Philosophy Enters the Mathematics Classroom

PAUL WOLFSON

I Joseph Agassi's recent article, "On Mathematics Education: The Lakatosian Revolution", which appeared in this journal [1], is a call to arms It asks like-minded teachers to initiate radical change in our educational system Since others, too, have been impressed by Lakatos's writings, there is some chance that Agassi will be heeded and a reform movement will be started. It is therefore worthwhile for us to examine the ideas of Lakatos from many points of view.

What is clear from even a casual reading of *Proofs and Refutations*, his book most known to mathematicians, is that Lakatos finds stultifying that system of mathematical exposition loosely based upon the formalist philosophy of mathematics. Textbooks from this mold, he tells us, rigidly present a kind of fiction: the perfect, logically founded system In fact, he goes on, systems like that do not exist (not *interesting* ones, anyway), but by perpetuating the myth, expositors cover up the active nature of real mathematics. To see mathematics more fully, he concludes, we must view the subject historically

As an example, Lakatos considers Rudin's *Principles of Mathematical Analysis*, first edition [23] This, says Lakatos [17, p 145], "is one of the best textbooks within this tradition", but inevitably, since it is written in the "authoritarian deductivist style", it has weaknesses For example, when Rudin introduces functions of bounded variation, we might well ask, "Why should we be interested in just this set of functions? The deductivist's answer is: 'Wait and see'." The answer comes later, in the theorem that such functions are Riemann-Stieltjes integrable. But since the Riemann-Stieltjes integral is introduced in the same authoritarian manner, "now we have got a theorem in which two mystical (sic) concepts, bounded variation and Riemann-integrability, occur. But two mysteries do not add up to understanding" [p. 147]

Many readers will go so far with Lakatos. If he was ahead of his time in 1963 when he published the first portions of *Proofs and Refutations*, by now the mathematical community has caught up with him somewhat. Dissatisfaction with a formalist philosophy of mathematics seems rather widespread [7, 25, 26], and the expositions it inspired no longer hold a monopoly. In fact, more attention is being given to history, even to the publishing of serious volumes [2, 3, 13]. (See Note 1)

A respect for history is nothing new, however. Otto Toeplitz's beautiful book, *The Calculus: A Genetic Approach* [27], persuasively illustrates the theory that we will understand a mathematical idea better for knowing its origins and development. George Polya reinforced this same theory with a metaphor ("the biogenetic law") [22] before writing the books on heuristic which inspired Lakatos [20, 21].

The imperfect nature of mathematics, too, had been discussed before Lakatos. In his *Remarks on the Foundation of Mathematics* [30], Ludwig Wittgenstein wrote [p. 171 e]:

> What does mathematics need a foundation for? It no more needs one, I believe, than propositions about physical objects — or about sense impressions, need an *analysis* ...
> The *mathematical* problems of what is called foundations are no more the foundations of mathematics for us then the painted rock is the support of a painted tower.

Lakatos echoed this conclusion in such statements as these:

> By each "revolution of rigour" proof-analysis penetrated deeper into the proofs down to the *foundational layer* of "familiar background knowledge"... where crystal-clear intuition, the rigour of proof, reigned supreme and criticism was banned. Thus, *different levels of rigour differ only about where they draw the line between the rigour of proof-analysis and the rigour of proof, i e. about where criticism should stop and justification should start* "Certainty is never achieved": "foundations" are never found — but the "cunning of reason" turns each increase in rigour into an increase in *content*, in the scope of mathematics. [17, p. 56].

Mathematicians themselves have been aware of imperfections in rigour [4, 19]. Just recently, I. Grattan-Guinness [6] has criticized our mathematical curriculum and textbooks for conveying a false sense of the absoluteness of mathematics, and recommends history as an antidote. His article is worth quoting at length [p 430].

> Whether explicitly or implicitly, teachers usually believe in the solidity of foundations and the correctness of the knowledge built upon them. For centuries this spirit was represented by a dogmatic belief in Euclidean geometry. There one found the apotheosis of mathematics, the paradigm of the Immutable Fact, the tree of secure knowledge, the cathedral of rigour to which all other parishes must look for inspiration and enlightment...
>
> Now one of the principal claims of the New Mathematics is that it has swept this type of thinking away. Nobody believes in the primacy of geometry any more...
>
> My own view is that the old Euclideanism has been replaced by a new one of a different type. Instead of the Immutable Fact, we have the Immutable Structure; the tree of secure knowledge has become the tower of basic artifacts; the church of rigour has been taken over by a Corporation of Fundamental Concepts, whose products we all need in order to think.

