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Ten Misconceptions about Mathematics and Its History

For over two decades, one of my major interests has been reading, teaching, and writing history of mathematics. During those decades, I have become convinced that ten claims I formerly accepted concerning mathematics and its development are both seriously wrong and a hindrance to the historical study of mathematics. In analyzing these claims, I shall attempt to establish their initial plausibility by showing that one or more eminent scholars have endorsed each of them; in fact, all seem to be held by many persons not fully informed about recent studies in history and philosophy of mathematics. This paper is in one sense a case study; it has, however, the peculiar feature that in it I serve both as dissector and frog. In candidly recounting my changes of view, I hope to help newcomers to history of mathematics to formulate a satisfactory historiography and to encourage other practitioners to present their own reflections. My attempt to counter these ten claims should be prefaced by two qualifications. First, in advocating their abandonment, I am not in most cases urging their inverses; to deny that all swans are white does not imply that one believes no swans are white. Second, I realize that the evidence I advance in opposition to these claims is scarcely adequate; my arguments are presented primarily to suggest approaches that could be taken in more fully formulated analyses.

1. The Methodology of Mathematics Is Deduction

In a widely republished 1945 essay, Carl G. Hempel stated that the method employed in mathematics "is the method of mathematical demonstration, which consists in the logical deduction of the proposition to be proved from other propositions, previously established." Hempel added the qualification that mathematical systems rest ultimately on axioms and postulates, which cannot themselves be secured by deduction.[1] Hempel's claim concerning the method of mathematics is widely shared;

I accepted it as a young historian of mathematics, but was uneasy with two aspects of it. First, it seemed to make mathematicians unnecessary by implying that a machine programmed with appropriate rules of inference and, say, Euclid's definitions, axioms, and postulates could deduce all 465 propositions presented in his *Elements*. Second, it reduced the role of historians of mathematics to reconstructing the deductive chains attained in the development of mathematics.

I came to realize that Hempel's claim could not be correct by reading a later publication, also by Hempel; in his *Philosophy of Natural Science* (1966), he presented an elementary proof that leads to the conclusion that deduction cannot be the sole method of mathematics. In particular, he demonstrated that from even a single true statement, an infinity of other true statements can be validly deduced. If we take "or" in the nonexclusive sense and are given a true proposition p, Hempel asserted that we can deduce an infinity of statements of the form "p or q" where q is any proposition whatsoever. Note that all these propositions are true because with the nonexclusive meaning of "or," all propositions of the form "p or q" are true if p is true. As Hempel stated, this example shows that the rules of logical inference provide only tests of the validity of arguments, not methods of discovery.[2] Nor, it is important to note, do they provide guidance as to whether the deduced propositions are in any way significant. Thus we see that an entity, be it man or machine, possessing the deductive rules of inference and a set of axioms from which to start, could generate an infinite number of true conclusions, none of which would be significant. We would not call such results mathematics. Consequently, mathematics as we know it cannot arise solely from deductive methods. A machine given Euclid's definitions, axioms, and postulates might deduce thousands of valid propositions without deriving any Euclidean theorems. Moreover, Hempel's analysis shows that even if definitions, axioms, and postulates could be produced deductively, still mathematics cannot rely solely on deduction. Furthermore, we see from this that historians of mathematics must not confine their efforts to reconstructing deductive chains from the past of mathematics. This is not to deny that deduction plays a major role in mathematical methodology; all I have attempted to show is that it cannot be the sole method of mathematics.

2. Mathematics Provides Certain Knowledge

In the same 1945 essay cited previously, Hempel stated: "The most distinctive characteristic which differentiates mathematics from the various branches of empirical science . . . is no doubt the peculiar certainty and necessity of its results." And, he added: "a mathematical theorem, once proved, is established once and for all. . . ."[3] In noting the certainty of mathematics, Hempel was merely reasserting a view proclaimed for centuries by dozens of authors who frequently cited Euclid's *Elements* as the prime exemplification of that certainty. Writing in 1843, Philip Kelland remarked: "It is certain that from its completeness, uniformity and faultlessness, . . . and from the universal adoption of the completest and best line of argument, Euclid's 'Elements' stand preeminently at the head of all human productions."[4] A careful reading of Hempel's essay reveals a striking feature; immediately after noting the certainty of mathematics, he devoted a section to "The Inadequacy of Euclid's Postulates." Here in Hilbertian fashion, Hempel showed that Euclid's geometry is marred by the fact that it does not contain a number of postulates necessary for proving many of its propositions. Hempel was of course correct; as early as 1892, C. S. Peirce had dramatically summarized a conclusion reached by most late-nineteenth-century mathematicians: "The truth is, that elementary [Euclidean] geometry, instead of being the perfection of human reasoning, is riddled with fallacies. . . ."[5]

