enjoy the privilege of universal consent" (Duhem 1954, p. 10).

Elsewhere in his book, Duhem elaborated on this point in more detail, contrasting the linear and cumulative character of the development of mathematics with the organic pattern of growth he attributed to physics.

Physics makes progress through . . . continually supplementing laws in order to include the exceptions. It was because the laws of weight were contradicted by a piece of amber rubbed by wool that physics created the laws of electrostatics, and because a magnet lifted iron despite these same laws of weight that physics formulated the laws of magnetism . . . Physics does not progress as does geometry, which adds new final and indisputable propositions to the final and indisputable propositions it already possessed . . . . (Duhem 1954, p. 177)

Duhem later repeated this point, drawing implications from it for the pedagogy of physics:

Instruction [in physics] ought to get the student to grasp this primary truth: Experimental verifications are not the base of theory but its crown. Physics does not make progress in the way geometry does: the latter grows by the continual contribution of a new theorem demonstrated once and for all and added to theorems already demonstrated; the former is a symbolic painting in which continual retouching gives greater comprehensiveness and unity, and the *whole* of which gives a picture resembling more and more the *whole* of the experimental facts, whereas each detail of this picture cut off and isolated from the whole loses all meaning and no longer represents anything. (Duhem 1954, pp. 204–5)

It was no doubt because he felt these points were so significant that he stressed the importance for the physicist of a knowledge of the history of physical theory, even while denying that the history of mathematics has a comparable role to play in mathematics. He asserted:

This importance which the history of the methods by which discoveries are made acquires in the study of physics is an additional mark of the great difference between physics and geometry.

In geometry, where the clarity of deductive method is fused directly with the self-evidence of common sense, instruction can be offered in a completely logical manner. It is enough for a postulate to be stated for a student to grasp immediately the data of common-sense knowledge that such a judgment condenses; he does not need to know the road by which this postulate has penetrated into science. The history of mathematics is, of course, a legitimate object of curiosity, but it is not essential to the understanding of mathematics.

It is not the same with physics. There, we have seen, it is forbidden to be purely and completely logical in teaching. Consequently, the only way to relate the formal judgments of a theory to the factual matter which these judgments are to represent, and still avoid the surreptitious entry of false ideas, is to justify each essential hypothesis through its history.

To give the history of a physical principle is at the same time to make a logical analysis of it. (Duhem 1954, p. 269)

PART TWO

With this information as background, let us examine the validity of Duhem's claims about the history and philosophy of mathematics. The theses that I shall attempt to develop are (1) that the above cited claims of Duhem are all seriously defective, and (2) that a number of Duhem's most famous claims about physical theory can shed light on the history and philosophy of mathematics.

It is an interesting fact that Duhem's claims about the calm inevitability and the linearity of the development of mathematics were made at a time when mathematics was undergoing major changes and was beset by a variety of controversies. To see evidence of these alterations and altercations concerning mathematics, one needs look no farther than the philosophical writing of Duhem's contemporary Henri Poincaré. One wave of controversy began with the creation during the 1840s by William Rowan Hamilton and by Hermann Günther Grassmann of nontraditional algebras, for example, algebras in which the commutative law for multiplication is not obeyed, that is, where A × B does not equal B × A. The broadened view of algebra that resulted included the realization that mathematicians can create new and useful algebraic systems very different from that single system that had been central to mathematics before 1830 (Crowe 1985). One example of the rich opportunities that were opened up by this new view of algebra is Benjamin Pierce's *Linear Associative Algebra* of 1870 in which Pierce delineated 162 different algebraic systems. Another embodiment of this result was a debate that raged from about 1870 to about 1900 over the various systems of vectorial analysis. Duhem, from the beginning of his scientific training, must have encountered this controversy concerning which vectorial system – the Hamiltonian, the Grassmannian, or the Gibbs-Heaviside system – should be employed in physics and geometry, or whether no vectorial methods should be employed. Aspects of this debate surfaced in Duhem's *Aim and Structure of Physical Theory*, where he somewhat disparagingly dismissed the British penchant for vectorial methods as another example of the British passion for concrete representations of physical quantities (Duhem 1954, pp. 77–79).