For the Learning of Mathematics **2**, 1 (July 1981)
FLM Publishing Association Montreal. Quebec. Canada

Most mathematicians who are occupied with research seem to share this attitude with the (majority of) mathematics teachers, and as Reuben Hersh has noted, that undermines their confidence. " . . We can see the reason for the 'working mathematician's' uneasy oscillation between formalism and Platonism. Our inherited and unexamined philosophical dogma is that mathematical truth should possess absolute certainty" [10, p. 38] Hersh goes on to argue that this unexamined dogma, and mathematical philosophy in general, affects all mathematical activity. Since he is an established mathematician speaking to his colleages, it is especially valuable for us to listen in

> Many practical problems and impasses confronting mathematics today have philosophical aspects. The dearth of well-founded philosophical discourse on mathematics has observable harmful consequences, in teaching, in research, and in the practical affairs of our organizations
>
> Most mathematicians live with two contradictory views on the nature and meaning of their work. Is it credible that this tension has no effect on the self-confidence and self-esteem of people who are supposed above all things to hate contradiction? .
>
> It would be surprising if this had no practical consequences.
>
> Let us pause to consider two possible examples of such practical consequences. The last half century or so has seen the rise of formalism as the most frequently advocated point of view in mathematical philosophy. In this same period, the dominant style of exposition in mathematical journals, and even in texts and treatises, has been to insist on precise details of definitions and proofs, but to exclude or minimize discussion of why a problem is interesting, or why a particular method of proof is used . .
>
> Another example is the importation, during the 60's, of set-theoretic notation and axiomatics into the high school curriculum. This was not an inexplicable aberration, as its critics sometimes seem to imagine. It was a predictable consequence of the philosophic doctrine that reduces all mathematics to axiom systems expressed in set-theoretic language
>
> The criticism of formalism in the high schools has been primarily on pedagogic grounds: "This is the wrong thing to teach, or the wrong way to teach". But all such arguments are inconclusive if they leave unquestioned the dogma that real mathematics *is* precisely formal derivations from formally stated axioms. . . In the end, the critique of formalism can be successful only through the development of an alternative: a more convincing, more satisfactory philosophical account of the meaning and nature of mathematics

II Such an account is what Lakatos tries to give, and in this way he goes beyond some others who also criticized the existing philosophies of mathematics. To understand his ideas, we shall begin with a brief review of *Proof and Refutations*. This work consists largely of a discussion of Euler's conjecture that for any polyhedron, $F + V = E + 2$, where F equals the number of faces, V equals the number of vertices, and E equals the number of edges of the polyhedron. Lakatos's discussion is in the form of a dialogue. This dramatizes his idea that mathematics is a dialectical process in which several steps may be distinguished (Figure 1). When a conjecture has gone through the five stages illustrated, it has closed a loop (or more appropriately, rotated about a spiral); the revised conjecture awaits a new proof.

Stages in Proofs and Refutations	*Examples from the history of Euler's conjecture*
1 Forming a primitive conjecture	$F + V = E + 2$ for all polyhedra
2 Devising a proof	Cauchy's proof by flattening
3 Finding counterexamples	The cube within a cube
4 Analyzing the proof	Discovering the hidden lemma that the polyhedron can be flattened after removing a face
5 Formulating a new conjecture	All *convex* polyhedra satisfy $F + V = E + 2$

Figure 1

Of course, this scheme must not be taken too literally. For one thing, the historical sequence (or one's own sequence of steps in solving a problem) may vary. Again, more than one proof may be attempted, or more than one new conjecture suggested. Thus, there is an important element of choice and judgement (see Note 2); one can make better or worse moves at each stage in the game. There is, then, at least an art of heuristic (in the sense of Polya) and therefore a more active role for the researcher than merely to await inspiration. Correspondingly, the learner and the teacher both have more active roles.

This is where Agassi's reflections on "the Lakatosian Revolution" begin. Now Agassi makes a very bold assertion about Lakatos's view of mathematics: he says that Lakatos rejects both the view that mathematics is knowledge of the truths of nature, and the principal alternative, that mathematics is merely a convention.