What is striking in Hempel's essay is that he seems not to have realized the tension between his claim for the certainty of mathematics and his demonstration that perhaps the most famous exemplar of that certainty contains numerous faulty arguments. Hempel's claim may be construed as containing the implicit assertion that a mathematical system embodies certainty only after all defects have been removed from it. What is problematic is whether we can ever be certain that this has been done. Surely the fact that the inadequacy of some of Euclid's arguments escaped detection for over two millennia suggests that certainty is more elusive than usually assumed. Moreover, in opposition to the belief that certainty can be secured for formalized mathematical systems, Reuben Hersh has stated: "It is just not the case that a doubtful proof would become certain by being formalized. On the contrary, the doubtfulness of the proof would then be replaced by the doubtfulness of the coding and programming."[6] Morris Kline has recently presented a powerful demonstration that the certainty purportedly present throughout the development of mathematics is an illusion; I refer to his *Mathematics: The Loss of Certainty*, in which he states: "The hope of finding objective, infallible laws and standards has faded. The Age of Reason is gone."[7] Much in what follows sheds further light on the purported certainty of mathematics, but let us now proceed to two related claims.

3. Mathematics Is Cumulative

An elegant formulation of the claim for the cumulative character of mathematics is due to Hermann Hankel, who wrote: "In most sciences one generation tears down what another has built and what one has established another undoes. In Mathematics alone each generation builds a new story to the old structure."[8] Pierre Duhem made a similar claim: "Physics does not progress as does geometry, which adds new final and indisputable propositions to the final and indisputable propositions it already possessed. . . ."[9] The most frequently cited illustration of the cumulative character of mathematics is non-Euclidean geometry. Consider William Kingdon Clifford's statement: "What Vesalius was to Galen, what Copernicus was to Ptolemy, that was Lobatchewsky to Euclid."[10] Clifford's claim cannot, however, be quite correct; whereas acceptance of Vesalius entailed rejection of Galen, whereas adoption of Copernicus led to abandonment of Ptolemy, Lobachevsky did not refute Euclid; rather he revealed that another geometry is possible. Although this instance illustrates the remarkable degree to which mathematics is cumulative, other cases exhibit opposing patterns of development. As I wrote my *History of Vector Analysis*,[11] I realized that I was also, in effect, writing *The Decline of the Quaternion System*. Massive areas of mathematics have, for all practical purposes, been abandoned. The nineteenth-century mathematicians who extended two millennia of research on conic section theory have now been forgotten; invariant theory, so popular in the nineteenth century, fell from favor.[12] Of the hundreds of proofs of the Pythagorean theorem, nearly all are now nothing more than curiosities.[13] In short, although many previous areas, proofs, and concepts in mathematics have persisted, others are now abandoned. Scattered over the landscape of the past of mathematics are numerous citadels, once proudly erected, but which, although never attacked, are now left unoccupied by active mathematicians.

4. Mathematical Statements Are Invariably Correct

The most challenging aspect of the question of the cumulative character of mathematics concerns whether mathematical assertions are ever refuted.

The previously cited quotations from Hankel and Duhem typify the widespread belief that Joseph Fourier expressed in 1822 by stating that mathematics "is formed slowly, but it preserves every principle it has once acquired. . . ."[14] Although mathematicians may lose interest in a particular principle, proof, or problem solution, although more elegant ways of formulating them may be found, nonetheless they purportedly remain. Influenced by this belief, I stated in a 1975 paper that "Revolutions never occur in mathematics."[15] In making this claim, I added two important qualifications: the first of these was the "minimal stipulation that a necessary characteristic of a revolution is that some previously existing entity (be it king, constitution, or theory) must be overthrown and irrevocably discarded"; second, I stressed the significance of the phrase "in mathematics," urging that although "revolutions may occur in mathematical nomenclature, symbolism, metamathematics, [and] methodology . . . ," they do not occur *within* mathematics itself.[16] In making that claim concerning revolutions, I was influenced by the widespread belief that mathematical statements and proofs have invariably been correct. I was first led to question this belief by reading Imre Lakatos's brilliant *Proofs and Refutations*, which contains a history of Euler's claim that for polyhedra $V\text{-}E + F = 2$, where V is the number of vertices, E the number of edges, and F the number of faces.[17] Lakatos showed not only that Euler's claim was repeatedly falsified, but also that published proofs for it were on many occasions found to be flawed. Lakatos's history also displayed the rich repertoire of techniques mathematicians possess for rescuing theorems from refutations.

Whereas Lakatos had focused on a single area, Philip J. Davis took a broader view when in 1972 he listed an array of errors in mathematics that he had encountered.[18] Philip Kitcher, in his recent *Nature of Mathematical Knowledge*, has also discussed this issue, noting numerous errors, especially from the history of analysis.[19] Morris Kline called attention to many faulty mathematical claims and proofs in his *Mathematics: The Loss of Certainty*. For example, he noted that Ampère in 1806 proved that every function is differentiable at every point where it is continuous, and that Lacroix, Bertrand, and others also provided proofs until Weierstrass dramatically demonstrated the existence of functions that are everywhere continuous but nowhere differentiable.[20] In studying the history of complex numbers, Ernest Nagel found that such mathematicians as Cardan, Simson, Playfair, and Frend denied their existence.[21] Moreover, Maurice Lecat in a 1935 book listed nearly 500 errors published by over 300 mathematicians.[22] On the other hand, René Thom has asserted: "There is no case in the history of mathematics where the mistake of one man has thrown the entire field on the wrong track.... Never has a significant error slipped into a conclusion without almost immediately being discovered."[23] Even if Thom's claim is correct, the quotations from Duhem and Fourier seem difficult to reconcile with the information cited above concerning cases in which concepts and conjectures, principles and proofs *within* mathematics have been rejected.