The shock experienced by the mathematical community at the creation of nontraditional algebras was far surpassed by the tremor that gradually began to spread after 1829 when Nicolai Lobachevsky published the first non-Euclidean geometry (Bonola 1955; Gray 1979; Trudeau 1987). Four years later and independently of Lobachevsky, Johann Bolyai published his essentially identical system. In 1851, Bernhard Riemann presented his famous 'Ueber die Hypotheses, welche der Geometrie zu Grunde liegen', in which he introduced a second major non-Euclidean system. Among the French, it was above all Jules Hoüel who introduced his countrymen to these radically new and different geometrical systems. The spread of non-Euclidean geometries in France can be dated from 1866 when Hoüel published a French translation of one of Lobachevsky's presentations along with selections from Gauss's correspondence with Schumacher. The publication of Gauss's letters was crucially important because they revealed that this eminent mathematician had endorsed these geometries before his death in 1855.

Although Duhem made no mention of non-Euclidean geometry in his *Aim and Structure*,[3] the philosophical implications of the new geometries were noted by a number of French authors, particularly Poincaré, who in his *Science and Hypothesis* of 1902 put forth the radical assertion that "*The geometrical axioms . . . are neither synthetic a priori intuitions nor experimental facts.* They are conventions" (Poincaré 1952, p. 50). The changes in geometry went substantially beyond this. Not only was geometry forced to expand so as to be capacious enough to include both the Euclidean and the non-Euclidean systems as well as geometries of more than three dimensions, but also Euclid's paradigmatic *Elements* was seriously challenged. In this regard, C. S. Peirce asserted in 1892:

> Euclid's treatise was acknowledged by all kinds of minds to be all but absolutely perfect in its reasoning, and the very type of what science should aim at as to form and matter . . .
>
> The truth is that elementary geometry, instead of being the perfection of human reasoning, is riddled with fallacies, and is thoroughly unmathematical in its method of development. (Peirce 1975, pp. 136–7)

As Joan Richards has recently documented in detail, a major controversy erupted in England during the final decades of the nineteenth century concerning not only the non-Euclidean geometries, but over Euclidean geometry itself (Richards 1988). One major culmination of this controversy was the publication in 1899 by David Hilbert of his *Grundlagen der Geometrie* in which he reformulated the axioms of Euclidean geometry in a strikingly new and more rigorous manner.

The third major branch of mathematics, analysis, was also beset by changes. The very foundations of the calculus were repeatedly reformulated by various mathematicians during the nineteenth century, most notably Cauchy and Weierstrass (Boyer 1968, chaps. 23, 25; Hahn 1956; Kline 1980, chaps. 8–9). The realization of the necessity for this was linked to such results as the violation of traditional intuition by such discoveries as that of functions that are everywhere continuous but nowhere differentiable. By 1900, probably the greatest controversy surrounded the issue of what to make of the introduction by Georg Cantor of transfinite numbers – which involved the acceptance of orders of infinite quantities within mathematics.

The list of such fundamental changes in mathematics could be substantially extended, for example, by a discussion of the work commencing in the 1890s by Whitehead, Russell, Peano, and Frege on the logical foundations of mathematics. Moreover, much could be said about the problems evident in Duhem's description of mathematical propositions as "universally accepted truths" and of mathematical theorems as "demonstrated once and for all" in light of his statement in his *La Science allemande* that "The great men who, from the XVIIth to the middle of the XIXth century, have created Algebra, Integral Calculus, and Celestial Mechanics, have often justified their most important discoveries with the aid of defective arguments or even by flagrant paralogisms" (Duhem 1915, p. 7). But enough has already been noted to suggest that Duhem's characterization of mathematics as, unlike physics, enjoying a "calm and . . . regular" development in which progress is made by the adding of "new final and indisputable propositions to the final and indisputable propositions it already possessed . . ." is beset by problems.

PART THREE

It seems unnecessary to elaborate further at this point on the questionable character of Duhem's claims about mathematics. What I shall do now is investigate whether any of the central theses in Duhem's analysis of physical theory can be applied to mathematics and its development. If it can be shown that this is in fact the case, it will emerge as a secondary result that Duhem's explicit contrast between physical theory and mathematics should be viewed as flawed. In other words, if it can be shown that the methodology and development of mathematics fit with some of Duhem's fundamental theses about the nature and development of physical theory, then it will be evident that these disciplines are not as 'profoundly different' as the previously cited quotations from Duhem would lead one to believe.

What are Duhem's most important claims about physical theory? Although not complete, the following list includes a number of them.

