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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 $A \times B$ does not equal $B \times A$. 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:

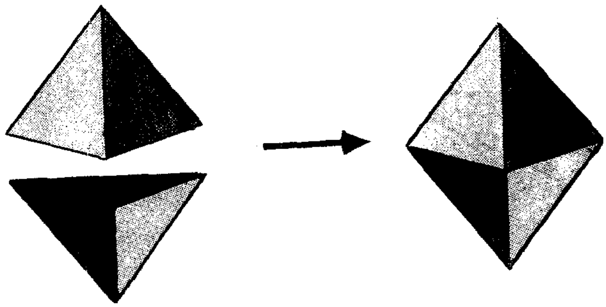
> It is as impracticable for the physicist to follow the inductive method . . . as it is for the mathematician to follow that perfect deductive method which would consist in defining and demonstrating everything, a method of inquiry to which certain geometers seem passionately attached, although Pascal 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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