5. The Structure of Mathematics Accurately Reflects Its History

In recent years, I have been teaching a course for humanities students that begins with a careful reading of Book I of Euclid's *Elements*. That experience has convinced me that the most crucial misconception that students have about mathematics is that its structure accurately reflects its history. Almost invariably, the students read this text in light of the assumption that the deductive progression from its opening definitions, postulates, and common notions through its forty-eight propositions accurately reflects the development of Euclid's thought. Their conviction in this regard is reinforced by the fact that most of them have earlier read Aristotle's *Posterior Analytics*, in which that great philosopher specified that for a valid demonstration "the premises . . . must be . . . better known than and prior to the conclusion. . . ."[24] My own conception is that the development of Euclid's thought was drastically different. Isn't it plausible that in composing Book I of the *Elements*, Euclid began not with his definitions, postulates, and common notions but rather either with his extremely powerful 45th proposition, which shows how to reduce areas bounded by straight lines to a cluster of measurable triangular areas, or with his magnificent 47th proposition, the Pythagorean theorem, for which he forged a proof that has been admired for centuries. Were not these two propositions the ones he knew best and of which he was most deeply convinced? Isn't it reasonable to assume that it was only after Euclid had decided on these propositions as the culmination for his first book that he set out to construct the deductive chains that support them? Is it probable that Euclid began his efforts with his sometimes abstruse and arbitrary definitions—"a point is that which has no parts"—and somehow arrived forty-seven propositions later at a result known to the Babylonians fifteen centuries earlier? An examination of Euclid's 45th and 47th propositions shows that they depend upon the proposition that if two coplanar straight lines meet at a point and make an angle with each other equal to two right angles, then those lines are collinear. Should it be seen as a remarkable coincidence that thirty-one propositions earlier Euclid had proved precisely this result, but had not used it a single time in the intervening propositions? It seems to me that accepting the claim that the history and deductive structures of mathematical systems are identical is comparable to believing that Saccheri was surprised when after proving dozens of propositions, he finally concluded that he had established the parallel postulate.

Is not the axiomatization of a field frequently one of the last stages, rather than the first, in its development? Recall that it took Whitehead and Russell 362 pages of their *Principia Mathematica* to prove that $1 + 1 = 2$. Calculus texts open with a formulation of the limit concept, which took two centuries to develop. Geometry books begin with primary notions and definitions with which Hilbert climaxed two millennia of searching. Second-grade students encounter sets as well as the associative and commutative laws—all hard-won attainments of the nineteenth century. If these students are gifted and diligent, they may years later be able to comprehend some of the esoteric theorems advanced by Archimedes or Apollonius. When Cauchy established the fundamental theorem of the calculus, that subject was nearly two centuries old; when Gauss proved the fundamental theorem of algebra, he climaxed more than two millennia of advancement in that area.[25] In teaching complex numbers, we first justify them in terms of ordered couples of real numbers, a creation of the 1830s. After they have magically appeared from this process, we develop them to the point of attaining, say, Demoive's theorem, which came a century before the Hamilton-Bolyai ordered-couple justification of them. In presenting a theorem, first we name it and state it precisely so as to exclude the exceptions it has encountered in the years since its first formulation; then we prove it; and, finally, we employ it to prove results that were probably known long before its discovery. In short, we reverse history. Hamilton created quaternions in 1843 and simultaneously supplied a formal justification for them, this being the first case in which a number system was discovered and justified at the same time; half a century later Gibbs and Heaviside, viewing the quaternion method of space analysis as unsatisfactory, proposed a simpler system derived from quaternions by a process now largely forgotten.

Do not misunderstand: I am not claiming that the structure of mathematics, as a whole or in its parts, is in every case the opposite of its history. Rather I am suggesting that the view that students frequently have, implicitly or explicitly, that the structure in which they encounter areas of mathematics is an adequate approximation of its history, is seriously defective. Mathematics is often compared to a tree, ever attaining new heights. The latter feature is certainly present, but mathematics also grows in root and trunk; it develops as a whole. To take another metaphor, the mathematical research frontier is frequently found to lie not at some remote and unexplored region, but in the very midst of the mathematical domain. Mathematics is often compared to art; yet reflect for a moment. Homer's *Odyssey*, Da Vinci's *Mona Lisa*, and Beethoven's Fifth Symphony are completed works, which no later artist dare alter. Nonetheless, the latest expert on analysis works alongside Leibniz and Newton in ordering the area they created; a new Ph.D. in number theory joins Euclid, Fermat, and Gauss in perfecting knowledge of the primes. Kelvin called Fourier's *Théorie analytique de la chaleur* a "mathematical poem,"[26] but many authors shaped its verses. Why did some mathematicians oppose introduction of complex or transfinite numbers, charging that they conflicted with the foundations of mathematics? Part of the reason is that, lacking a historical sense, they failed to see that foundations are themselves open to alteration, that not only premises but results dictate what is desirable in mathematics.