(1) The so-called Newtonian method of doing physics in which each

of the fundamental laws of physics is built up directly from experiment is not the method that physicists have followed (whatever their claims to the contrary may be), nor is it the method physicists should invariably pursue in attempting to develop theories. One aspect of this claim is Duhem's assertion that experiments, rather than being the basis of physics, are its crown. I shall call this first claim the 'Newtonian method as myth claim'.

(2) According to Duhem, theories in physics, rather than being isolated entities that can be directly tested, are bound together in clusters. Moreover, he asserted that when confronted with a contradiction, theories can in many cases be rescued by modifying another element in the cluster. In short, this claim concerns the ability of theories to resist falsification.

(3) The role that logic has played and should play in physical theory is substantially more limited than is commonly assumed. The physicist must in a fundamental way rely on good judgment, on 'bon sens'. Correspondingly, physical theories must be judged as wholes. The physicist, rather than being like a watchmaker who examines a watch by taking it apart, is like the physician who, prevented from dissecting patients, must examine them as entire entities, attempting to postulate causes of disease that explain the symptoms afflicting patients (Duhem 1954, p. 188). In this sense, Duhem stressed the human quality of the work of the theoretical physicist. In what follows this overall claim will be referred to as the 'restricted role of logic claim'.

(4) A knowledge of the history of physical theory is important for the physicist; for example, it can save the physicist from the "mad ambitions of dogmatism as well as the despair of Pyrrhonian skepticism" (Duhem 1954, p. 270). Duhem's fourth claim can be designated as the 'relevance of history claim'.

Let us now examine each of these four Duhemian claims about physical theory, attempting in each instance to see whether analogues applicable to mathematics can be formulated.

One of the most brilliant insights that Duhem drew from his experience teaching physics was that what he called the "Newtonian method" of developing physical theory is a myth. He described this doctrine, which he associated with Newton's "General Scholium" in his *Principia*, as the requirement that the fundamental hypotheses of a physical theory "must be tested one by one; none would have to be accepted until it presented all the certainty that experimental method can confer on an abstract and general proposition; that is to say, each would necessarily be either a law drawn from observation by the sole use of those two intellectual operations called induction and generalization, or else a corollary mathematically deduced from such laws" (Duhem 1954, p. 190). In his 'Physics of a Believer', Duhem recounted how, after having been taught at the École normale that this is the proper method for physical theory, he found when he first began teaching physics at Lille that this method is a myth (Duhem 1954, pp. 275–79), a "chimera" as he called it (Duhem 1954, p. 200). In arguing against the Newtonian method in his *Aim and Structure*, Duhem demonstrated that neither Newton nor Ampère, despite their claims to the contrary, followed this method. Near the end of his analysis, Duhem asserted:

> Experimental verifications are not the base of theory but its crown. Physics does not make progress in the way geometry does: the latter grows by the continual contribution of a new theorem demonstrated once and for all and added to theorems already demonstrated; the former is a symbolic painting in which continual retouching gives greater comprehensiveness and unity, and the *whole* of which gives a picture resembling more and more the *whole* of the experimental facts . . . . (Duhem 1954, pp. 204–5)

Let us now ask: is there a myth about mathematical method analogous to that which Duhem detected for physical theory? I suggest that this is in fact the case and that the myth can appropriately be called the 'myth of the Euclidean method'. The traditional interpretation of Euclid, derived partly from Aristotle's writings, is that Euclid began with a number of definitions, axioms, and postulates that were based on experience and that from these fundamentals, by purely deductive means, he derived the 465 theorems contained in his *Elements*. It is further asserted that the truth of Euclid's later propositions, for example, the Pythagorean theorem (Bk. I, Prop. 47), is guaranteed by the certainty of the postulates and axioms as well as by the deductive structure of the derivation. The idea is that the mathematician proceeds from the better known postulates and axioms to the less well known theorems. Moreover, it is frequently assumed that the logical structure of Euclid's *Elements* more or less exactly duplicates the historical sequence in which the propositions were discovered. But this portrayal of the Euclidean method is surely a myth. First of all, it may be significant that Euclid himself made no such explicit claims about the certainty of his axioms and postulates. In fact, historical research has shown that even before Euclid, a number of Greek mathematicians favored a quasi-formalist approach, according to which the beginning principles are taken simply as postulates, rather than as indubitable generalizations from experience (Lasserre 1964, chap.1). And there are deeper difficulties. Recall that the Pythagorean theorem, rather than being a creation of Euclid or even of Pythagoras, has been traced to Babylonian clay tablets of the eighteenth century B.C. Such information suggests that what Euclid knew best were not his somewhat artifically formulated definitions, axioms, and postulates but such results as the Pythagorean theorem, that Euclid, rather than composing this theorem as the last stage of his preparation of Book One of his *Elements*, may very well have formulated his definitions, axioms, and postulates late in the process of composing Book One. Moreover, it seems plausible to argue that what gave Euclid confidence in those beginning principles was above all that he found he could derive from them such certain results as the Pythagorean theorem. This is to suggest that in an important sense, mathematicians, including those who work in pure mathematics, employ the hypothetico-deductive method in which the fundamental principles are warranted by the conclusions that can be drawn from them.[4]