> When we come to any branch of learning, but particularly to logic and mathematics, the dichotomy of nature and convention is so dreadful because it cuts out purpose: nature leaves no room for my desires and convention makes them arbitrary. This is why in logic and in mathematics most philosophers are either naturalists, logicists, ideal language theorists, etc., or formalists who deem any axiom system as good as any other. Both are in error: systems are man-made but not arbitrary; they are designed to answer certain desiderata, and these desiderata are themselves subject to debate. [1, p. 29]

That debate and the resulting decisions should themselves be a most important part of education, Agassi believes.

III From this quotation we can see that Agassi, like Lakatos, was writing a polemic. The better the polemicist, the more we should be on our guard. So let us think critically before we join the revolution.

Several problems present themselves when we examine Lakatos's writings as philosophical accounts of mathematics. A first criticism of several of his writings is that they are historically inaccurate. For example, Grattan-Guinness [5, p. 319] criticizes Lakatos's account [15] of the Cauchy conjecture (that the sum of a convergent series of continuous functions is continuous), remarking that "Lakatos's command of the history of this theorem is inadequate."

Lakatos's apparently cavalier treatment of facts may have been encouraged by some systematic biases. One is his leaning toward so-called "Whig History", "reading the past in terms of the present to which it led" [3, p. 328]. One example of this is Lakatos's use of Robinson's infinitesimals to explain Cauchy's conjecture.

In addition to this, as we have seen, Lakatos is partial to a dialectical view of mathematical development. The dialogue form of the bulk of *Proofs and Refutations* helps to emphasize this view, but it leaves little place for accidental influences. What is internal in the history of mathematics may nevertheless be external to some particular mathematical problem. For example, another reviewer criticizes Lakatos's account of the concept of uniform convergence (again in *Proofs and Refutations*):

> Unfortunately, the rational reconstruction of history is much less convincing here than in the case of the Euler conjecture. There are two related reasons for the difference. Lakatos has tried to detach the problem of convergence from the cluster of issues addressed in early 19th century analysis, and the development of ideas on these other issues is relevant to the elaboration of concepts of convergence. Moreover, because Lakatos has not provided an account of how mathematical principles can be justified during the course of theoretical evolution, he is unable to explain the rationality of the process that led to the notion of uniform convergence. [12: 782-783].

If we are to introduce dialogues in our classrooms, then we must realize that they are artificial, not strictly representative of mathematical development. In his article, Agassi remarks that motivation theory "is an advocacy of lies" [p. 30], but if the criticisms cited are valid, then Lakatos, too, systematically misrepresents the truth, and we should at least be wary of exposition based upon his theories and examples.

To these criticisms I must add the admission that it is difficult for me, at least, to find in Lakatos's writings a coherent position on the tension between truth and convention to which Agassi refers in the quotation above. There are times when Lakatos seems to hold something like a Platonic notion of mathematical truth. In other places, his view seems different. For example, in [18] Lakatos distinguishes three types of proof — preformal, formal, and post-formal. "Roughly, the first and third prove something about that sometimes clear and empirical, sometimes vague and "quasi-empirical" stuff, which is the real though rather evasive subject of mathematics. This sort of proof is always liable to some uncertainty on account of hitherto unthought-of possibilities. The second sort of mathematical proof is absolutely reliable; it is a pity that it is not quite certain — although it is approximately certain — what it is reliable about."

IV The criticisms listed above are intended to show where we in the mathematical community need to do more thinking. They are not intended to suggest that we dismiss Lakatos's ideas. We should try to avoid such dismissals. Indeed, Lakatos himself is sometimes guilty of dismissing formalism and other traditional philosophies with scant acknowledgment of their positive features. He says little, too, about the mathematical — as opposed to philosophical — reasons for the rise of the axiomatic method (see Note 3), and its natural and appropriate incorporation in some expositions. What we should reach for is a philosophy of mathematics which can find a place for this tradition and also for a coherent revision of Lakatos's ideas. It seems that we may make a start on this by considering Lakatos's own later writings on the philosophy of science.

Instead of speaking of scientific theories, Lakatos developed the concept of a scientific research program, which is characterized by two parts: (1) its "negative heuristic," certain specific tenets taken as infallible, and (2) its "positive heuristic," a tentative model, that is, a view (subject to modification) of the nature of the subject matter which is under consideration.