6. Mathematical Proof in Unproblematic

Pierre Duhem in his *Aim and Structure of Physical Theory* reiterated the widely held view that there is nothing problematic in mathematical proof by stating that geometry "grows by the continual contribution of a new theorem demonstrated once and for all and added to theorems already demonstrated. . . ."[27] In short, Duhem was claiming that once a proposition has been demonstrated, it remains true for all time. Various authors, both before and after Duhem, have taken a less absolutist view of the nature and conclusiveness of proof. In 1739, David Hume observed:

> There is no . . . Mathematician so expert . . . as to place entire confidence in any truth immediately upon his discovery of it, or regard it as any thing, but a mere probability. Every time he runs over his proofs, his confidence encreases; but still more by the approbation of his friends; and is rais'd to its utmost perfection by the universal assent and applauses of the learned world."[28]

G. H. Hardy concluded in a 1929 paper entitled "Mathematical Proof" that "If we were to push it to its extreme, we should be led to rather a paradoxical conclusion: that there is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but *point*; that proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology...."[29] E. T. Bell in a number of his writings developed the point that standards of proof have changed dramatically throughout history. For example, in his *Development of Mathematics* (1940), he challenged the assertion of an unnamed "eminent scholar of Greek mathematics" that the Greeks, by their "'unerring logic,' had attained such perfect mathematical results that 'there has been no need to reconstruct, still less to reject as unsound, any essential part of their doctrine....'" Bell responded that among, for example, Euclid's proofs, "many have been demolished in detail, and it would be easy to destroy more were it worth the trouble."[30] Raymond Wilder, who also discussed the process of proof in various writings, asserted in 1944 that "we don't possess, and probably will never possess, any standard of proof that is independent of the time, the thing to be proved, or the person or school of thought using it." Over three decades later, he put this point most succinctly: "'proof' in mathematics is a culturally determined, relative matter."[31]

That research in history and philosophy of mathematics has contributed far more toward understanding the nature of proof than simply showing that standards of proof have repeatedly changed can be illustrated by briefly examining the relevant writings of Imre Lakatos. In his *Proofs and Refutations* (1963-64), Lakatos, proceeding from his conviction (derived from Karl Popper) that conjectures play a vital role in the development of mathematics and his hope (derived from George Pólya) that heuristic methods for mathematics can be formulated, reconstructed the history of Euler's conjecture concerning polyhedra so as to show that its history ill accords with the traditional accumulationist historiography of mathematics. Whereas some had seen its history as encompassing little more than Euler's formulation of the conjecture and Poincaré's later proof for it, Lakatos showed that numerous "proofs" had been advanced in the interim, each being falsified by counterexamples. Fundamental to this essay is Lakatos's definition of proof as "*a thought-experiment—or 'quasi-experiment'—which suggests a decomposition of the original conjecture into subconjectures or lemmas,* thus *embedding it* in a possibly quite distant body of knowledge."[32] On this basis, Lakatos, in opposition to the belief that the proof or refutation of a mathematical claim is final, argued forcefully that on the one hand mathematicians should seek counterexamples to proved theorems (pp. 50ff.) and on the other hand be cautious in abandoning refuted theorems (pp. 13ff.). Moreover, he warned of the dangers involved in recourse, if counterexamples are found, to the techniques he called "monster-barring," "monster-adjustment," and "exception-barring" (pp. 14-33). Lakatos also wrote other papers relevant to the nature of mathematical proof; for example, in his "Infinite Regress and the Foundations of Mathematics" (1962), he provided insightful critiques of the "Euclidean programme" as well as of the formalist conception of mathematical method. In his "A Renaissance of Empiricism in Recent Philosophy of Mathematics" (1967), he stressed the importance of empirical considerations in mathematical proof, while in his "Cauchy and the Continuum . . . ," he urged that Abraham Robinson's methods of nonstandard analysis could be used to provide a radically new interpretation of the role of infinitesimals in the creation of the calculus.[33] The sometimes enigmatic character of Lakatos's writings and the fact that his interests shifted in the late 1960s toward the history and philosophy of science—to which he contributed a "methodology of scientific research programmes"—left, after his death in 1974, many unanswered questions about his views on mathematics. Various authors have attempted to systematize his thought in this regard,[34] and Michael Hallett has advanced and historically illustrated the thesis that "mathematical theories *can* be appraised by criteria like those of [Lakatos's] methodology of scientific research programmes. . . ."[35]