When examined from a broader perspective, this claim may appear less extreme. Where and when did the fundamental postulates of modern Euclidean geometry have their origin? Their source is not lost in the mists of Greek antiquity as is sometimes assumed; they derived from late nineteenth-century Germany, in particular, from Hilbert's *Grundlagen der Geometrie*. Possibly even this claim looks too far into the past. The fundamental principles of the most recent geometry texts are no doubt of more recent vintage, resulting from subsequent critiques of Hilbert's formulation.

The same point emerges from a knowledge of the history of other areas of mathematics. When was the fundamental theorem of algebra first proven? Early in the nineteenth century. The same period saw the formulation of such other fundamental algebraic entities as the associative, commutative, and distributive laws. Where in algebra texts are these fundamental principles presented? At the very beginning, whereas algebraic theorems developed in many cases centuries earlier appear on subsequent pages. Similarly, examination of a calculus text reveals that many of its most complicated theorems are of early vintage, whereas the fundamental principles, the definitions of such crucial entities as function and limit, came forth a century or more later as a result of the rigorization of calculus that was among the major achievements of nineteenth-century mathematics. Mathematics is not a tree that grows only at its upper extremities; rather the roots are involved in continuous transformation. We can put this overall point in Duhemian terms: the postulates and fundamental principles of mathematics are not only the base of mathematics but also its crown. Mathematics develops as a whole, with alterations occurring in every part, including at its foundation. The growth of mathematics is not linear, but organic.

Before leaving this point, let us return twice to Duhem's text. In the course of his refutation of the Newtonian method, Duhem made the surprising remark:

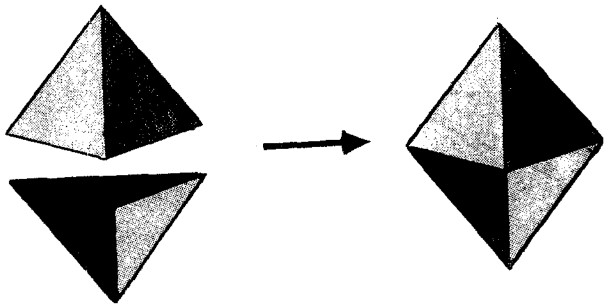
> It is as impracticable for the physicist to follow the inductive method . . . as it is for the mathematician to follow that perfect deductive method which would consist in defining and demonstrating everything, a method of inquiry to which certain geometers seem passionately attached, although Pascal properly and rigorously disposed of it a long time ago. (Duhem 1954, p. 201)[5]

It seems that Duhem, who as Niall Martin has shown drew so heavily upon Pascal (Martin 1981, chaps. 6–7), failed to realize fully the implications of this assertion. Another conclusion that Duhem drew from his analysis of the Newtonian method also merits consideration. Late in that analysis in which he had vigorously contrasted the methodologies of physics and mathematics, Duhem asserted that physical theory, rather than beginning from experiments, is "grounded on postulates, that is to say, on propositions that it is at leisure to state as it pleases, provided that no contradiction exists among the terms of the same postulate or between two distinct postulates" (Duhem 1954, p. 206). If postulates play such a prominent role in physical theory, this surely suggests that its methodology is not so dissimilar from that of mathematics.

