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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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