7. Standards of Rigor Are Unchanging

Writing in 1873, the Oxford mathematician H. J. S. Smith repeated a conclusion often voiced in earlier centuries; Smith stated: "The methods of Euclid are, by almost universal consent, unexceptionable in point of rigour."[36] By the beginning of the present century, Smith's claim concerning Euclid's "perfect rigorousness" could no longer be sustained. In his *Value of Science* (1905), Henri Poincaré asked: "Have we finally attained absolute rigor? At each stage of the evolution our fathers . . . thought they had reached it. If they deceived themselves, do we not likewise cheat ourselves?" Surprisingly, Poincaré went on to assert that "in the analysis of today, when one cares to take the trouble to be rigorous, there can be nothing but syllogisms or appeals to this intuition of pure number, the only intuition which can not deceive us. It may be said that today absolute rigor is attained."[37] More recently, Morris Kline remarked: "No proof is final. New counterexamples undermine old proofs. The proofs are then revised and mistakenly considered proven for all time. But history tells us that this merely means that the time has not yet come for a critical examination of the proof."[38]

Not only do standards of rigor intensify, they also change in nature; whereas in 1700 geometry was viewed as providing the paradigm for such standards, by the late nineteenth century arithmetic-algebraic considerations had assumed primacy, with these eventually giving way to standards formulated in terms of set theory. Both these points, as well as a number of others relating to rigor, have been discussed with unusual sensitivity by Philip Kitcher. For example, in opposition to the traditional view that rigor should always be given primacy, Kitcher has suggested in his essay "Mathematical Rigor—Who Needs It?" the following answer: "Some mathematicians at some times, but by no means all mathematicians at all times."[39] What has struck me most forcefully about the position Kitcher developed concerning rigor in that paper and in his *Nature of Mathematical Knowledge* are its implications for the historiography of mathematics. I recall being puzzled some years ago while studying the history of complex numbers by the terms that practitioners of that most rational discipline, mathematics, used for these numbers. Whereas their inventor Cardan called them "sophistic," Napier, Girard, Descartes, Huygens, and Euler respectively branded them "nonsense," "inexplicable," "imaginary," "incomprehensible," and "impossible." Even more mysteriously, it seemed, most of these mathematicians, despite the invective implied in thus naming these numbers, did not hesitate to use them. As Ernest Nagel observed, "for a long time no one could defend the 'imaginary numbers' with any plausibility, except on the logically inadequate ground of their mathematical usefulness." He added: "Nonetheless, mathematicians who refused to banish them . . . were not fools . . . as subsequent events showed."[40]

What I understand Kitcher to be suggesting is that the apparent irrationality of the disregard for rigor found in the pre-1830 history of both complex numbers and the calculus is largely a product of unhistorical, present-centered conceptions of mathematics. In particular, if one recognizes that need for rigor is a relative value that may be and has at times been *rationally* set aside in favor of such other values as usefulness, then one will be less ready to describe various periods of mathematics as ages of unreason and more prone to undertake the properly historical task of understanding why mathematicians adopted such entities as "impossible numbers" or infinitesimals. As Kitcher suggests, it may be wise for historians of mathematics to follow the lead of historians of science who long ago became suspicious of philosophic and historiographic systems that entail the reconstruction of scientific controversies in terms of such categories as irrationality, illogicality, and stubbornness.[41]

8. The Methodology of Mathematics Is Radically Different from the Methodology of Science

The quotations previously cited from Duhem's *Aim and Structure of Physical Theory* illustrate a subtheme running through that book: that the methodology of mathematics differs greatly from that of physics. In other passages, Duhem lamented that physics had not achieved "a growth as calm and as regular as that of mathematics" (p. 10) and that physics, unlike mathematics, possesses few ideas that appear "clear, pure, and simple" (p. 266). Moreover, largely because Duhem believed that "these two methods reveal themselves to be profoundly different" (p. 265), he concluded that, whereas history of physics contributes importantly to understanding physics, "The history of mathematics is, [although] a legitimate object of curiosity, not essential to the understanding of mathematics" (p. 269). The position developed in this and the next section is that important parallels exist between the methods employed in mathematics and in physics.

The first author who explicitly described the method that, according to most contemporary philosophers of science, characterizes physics was Christiaan Huygens. He prefaced his *Treatise on Light* by stating that in presenting his theory of light he had relied upon "demonstrations of those kinds which do not produce as great a certitude as those of Geometry, and which even differ much therefrom, since whereas the Geometers prove their Propositions by fixed and incontestable Principles, here the Principles are verified by the conclusions to be drawn from them. . . ."[42] What I wish to suggest is that, to a far greater extent than is commonly realized, mathematicians have employed precisely the same method—the so-called hypothetico-deductive method. Whereas the pretense is that mathematical axioms justify the conclusions drawn from them, the reality is that to a large extent mathematicians have accepted axiom systems on the basis of the ability of those axioms to bring order and intelligibility to a field and/or to generate interesting and fruitful conclusions. In an important sense, what legitimized the calculus in the eyes of its creators was that by means of its methods they attained conclusions that were recognized as correct and meaningful. Although Hamilton, Grassmann, and Cantor, to name but a few, presented the new systems for which they are now famous in the context of particular philosophies of mathematics (now largely discarded), what above all justified their new creations, both in their own eyes and among their contemporaries, were the conclusions drawn from them. This should not be misunderstood; I am not urging that only utilitarian criteria have determined the acceptability of mathmatical systems, although usefulness has undoubtedly been important. Rather I am claiming that characteristics of the results attained—for example, their intelligibility—have played a major role in determining the acceptability of the source from which the results were deduced. To put it differently, calculus, complex numbers, non-Euclidean geometries, etc., were in a sense hypotheses that mathematicians subjected to test in ways comparable in logical form to those used by physicists.