Let us turn now to Duhem's claim concerning the ability of theories to resist falsification.[6] In his exposition of this famous claim, Duhem, again contrasting the methods of physics and mathematics, asserted:

> Those who assimilate experimental contradiction to reduction to absurdity imagine that in physics we may use a line of argument similar to the one Euclid employed so frequently in geometry. Do you wish to obtain from a group of phenomena a theoretically certain and indisputable explanation? Enumerate all the hypotheses that can be made to account for this group of phenomena; then, by experimental contradiction eliminate all except one; the latter will no longer be a hypothesis, but will become a certainty. (Duhem 1954, p. 188)

Duhem proceeded to argue that the reduction to absurdity method, although of such great power in mathematics, is not comparably applicable in physics. As he stated: "Unlike the reduction to absurdity employed by geometers, experimental contradiction does not have the power to transform a physical hypothesis into an indisputable truth . . ." (Duhem 1954, p. 190). Implicit in his analysis was the doctrine that whenever a contradiction is encountered in mathematics, the mathematical claim from which the contradiction was derived must be abandoned. Although this may seem sensible, good evidence indicates that this is not a mandate that mathematicians have always felt constrained to follow. I know of no better demonstration of this point than Imre Lakatos's *Proofs and Refutations*. In that work, Lakatos examined the history of Euler's claim that the number of faces, edges, and vertices of polyhedra always obey the equation $V - E + F = 2$. What Lakatos found was that throughout its history, this claim, as well as proofs presented for it, repeatedly encountered contradictions, none of which was deemed decisive; in fact, Euler's conjecture was in every instance rescued from falsification. In tracing this history, Lakatos revealed the rich repertoire of techniques available to mathematicians wishing to rescue mathematical entities beset by counterexamples. Moreover, numerous other cases of apparent contradictions can be cited from the history of mathematics in which the favored concept, law, or theorem was salvaged. Consider the celebrated theorem with which Euclid brought his *Elements* to a close and for the sake of which, according to some commentators, he composed the entire work: "No other figure, besides [the five regular solids] can be constructed which is contained by equilateral and equiangular figures equal to one another". Suppose Euclid were shown the six-sided figure (see figure) formed by placing together two regular tetraheda. This new solid, although fully conforming to Euclid's definition of 'regular solid', refutes his theorem. One can scarcely imagine that Euclid would have been led thereby to abandon his theorem. Rather what he would have done is to salvage his theorem by modifying his definition of regular solid, as was later done, so as to exclude this counterexample. To provide another example: think of complex numbers, which faced constant contradictions. Throughout most of their history they stood in contradiction to the such laws as that every number must be greater than, equal to, or less than zero, that the square of any number must be positive, and that any algebraic entity must be geometrically representable. They survived



tations, although other elements in mathematics, for example, the definition of number itself required modification. It can of course occur that the mathematical community will decide to declare a contradiction to be conclusive, but this is a matter of choice and may involve extensive controversy

This analysis of Duhem's second claim sets the stage for a consideration of his third claim, what I have called the "restricted role of logic claim". In one of the most controversial, and possibly least understood sections of Duhem's *Aim and Structure*, he stressed that at times physicists find themselves convinced that a theoretical system must be modified, even though experiment has not provided sufficient evidence as to what elements are to be altered. In those instances, Duhem asserted, "No absolute principle directs this inquiry, which different physicists may conduct in very different ways without having the right to accuse one another of illogicality" (Duhem 1954, p. 216). What is to be done in such cases? Duhem's answer, which some see as implying the abandonment of logic and as entailing surrendering to relativism, was to remind his readers that

> Pure logic is not the only rule for our judgments; certain opinions [in theoretical physics] which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic and yet direct our choices, these "reasons which reason does not know" and which speak to the ample "mind of finesse" but not to the "geometrical mind", constitute what is appropriately called good sense. (Duhem 1954, p. 217)

Duhem further underlined the inevitably human character of theoretical work in physics by adding:

> The sound experimental criticism of a hypothesis is subordinate to certain moral conditions; in order to estimate correctly the agreement of a physical theory with the facts,

it is not enough to be a good mathematician and a skilled experimenter; one must also be an impartial and faithful judge. (Duhem 1954, p. 218)

In short, Duhem espoused the position, too rarely explicitly admitted in treatises on scientific method and sometimes implicitly denied in them, that important as logic is in physical inquiry, human factors influence the inquirer. Among such factors are impartiality and the 'reasons of the heart', which cannot, ultimately be reduced to quasi-mechanical processes of reasoning.

