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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).

REFERENCES

Benacerraf, P. and H. Putnam: 1983, *Philosophy of Mathematics: Selected Readings*, Cambridge University Press, Cambridge.

Berkeley, G.: 1734, *The Analyst*, in A. A. Luce and T. E. Jessop (eds.): 1957, *The Works of George Berkeley Bishop of Cloyne*, 9 vols, Thomas Nelson and Sons, London, vol. 4, pp. 53–102.

Cajori, F.: 1919, *A History of the Conceptions of Limits and Fluxions in Great Britain from Newton to Woodhouse*, Open Court, Chicago and London.

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