My claim that mathematicians have repeatedly employed the hypothetico-deductive method is not original; a number of recent authors have made essentially the same suggestion. Hilary Putnam began a 1975 paper by asking how we would react to finding that Martian mathematicians employ a methodology that, although using full-blown proofs when possible, also relies upon quasi-empirical tests; for example, his Martians accept the four-color conjecture because much empirical evidence supports and none contradicts it. Putnam proceeded to claim that we should not see this as resulting from some bizarre misunderstanding of the nature of mathematics; in fact, he asserted that "we have been using quasi-empirical and even empirical methods in mathematics all along. . . ."[43] The first example he used to illustrate this claim is Descartes's creation of analytical geometry, which depends upon the possibility of a one-to-one correspondence between the real numbers and the points on a line. The fact that no justification for this correspondence, let alone for the real numbers, was available in Descartes's day did not deter him or his contemporaries; they proceeded confidently ahead. As Putnam commented on his Descartes illustration: "This is as much an example of the use of hypothetico-deductive methods as anything in physics is" (p. 65). Philip Kitcher, who has stressed the parallels between the evolution of mathematics and of science, has advocated a similar view. In 1981, he stated:

> Although we can sometimes present parts of mathematics in axiomatic form, . . . the statements taken as axioms usually lack the epistemological features which [deductivists] attribute to first principles. Our knowledge of the axioms is frequently less certain than our knowledge of the statements we derive from them. . . . In fact, our knowledge of the axioms is sometimes obtained by nondeductive inference from knowledge of the theorems they are used to systematize.[44]

Finally, statements urging that mathematical systems are, like scientific systems, tested by their results occur in the writings of Haskell Curry, Willard Van Orman Quine, and Kurt Gödel.[45]

9. Mathematical Claims Admit of Decisive Falsification

In the most widely acclaimed section of his *Aim and Structure of Physical Theory*, Duhem attacked the view that crucial experiments are possible in physics. He stated: "Unlike the reduction to absurdity [method] employed by geometers, experimental contradiction does not have the power to transform a physical hypothesis into an indisputable truth" (p. 190). The chief reason he cited for this inability is that a supposed crucial experiment can at most decide "between two sets of theories each of which has to be taken as a whole, i.e., between two entire systems . . ." (p. 189). Because physical theories can be tested only in clusters, the physicist, when faced with a contradiction, can, according to Duhem, save a particular theory by modifying one or more elements in the cluster, leaving the particular theory of most concern (for example, the wave or particle theory) intact. In effect, Duhem was stating that individual physical theories can always be rescued from apparent refutations. Having criticized a number of Duhemian claims, I wish now to pay tribute to him by urging that a comparable analysis be applied to mathematics. In particular, I suggest that in history of mathematics one frequently encounters cases in which a mathematical claim, faced with an apparent logical falsification, has been rescued by modifying some other aspect of the system. In other words, mathematical assertions are usually not tested in isolation but in conjunction with other elements in the system.

Let us consider some examples. Euclid brought his *Elements* to a conclusion with his celebrated theorem that "no other figure, besides [the five regular solids], can be constructed which is contained by equilateral and equiangular figures equal to one another." How would Euclid respond if presented with a contradiction to this theorem—for example, with a hexahedron formed by placing two regular tetrahedra face to face? It seems indisputable that rather than rejecting his theorem, he would rescue it by revising his definition of regular solid so as to exclude polyhedra possessing noncongruent vertices. For centuries, complex numbers were beset with contradictions; some charged that they were contradicted by the rules that every number must be less than, greater than, or equal to zero and that the square of any number be positive. Moreover, others urged that no geometric interpretation of them is possible.[46] Complex numbers survived such attacks, whereas the cited rules and the traditional definition of number did not. Many additional cases can be found; in fact, Lakatos's *Proofs and Refutations* is rich in examples of refutations that were themselves rejected. Of course, mathematicians do at times choose to declare apparent logical contradictions to be actual refutations; nonetheless, an element of choice seems present in many such cases.

10. In Specifying the Methodology Used in Mathematics, the Choices Are Empiricism, Formalism, Intuitionism, and Platonism

For decades, mathematicians, philosophers, and historians have described the alternative positions concerning the methodology of mathematics as empiricism, formalism, intuitionism, and Platonism. This delineation of the options seems ill-conceived in at least two ways. First, it tends to blur the distinction between the epistemology and methodology of mathematics. Although related in a number of complex ways, the two areas can and should be distinguished. Epistemology of mathematics deals with how mathematical knowledge is possible, whereas methodology of mathematics focuses on what methods are used in mathematics. The appropriateness of distinguishing between these two areas is supported by the fact that the history of the philosophy of mathematics reveals that different epistemological positions have frequently incorporated many of the same methodological claims. Second, this fourfold characterization tends to blur the distinction between normative and descriptive claims. To ask what methods mathematicians should use is certainly different from asking what methods they have in fact used. Failure to recognize this distinction leads not only to the so-called naturalistic fallacy—the practice of inferring from "is" to "ought"—but also to the unnamed opposite fallacy of inferring from "ought" to "is" (or to "was").