Certainly a comparably human element is to be found among mathematicians, who repeatedly face decisions that are not governed solely by logic. Many areas of mathematics, analysis most famously, have been beset by inconsistencies, anomalies, contradictions (real and apparent), and counter-intuitive deductions, concerning which mathematicians have been forced to adopt a position. Mathematicians must also select the postulates from which a mathematical system begins. In this regard, it is relevant to recall Duhem's statement, cited previously, that it is impractical for the mathematician to rely on that "perfect deductive method which would consist in defining and demonstrating everything, a method of inquiry [that] Pascal properly and rigorously disposed of . . . a long time ago" (Duhem 1954, p. 201). Moreover, mathematicians must regularly choose among various mathematical methods of attacking problems; a relevant example, where Duhem was himself involved, was the decision as to whether or which vectorial methods should be employed.

In this overall context, it is interesting to note that Duhem, following Pascal, stressed the variety of styles exhibited by working mathematicians. In particular, he pointed out that important contributions have been made to mathematics by persons possessing the ample mind of finesse rather than the geometrical mind. Duhem asserted:

It is . . . ampleness of mind which constitutes the peculiar genius of many a geometer and algebraist. More than one reader of Pascal, perhaps, will not fail to be astonished on seeing him sometimes place mathematicians among the number of ample but weak minds. This cross-classification is not one of the lesser proofs of [Pascal's] penetration. (Duhem 1954, p. 62).

And Duhem illustrated this point by a rich array of examples.[7]

Duhem's use of Pascal's famous classification of minds suggests another point, which is of general relevance. It should come as no surprise that Duhem's ideas about physical theory have applications to mathematics if it is recalled that Duhem, when formulating his views on methodology, relied heavily on the writing of Pascal, who had originally formulated many of his methodological ideas with reference primarily to mathematics.

Finally, let us turn to Duhem's claim that a knowledge of the history of physical theory is of direct value to physicists. One of the chief arguments Duhem provided for this claim was that, as he stated, "To give the history of a physical principle is at the same time to make a logical analysis of it" (Duhem 1954, p. 269). This statement, however, needs commentary, because its deeper meaning is somewhat different from what one might infer from a first reading. In particular, it seems probable that what Duhem was suggesting was not that historical analysis does precisely what ordinary logic can also accomplish, but rather that a historical investigation of a physical theory can create an awareness of the deeper logic of the theory, of those "reasons that reason does not know", those reasons that transcend ordinary logic but that are the province of 'bon sens'. A number of the arguments made in this paper, and not least its central theses, suggest that a comparable benefit should result from approaching mathematics in a historical manner. Moreover, a knowledge of the historical development of mathematics may save not only the mathematician, but also the philosopher of physical theory, from distorted claims about the aim and structure and development of mathematics.

Before concluding this paper, I should add a final note that is both historically significant and a further support for its central claim. After drafting the paper, I read an essay published in 1907 by Pierre Boutroux (1880–1922), the son of the philosopher Emile Boutroux. The younger Boutroux was a mathematician who also made important contributions to the history and philosophy of mathematics. In his essay, which he entitled '*La Théorie physique* de M. Duhem et les mathématiques', Boutroux, using a different set of arguments from those I have presented, urged that a number of Duhem's doctrines concerning physical theory are also applicable to mathematics. For example, Boutroux stated:

> For some years I have sought to show . . . that Mathematical Analysis is not a perfect and exceptional science, that its evolution recalls to mind, in many cases, the evolution of the physical sciences . . . . I have the impression that one can apply to mathematics what Duhem says of physics. (Boutroux 1907, p. 368)

Boutroux illustrated this claim by noting, for example, the importance of experimentation in mathematics as well as of 'bon sens' and intuition.

I shall not attempt to recapitulate his valuable analysis, but shall only note that I share not only Boutroux's view but also his hope that Duhem would find such an analysis of interest and value.

NOTES

* I am indebted to Professors Douglas Jesseph and Philip Quinn for helpful comments on this paper.

[1] On Duhem's views concerning the nature of mathematics, see Boutroux (1907), his nearly identical Boutroux (1920), and Jaki (1984), 349–51, 361.

[2] I have discussed the views of a number of authors, including Duhem, on the historiography of mathematics in Crowe (1988).

[3] Duhem did discuss non-Euclidean geometries to some extent in his *La Science allemande*; see, for example, pp. 113–22, where he expressed major reservations about such geometries.

[4] This point is developed in more detail in Crowe (1988), where it is shown that Hilary Putnam and others have maintained that mathematicians employ the hypothetico-deductive method.