> The classical example of a successful research programme is Newton's gravitational theory. When it was first produced, it was submerged in an ocean of "anomalies" (or, if you wish, "counter examples"), and opposed by the observational theories supporting these anomalies. But Newtonians turned, with brilliant tenacity and ingenuity, one counter-instance after another into corroborating instances, primarily by overthrowing the original observational theories in the light of which this "contrary evidence" was established. In the process they themselves produced new counter-examples which they again resolved. [16, p. 133]

Several aspects of a scientific research program may be inferred from this quotation. In the first place, a research program is just that — a program or plan, and therefore unfolding, dynamic, as opposed to the traditional view in which a theory (as usually presented in textbooks — see Note 4) is laid out, static. Furthermore, the reference to counterexamples correctly suggests that a research program unfolds in a process similar to the proofs and refutations outlined above. Thirdly, we note that the Newtonian program resisted collapse even though it "was opposed by the observational theories supporting . . anomalies." This was due to the bolstering effect of irrefutable tenets: Newton's three laws of dynamics and his law of gravitation. These assertions formed the "hard core" of the program, as Lakatos called it, and it indicated what kinds of explanations (e.g. for the anomalies) were unacceptable to the program. Thus it formed a *negative heuristic*.

The hard core points us away from certain explanations; clearly it should be supplemented by a *positive heuristic* which tells us where we *should* look for explanations The positive heuristic takes the form of auxiliary hypotheses or of a model which is introduced to provide explanations, especially for anomalies As one hypothesis may not explain all known anomalies, the hypotheses are allowed to be changed as needed In this way one can hope always to keep a lethal blow from being struck at the irrefutable hard core. Lakatos dubbed this second part of the program its "protective belt", as we might have feared. Figure 2 illustrates the development of the Newtonian research program.

The meaning and validity of the idea of a scientific research program have been debated [see, for example, 5, pp 320-321 and 24, pp. 309-310], but at the very least it is suggestive. As I shall indicate later, the idea of a scientific research program may profitably be taken into mathematics to solve some of the difficulties discussed earlier. In order to see some value in the idea of a scientific research program, we must realize that it is more than just a model. For example Lakatos's account of the Newtonian program presents a list of progressively more sophisticated models of planetary motion. In the first one, a point-like planet moves about a fixed point-like sun, and in this model, with Newton's laws, Kepler's laws of planetary motion can be derived. However, the model itself contradicts Newton's third law, because the planet is not allowed to affect the sun. Thus a second model, in which the planet and the sun move around a common center of gravity, is developed. Here, we may argue that the original model was simply incorrect But at a later stage, point masses are replaced by massive balls. Lakatos accounted for this replacement in the following way [16, p. 135]: "Infinite density was forbidden by an (inarticulated) touchstone of the theory, therefore planets *have* to be extended" But this casual comment does no justice to the change. What happened at this stage was that an idea *was* articulated. Elsewhere [18] Lakatos emphasized how important and surprising such articulations could be Indeed, that is one of the themes of *Proofs and Refutations*, where the idea of a polyhedron, at first a naive one, is articulated with surprising, occasionally troubling, and deep consequences The situation was the same in our examples from the Newtonian program That it was possible to apply Newton's laws to extensive bodies was not at all obvious; Lakatos himself, on the next page of his account, remarked upon it as a cause of the twenty-year delay in publishing the *Principia*. All of this suggests that it would be worthwhile to develop a suitably subtle and flexible form of conventionalism in the philosophies of science and mathematics.

The Newtonian research program

HARD CORE: Newton s three laws of motion and his law of gravitation

PROTECTIVE BELT:

Changing models	*What they explain*
1 Fixed point-like sun and single point-like planet	Kepler's law of elliptical motion
2 Sun and planet move around common center of gravity	Removes contradiction of Newton's third law
3 More planets, but no interplanetary forces	
4 Planets are massive balls	Removes implicit proscription of infinite densities
5 The masses are spinning	
6 The masses are bulging	

Note that interactions among several masses are still not accounted for in this scheme of Lakatos That begins with discussions of the classical "three-body problem"

Figure 2

V Let us see what a theory of mathematical research programs might look like If we can adapt this idea of Lakatos, we shall be able to give a fuller account of the nature of mathematics than was possible under the earlier philosophies. We certainly should be able to offer guidance to historians of our subject. The same insights might also help us to develop courses or even a curriculum that studies long sequences of great mathematical ideas [see 6 and 29]. Lastly, the study of the history of mathematics has been justified by the adage "ontogeny recapitulates phylogeny"; therefore, an organization of historical facts into mathematical research programs should give implicit instruction in heuristic even as it inculcates subject matter.