It has been my experience that both mathematicians and historians of mathematics are primarily interested in issues of the methodology rather than of the epistemology of mathematics. When they turn to writings in the philosophy of mathematics, they usually find these composed largely in terms of one or more of the four primarily epistemological positions mentioned previously. Unfortunately, these categories seem relatively unilluminating in exploring methodological issues. Reuben Hersh very effectively discussed this problem in a 1979 paper in which he asked: "Do we really have to choose between a formalism that is falsified by our everyday experience, and a Platonism that postulates a mythical fairyland where the uncountable and the inaccessible lie waiting to be observed. . . ?" Hersh proposed a different and more modest program for those of us interested in investigating the nature of mathematics; he suggested that we attempt "to give an account of mathematical knowledge as it really is—fallible, corrigible, tentative and evolving. . . . That is, reflect honestly on what we do when we use, teach, invent or discover mathematics—by studying history, by introspection, and by observing ourselves and each other. . . ."[47] If Hersh's proposal is taken seriously, new categories will probably emerge in the philosophy and historiography of mathematics, and these categories should prove more interesting and illuminating than the traditional ones. Moreover, increased study of the descriptive methodology of mathematics should itself shed light on epistemological issues. This paper not only concludes with an endorsement of Hersh's proposed program of research, but has been designed to serve as an exemplification of it.

Notes

1. Carl G. Hempel, "Geometry and Empirical Science," in *Readings in the Philosophy of Science*, ed. Philip P. Wiener (New York: Appleton-Century-Crofts, 1953), p. 41; reprinted from *American Mathematical Monthly* 52 (1945): 7-17.
2. Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966) pp. 16-17.
3. Hempel, "Geometry," pp. 40-41.
4. As quoted in *On Mathematics and Mathematicians*, ed. Robert Edouard Moritz (New York: Dover, 1942), p. 295.
5. Charles S. Peirce, "The Non-Euclidean Geometry," in *Collected Papers of Charles Sanders Peirce*, vol. 8, ed. Arthur W. Burks (Cambridge, Mass.: Harvard University Press, 1966), p. 72.
6. Reuben Hersh, "Some Proposals for Reviving the Philosophy of Mathematics," *Advances in Mathematics* 31 (1979): 43.
7. Morris Kline, *Mathematics: The Loss of Certainty* (New York: Oxford University Press, 1980), p. 7.
8. As quoted in Moritz, *Mathematics*, p. 14.
9. Pierre Duhem, *The Aim and Structure of Physical Theory*, trans. Philip P. Wiener