[5] Although Duhem did not specify where Pascal had formulated this claim, he was no doubt thinking of Pascal's fragmentary 'De l'esprit géométrique'.

[6] For an important analysis of Duhem's ideas in this regard, see Ariew (1984).

[7] Duhem extensively discussed the relation to mathematics of the two types of minds in his *La Science allemande*

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RIGOROUS PROOF AND THE HISTORY OF MATHEMATICS: COMMENTS ON CROWE

ABSTRACT. Duhem's portrayal of the history of mathematics as manifesting calm and regular development is traced to his conception of mathematical rigor as an essentially static concept. This account is undermined by citing controversies over rigorous demonstration from the eighteenth and twentieth centuries.

In contrast to the history and philosophy of the physical sciences, relatively little scholarly attention has been devoted to the history and philosophy of mathematics. As Professor Crowe's paper suggests, however, the field is by no means sterile and we can be glad that the history and philosophy of mathematics is becoming the focus of more sustained and widespread scholarly activity. The main lesson to be drawn from Professor Crowe's investigation is that Duhem's views on the history and philosophy of mathematics, although not elaborated in great detail, stand in sharp contrast with his widely known account of the history and philosophy of physical science. I accept this fundamental claim as well as the suggestion that our understanding of the history and philosophy of mathematics would be improved if we applied Duhem's more celebrated account of the development of physical theory to episodes of conceptual change in the history of mathematics. In what follows, I would like to offer my own account of why we find Duhem treating physical and mathematical theories so differently and to show how his mistaken conception of the history and philosophy of mathematics is rooted in a misunderstanding of mathematical rigor. Thus, my purpose is to extend Professor Crowe's analysis in some respects and to link his treatment of Duhem with some of my own concerns about the history of the ideal of rigorous proof.

The best way to characterize Duhem's approach to the history and philosophy of mathematics is to see him as embracing an extreme continuity thesis – a thesis which holds in effect that the mathematical work of all eras has been the elaboration of the very same set of fundamental concepts, with innovation kept to an absolute minimum.

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Duhem is noted for claiming continuity between the physical theories of the Middle Ages and the seventeenth century, so it might not be too surprising to find him treating mathematical theories from Ancient Greece to the early twentieth century as continuous. Although I find the famous Duhem continuity thesis appealing as an account of the development of physical theory, I think his extreme conservatism about the history of mathematics goes too far. Let me first explain why I think it appropriate to characterize Duhem's approach to the history and philosophy of mathematics as a continuity thesis, and then go on to show what is wrong with it.

As Professor Crowe has noted, Duhem claimed that there are various respects in which the history of physics and the history of mathematics are different. It is worth observing, however, that these differences suggest that mathematical theories should be relatively unchanging when compared with physical theories. For example, Duhem's characterization of the growth of mathematics as 'calm and regular' suggests that mathematicians of the past have broken new ground by plodding along down the same path as their predecessors, only making an original contribution when they reached the limits of what had been previously established. In a similar vein, Duhem insists that the development of mathematics has been cumulative; on this account, geometry "only adds new final and indisputable propositions to the final and indisputable propositions it already possessed". Moreover, Duhem claims that the development of mathematics has not been marred by the sterile metaphysical disputes which have hindered the progress of physical theory.

These alleged differences between the history of mathematics and the history of physics all suggest an extreme continuity in the development of mathematics. In such a history of mathematics, all of the main players appear to be working on essentially the same project, results are added but never challenged, theories change (if at all) only by being generalized to include more cases, and there are no 'metaphysical' disputes which require that mathematicians return to the proverbial square one and wrangle over fundamental concepts.

Given that Duhem accepts such a view of the history and philosophy of mathematics, I think we can say that he was led to it by a conception of mathematical rigor which is essentially static. Such a static conception of rigor holds that the criteria for rigorous demonstration have been essentially the same over time, that they have been well understood and well articulated by mathematicians in all eras, and that new results have been added in the calm, regular development of mathematics when (and only when) they have been demonstrated according to this universally accepted standard of rigor.

Indeed, it is difficult to comprehend how Duhem's approach to the history of mathematics could get started without such an account of rigor. His repeated contrast of the 'method' of mathematics with the 'methods' of physics suggests that he believes that there is a unique mathematical method which has been followed for centuries, while the physical sciences have enjoyed no such unity of method. This unique mathematical method presumably requires an adherence to an unchanging conception of rigor and has (at least on Duhem's understanding of the matter) been followed at least since the time of Euclid.