In two very interesting papers [9], Michael Hallett has discussed several episodes from the history of mathematics (especially the work of Cantor) from the point of view of mathematical research programs. He emphasizes the role of heuristics in "binding series of related theories into programs". Moreover, his examples (from the work of Cantor, Lebesgue, and Poincaré), make clear that he conceives of the heuristics as specific to the programs, not just general rules such as Polya formulated and Lakatos discussed in *Proofs and Refutations* This idea that heuristics are specific to programs seems to me a major advance in Lakatos's later work. Nevertheless, we shall not follow Hallett here, but consider an alternative interpretation which is perhaps closer to Lakatos's own ideas about scientific research programs.

The positive heuristic of a program is probably to be found in the broad models of a problem area. Lakatos himself referred to one [17]: Fourier's primitive conjecture that every function can be expanded in a Fourier series. Hallett [9, pp. 146-149] refers to several models, such as Cantor's program, by which ranges of variability were made into sets which were to be treated as finite wholes and enumerated against a fixed stock of (transfinite) ordinal numbers Again, Felix Klein's Erlangen program was to study a geometric system by studying its group.

We may be tempted, then, to complete our outline by identifying the negative heuristic of a program with the set of axioms and theorems of the resulting theory, but this would be a mistake: the hard core should be not merely irrefutable, but essential. Therefore, in an effort to clarify our notions of both negative and positive heuristic, let us consider one more example.

The early steps in the development of algebraic topology were motivated largely by analysis. Mathematicians observed that integrals of the same function over different paths often yielded equal values. This led to the definition of a new equivalence relation among curves: namely when two curves are homologous. By 1870 Betti had introduced homology groups. Poincaré, in 1895, gave a precise definition of the homology groups via simplicial complexes and cycles, thereby avoiding analytic methods and replacing them with algebraic or combinatorial ones. This entailed triangulating the space to be studied Having done so, however, one would want to know that the constructions based on the triangulation were independent of the *particular* triangulation, i e. were invariant, so that the results were *topological* properties of the space. A more precise and specialized statement emerged: any two triangulations of a space have respective subdivisions that are isomorphic. The central role of this conjecture in early work is reflected in the name given to it, *hauptvermutung* (fundamental conjecture). Only recently this conjecture was disproved [Mazur and Milnor, 1961], but its use had been circumvented in 1915 by J. W. Alexander's proof of the topological invariance of the homology groups. The subject's development into what we now recognize as algebraic topology led to more kinds of complexes and more general notions of homology and cohomology.

What seems essential to the subject is the existence of a module of chains (generalizing the simplicial chains which were created in triangulated spaces) with a boundary operator such that $\partial^2 = 0$ Something like this then, seems to have been a negative heuristic, i.e., an irrefutable hard core. For the hard core, *however interpreted*, must lie at the base of any explanation offered by the research program. The early work of Poincaré and others went far to explain both the facts about integrals motioned earlier and Euler's formula for polyhedra, by interpreting the hard core in terms of triangulations of spaces. This interpretation, then, I take to be a positive heuristic, one which was modified by later developments, when polyhedra were no longer the exclusive reference spaces of algebraic topology.

VI Until now, philosophies and expositions of mathematics have concentrated on what we would call the negative heuristic, which is by nature static (since irrefutable) The dynamic quality of mathematics energes only with an understanding of the role of the positive heuristic. Again, we quote Grattan-Guinness [6]: "The distinction between "mathematics" and "history of mathematics" is false in principle: there are only mathematical problems and they have a history." I hope that my remarks help to show how we may use a richer philosophy of mathematics to incorporate history in our expositions

Notes

1 When he received the Steele Prize for mathematical exposition last year, Edwards paid tribute to Toeplitz s book [27] and spoke with approval of a growing interest in history and philosophy within the mathematical community.