(Princeton, N.J.: Princeton University Press, 1954), p.177.
10. As quoted in Moritz, *Mathematics*, p. 354.
11. Michael J. Crowe, *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System* (Notre Dame, Ind.: University of Notre Dame Press, 1967); reprinted New York: Dover Publications, 1985.
12. Charles S. Fisher, "The Death of a Mathematical Theory: A Study in the Sociology of Knowledge," *Archive for the History of the Exact Sciences* 3 (1966-67): 137-59.
13. For a compilation of 362 proofs, see Elisha Scott Loomis, *The Pythagorean Proposition* (Ann Arbor, Mich.: National Council of Teachers of Mathematics, 1940).
14. Joseph Fourier, *Analytical Theory of Heat*, trans. Alexander Freeman (New York: Dover, 1955), p. 7.
15. Michael J. Crowe, "Ten 'Laws' Concerning Patterns of Change in the History of Mathematics," *Historia Mathematica* 2 (1975): 161-66. For discussions of my claim concerning revolutions, see I. Bernard Cohen, *Revolution in Science* (Cambridge, Mass.: Harvard University Press, 1985) pp. 489-91, 505-7; Joseph Dauben, "Conceptual Revolutions and the History of Mathematics," in *Transformation and Tradition in the Sciences*, ed. Everett Mendelsohn (Cambridge: Cambridge University Press, 1984), pp. 81-103; Herbert Mehrtens, "T. S. Kuhn's Theories and Mathematics: A Discussion Paper on the 'New Historiography' of Mathematics," *Historia Mathematica* 3 (1976): 297-320; and Raymond L. Wilder, *Mathematics as a Cultural System* (Oxford: Pergamon, 1981), pp. 142-44.
16. Crowe, "Patterns," pp. 165-66.
17. Imre Lakatos, *Proofs and Refutations: The Logic of Mathematical Discovery*, ed. John Worrall and Elie Zahar (Cambridge: Cambridge University Press, 1976). This study first appeared in 1963-64 in *British Journal for the Philosophy of Science*.
18. Philip J. Davis, "Fidelity in Mathematical Discourse: Is One and One Really Two?" in *American Mathematical Monthly* 79 (1972): 252-63; see esp. pp. 260-62.
19. Philip Kitcher, *The Nature of Mathematical Knowledge* (New York: Oxford University Press, 1983), pp.155-61, 178-85, 236-68
20. Kline, *Mathematics*, pp. 161, 177.
21. Ernest Nagel, "'Impossible Numbers': A Chapter in the History of Modern Logic," *Studies in the History of Ideas* 3 (1935): 427-74; reprinted in Nagel's *Teleology Revisited* (New York: Columbia University Press, 1979), pp.166-94.
22. Maurice Lecat, *Erreurs de mathématiciens des origines à nos jours* (Brussels: Castaigne, 1935), p. viii.
23. René Thom, "'Modern Mathematics': An Educational and Philosophic Error?" *American Scientist* 59 (1971): 695-99.
24. Aristotle, *Posterior Analytics*, in *The Basic Works of Aristotle*, ed. Richard McKeon (New York: Random House, 1941), book 1, chap. 2, lines 20-22. In the same chapter, Aristotle admitted that "prior" can be understood in two senses, but he apparently saw no significance in this fact.
25. Morris Kline, *Mathematical Thought from Ancient to Modern Times* (New York: Oxford University Press, 1972), pp. 958, 595.
26. As quoted in Silvanus P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, vol. 2 (London: Macmillan, 1910), p. 1139.
27. Duhem, *Aim*, p. 204.
28. David Hume, *Treatise of Human Nature*, ed. Ernest C. Mossner (Baltimore: Penguin, 1969), p. 231.
29. G. H. Hardy, "Mathematical Proof," *Mind* 38 (1929): 18.
30. Eric Temple Bell, *The Development of Mathematics*, 2d ed. (New York: McGraw-Hill, 1945), p. 10. See also Bell's "The Place of Rigor in Mathematics," *American Mathematical Monthly* 41 (1934): 599-607.
31. Raymond L. Wilder, "The Nature of Mathematical Proof," *American Mathematical Monthly* 51 (1944): 319. See Also Wilder, *Mathematics*, e.g. pp. 39-41, and his "Relativity of Standards of Mathematical Rigor," *Dictionary of the History of Ideas*, ed. Philip P. Wiener, vol. 3 (New York: Charles Scribner's, 1973), pp. 170-77.

32. Lakatos, *Proofs*, p. 9.

33. Imre Lakatos, "Infinite Regress and the Foundations of Mathematics," "A Renaissance of Empiricism in Recent Philosophy of Mathematics," and "Cauchy and the Continuum: The Significance of the Non-Standard Analysis for the History and Philosophy of Mathematics," in Lakatos's *Mathematics, Science and Epistemology. Philosophical Papers*, vol. 2, ed. John Worrall and Gregory Currie (Cambridge: Cambridge University Press, 1978), pp. 3-23, 24-42, 43-60.

34. See, for example, Hugh Lehman, "An Examination of Imre Lakatos' Philosophy of Mathematics," *Philosophical Forum* 12 (1980): 33-48; Peggy Marchi, "Mathematics as a Critical Enterprise," in *Essays in Memory of Imre Lakatos*, ed. R. S. Cohen, P. K. Feyerabend, and M. W. Wartofsky (Dordrecht and Boston: Reidel, 1976), pp. 379-93.

35. Michael Hallett, "Towards a Theory of Mathematical Research Programmes," *British Journal for the Philosophy of Science* 30 (1979): 1-25, 135-59.

36. H. J. S. Smith, "Opening Address by the President...," *Nature* 8 (25 Sept. 1873): 450. Smith in that year was president of Section A of the British Association for the Advancement of Science.

37. Henri Poincaré, *The Value of Science*, trans. George Bruce Halsted (New York: Dover, 1958), pp. 19-20.

38. Kline, *Mathematics*, p. 313; see also his *Why Johnny Can't Add* (New York: Vintage, 1974), p. 69.

39. Philip Kitcher, "Mathematical Rigor—Who Needs It?" *Nous* 15 (1980): 490.

40. Nagel, "Numbers," pp. 435, 437.

41. Kitcher, *Mathematical Knowledge*, pp. 155-58. For his "rational reconstruction" of the history of calculus, see chap. 10.

42. Christiaan Huygens, *Treatise on Light*, trans. Silvanus P. Thompson (New York: Dover, n.d.), p. vi.

43. Hilary Putnam, "What Is Mathematical Truth?" in Putnam, *Mathematics, Matter and Method. Philosophical Papers*, vol. 1, 2d ed. (Cambridge: Cambridge University Press, 1979), p. 64.

44. Kitcher, "Rigor," p. 471; see also Kitcher, *Mathematical Knowledge*, pp. 217-24, 271.

45. See quotations in Kline, *Mathematics*, pp. 330-31.

46. Nagel, "Numbers," pp. 434, 437, 441.

47. Hersh, "Proposals," p. 43. See also Philip J. Davis and Reuben Hersh, *The Mathematical Experience* (Boston: Birkhäuser, 1981), p. 406.