Unfortunately for Duhem, this understanding of the history of mathematics is rather simplistic. Professor Crowe has drawn attention to nineteenth-century episodes in the history of mathematics which show the inadequacy of Duhem's approach, but I think that the case can be strengthened in important ways by directing our attention toward important controversies in the eighteenth and twentieth centuries. The controversies I have in mind are two: Berkeley's critique of the infinitesimal calculus in his 1734 work *The Analyst* and Brouwer's attack on nonconstructive analysis in the early decades of this century. These episodes are important not merely because they amplify the case made by Professor Crowe, but also because they suggest that the very notion of mathematical rigor has not been nearly as fixed and settled as Duhem apparently believed. A brief account of both of these controversies should serve to make my point.

In 1734, George Berkeley published a curious work entitled *The Analyst* which argued in part that the accepted methods of the calculus did not satisfy the proper criterion of rigor.[1] He observed that continental analysts in the Leibnizian tradition were quite happy to admit that there were quantities greater than nothing but less than any positive real number, but complained that the admission of such infinitesimal quantities did violence to the accepted canons of mathematical rigor. No such infinitesimal quantity can be observed, and it seems quite impossible to imagine a magnitude that satisfies these conditions. Moreover, he noted that the supposedly more rigorous Newtonian formulation of the calculus was equally unacceptable. Although Newton professed to be able to derive the fundamental results of the calculus without recourse to infinitesimal magnitudes, Berkeley noted that the

Newtonian demonstrations required a subtle but apparently fallacious maneuver in which a finite increment was supposed to be both greater than and equal to zero.

The responses to Berkeley's challenge are intriguing because they took exactly the form that Duhem suggests has never occurred in the history of mathematics. The dispute was unabashedly metaphysical, with emphasis being placed upon such topics as what laws of logic are correct, what kinds of entities may be introduced in a mathematical demonstration, and the subtle distinction between absolute nothing and the mere privation of something.[2]

The details are of no immediate interest here, but the point should be clear: in the mid-eighteenth century there was no universally accepted account of mathematical rigor, and the dispute between Berkeley and his opponents was largely a dispute over what constitutes rigorous demonstration. Berkeley advocated an essentially classical conception of rigor which denied the legitimacy of infinitesimal mathematics, while his opponents charged him with failing to understand the nature of mathematical demonstration. Curiously, Berkeley's opponents did not stop short of asserting that the calculus had to be legitimate simply because it worked, even though they admitted that its foundations were obscure.

But such disputes are not isolated episodes confined to the 1730s. Anyone who is familiar with mathematical intuitionism will admit that the issue of mathematical rigor has not always been the object of universal agreement. Brouwer and his followers claimed that much of what is accepted in 'classical' analysis is either false, improperly demonstrated, or downright meaningless.[3] Moreover, the Brouwerian insistence upon constructive proofs is quite obviously founded upon 'metaphysical' arguments concerning the capacity of human minds to comprehend infinitary quantifications. Thus, the dispute between intuitionists and classical mathematicians reduces to a dispute over the proper criteria for rigorous demonstration. Intuitionists are prepared even to reject classical logic in their campaign for a new standard of rigor, while their opponents insist that the accepted methods are unobjectionable and deserve to be retained because they are easier and more useful than the austere procedures of intuitionistic analysis. Whatever else one may chose to make of it, the development of analysis in the twentieth century suggests that Duhem's picture of the history of mathematics as a steady and unchallenged accumulation of new and universally accepted results is in need of drastic revision.

What, then, is the proper course to take in analyzing the history of mathematics? My proposal is that we abandon the idea that there is a fixed, immutable conception of mathematical rigor. This does not mean that 'anything goes' in mathematics, but rather that our understanding of the history of mathematics will be enhanced if we accept that the standards of rigor are not as unchanging as Duhem would have us believe. In this respect, Professor Crowe's suggestions for a reorientation of the history and philosophy of mathematics seem imminently reasonable, and we can expect to have a better understanding of the history and philosophy of mathematics if we discard the myth that mathematicians have always been guided by the same conception of rigor.

NOTES

[1] See Berkeley (1734) for the details of Berkeley's case against the calculus.

[2] See Cajori (1919) for a summary of this dispute.

[3] The case for intuitionism can be found in several papers by Brouwer, Heyting, and Dummett in Benacerraf and Putnam (1983).

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