2 Cf Hersh [10, p 33] on the criteria we use for evaluating mathematics: "To make these criteria explicit, to bring them in to the open for discussion, challenge, and controversy, would be one important philosophical activity for mathematicians "

3 See, for example, [11] and [28].

4 Cf. Kuhn [14 pp 135-136]: "Textbooks . record the stable outcome of past revolutions . . Textbooks thus begin by truncating the scientist s sense of his discipline's history and then proceed to supply a substitute for what they have eliminated Characteristically, textbooks of science contain just a bit of history

Bibliography

1 Agassi, Joseph. On Mathematics Education: the Lakatosian Revolution. *For the Learning of Mathematics*, 1:1 (1980), 27-31.

2 Edwards, Harold M. *Fermat's last theorem* New York and Berlin: Springer-Verlag, 1977.

3 Edwards, Harold M. *Riemann's zeta function* New York and London: Academic Press, 1974.

4 Grabiner, Judith V. Is Mathematical Truth Time Dependent? In: *American Mathematical Monthly,* (1974), 354-365

5 Grattan-Guinness, I. Lakatos and the Philosophy of Mathematics and Science. In: *British Journal for the History of Science* 12 (1979), 317-337.

6 Grattan-Guinness, I. Not from Nowhere: History and Philosophy behind Mathematical Education *International Journal for Mathematical Education in Science and Technology*, 4 (1973), 421-453

7 Goodman, Nicolas D Mathematics as an Objective Science. *American Mathematical Monthly*, 86 (1979), 540-551

8 Hacking, Ian Imre Lakatos's Philosophy of Science. *British Journal for the Philosophy of Science*, 30 (1979), 381-410

9 Hallett, Michael Towards a Theory of Mathematical Research Programmes. I and II. *British Journal for the Philosophy of Science*, 30 (1979), 1-25, 135-159.

10 Hersh, Reuben. Some Proposals for Reviving the Philosophy of Mathematics. *Advances in Mathematics*, 31 (1979), 31-50.

11 Hilbert, David Axiomatic Thinking. *Philosophia Mathematica*, 7 (1970), 1-12

12 Kilcher, Philip. On the Uses of Rigorous Proof. *Science*, 196 (1977) 782-783.

13 Kline, Morris *Mathematical thought from ancient to modern times*. New York: Oxford University Press, 1972.

14 Kuhn, Thomas S *The structure of scientific revolutions* Chicago: The University of Chicago Press, 1962.

15 Lakatos, Imre. Cauchy and the Continuum. In: *Mathematics science and epistemology* I. Lakatos Cambridge: Cambridge University Press. 1978, 43-60

16 Lakatos, Imre Falsification and the Methodology of Scientific Research Programmes In: *Criticism and the Growth of Knowledge*, ed I Lakatos and A Musgrave Cambridge: Cambridge University Press, 1970, 91-196

17 Lakatos, Imre *Proofs and Refutations* New York: Cambridge University Press, 1976.

18 Lakatos, Imre What Does a Mathematical Proof Prove? In: *Mathematics. Science and Epistemology*, I. Lakatos, Cambridge: Cambridge University Press 1978, 61-70

19 Pierpont, James Mathematical Rigor, Past and Present *American Mathematical Monthly*, 35 (1928), 23-53

20 Polya, George *Mathematical discovery*. New York: John Wiley & Sons, 1962

21 Polya, George *Mathematics and plausible reasoning* London: Oxford University Press, 1954.

22 Polya, George The Teaching of Mathematics and the Biogenetic Law. In: *The Scientist Speculates*, ed I J. Good London: Heinemann, 1972, 352-356.

23 Rudin, Walter *Principles of Mathematical Analysis*, First edition. New York; McGraw-Hill, 1953

24 Schuster, John A. Kuhn and Lakatos and the History of Science. *British Journal for the History of Science*, 12 (1979), 301-317

25 Snapper, Ernst The Three Crises in Mathematics: Logicism, Intuitionism and Formalism *Mathematics Magazine*, 52 (1979), 207-216.

26 Snapper, Ernst. What Is Mathematics? *American Mathematical Monthly*, 86 (1979), 551-557

27 Toeplitz, Otto *The calculus: a genetic approach*. Chicago: University of Chicago Press, 1963.

28 Weyl, Hermann, A Half-Century of Mathematics *American Mathematical Monthly*, 58 (1951), 523-553

29 Wilder, R L , History in the Mathematics Curriculum: Its Status. Quality, and Function *American Mathematical Monthly* 79 (1972), 479-495.

30 Wittgenstein, Ludwig, *Remarks on the foundations of mathematics*. Cambridge (Mass): The M.I.T Press, 1967.