Theorems in School
NEW DIRECTIONS IN MATHEMATICS AND SCIENCE EDUCATION
Volume 1

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Scope
Mathematics and science education are in a state of change. Received models of teaching, curriculum, and researching in the two fields are adopting and developing new ways of thinking about how people of all ages know, learn, and develop. The recent literature in both fields includes contributions focusing on issues and using theoretical frames that were unthinkable a decade ago. For example, we see an increase in the use of conceptual and methodological tools from anthropology and semiotics to understand how different forms of knowledge are interconnected, how students learn, how textbooks are written, etcetera. Science and mathematics educators also have turned to issues such as identity and emotion as salient to the way in which people of all ages display and develop knowledge and skills. And they use dialectical or phenomenological approaches to answer ever arising questions about learning and development in science and mathematics.

The purpose of this series is to encourage the publication of books that are close to the cutting edge of both fields. The series aims at becoming a leader in providing refreshing and bold new work—rather than out-of-date reproductions of past states of the art—shaping both fields more than reproducing them, thereby closing the traditional gap that exists between journal articles and books in terms of their salience about what is new. The series is intended not only to foster books concerned with knowing, learning, and teaching in school but also with doing and learning mathematics and science across the whole lifespan (e.g., science in kindergarten; mathematics at work); and it is to be a vehicle for publishing books that fall between the two domains—such as when scientists learn about graphs and graphing as part of their work.
Theorems in School

From History, Epistemology and Cognition to Classroom Practice

Editor:
Paolo Boero

Universita di Genova, Italy
CONTENTS

Preface

The ongoing value of proof
_Gila Hanna_ 3

Introduction

Theorems in school: An introduction
_Paolo Boero_ 19

Part I: The historical and epistemological dimension

1 Origin of mathematical proof: History and epistemology
_Gilbert Arsac_ 27

2 The proof in the 20th century: From Hilbert to automatic theorem proving
_Ferdinando Arzarello_ 43

3 Students’ proof schemes revisited
_Guershon Harel_ 65

Part II: Curricular choices, historical traditions and learning of proof:
Two national case studies

4 Curriculum change and geometrical reasoning
_Celia Hoyles and Lulu Healy_ 81

5 The tradition and role of proof in mathematics education in Hungary
_Julianna Szendrei-Radnai and Judit Török_ 117

Part III: Argumentation and proof

6 Cognitive functioning and the understanding of mathematical processes of proof
_Raymond Duval_ 137

7 Some remarks about argumentation and proof
_Nadia Douek_ 163
Part IV: Didactical aspects

8 Making possible the discussion of “impossible in mathematics”
Greisy Winicki-Landman 185

9 The development of proof making by students
Carolyn A. Maher, Ethel M. Muter and Regina D. Kiczek 197

10 Approaching and developing the culture of geometry theorems in school: A theoretical framework
Marolina Bartolini Bussi, Paolo Boero, Franca Ferri, Rossella Garuti and Maria Alessandra Mariotti 211

11 Construction problems in primary school: A case from the geometry of circle
Maria G. Bartolini Bussi, Mara Boni and Franca Ferri 219

12 Approaching theorems in grade VIII: Some mental processes underlying producing and proving conjectures, and conditions suitable to enhance them
Paolo Boero, Rossella Garuti and Enrica Lemut 249

13 From dynamic exploration to “theory” and “theorems” (from 6th to 8th grades)
Laura Parenti, Maria Teresa Barberis, Massima Pastorino and Paola Viglienzone 265

14 Geometrical proof: The mediation of a microworld
Maria Alessandra Mariotti 285

15 The transition to formal proof in geometry
Ferdinando Arzarello, Federica Olivero, Domingo Paola and Ornella Robutti 305
PREFACE
The Ongoing Value of Proof

Over the past thirty years or so proof has been relegated to a less prominent role in the secondary mathematics curriculum in North America. This has come about in part because many mathematics educators have been influenced by certain developments in mathematics and in mathematics education to believe that proof is no longer central to mathematical theory and practice, and that in any case its use in the classroom will not promote learning. As a result many educators appear to have sought relief from the effort of teaching proof by avoiding it altogether.

In mathematics itself the use of computer-assisted proofs, the growing recognition accorded mathematical experimentation, and the invention of new types of proof that do not fit the standard mould have led some to argue that mathematicians will come to accept such forms of mathematical validation in place of deductive proof. The influence of these developments in mathematics has been strongly reinforced by the claims of some mathematics educators, inspired in part by the work of Lakatos, that deductive proof is not central to mathematical discovery, that mathematics is “fallible” in any case, and that proof is an authoritarian affront to modern social values.

This state of affairs has caused great concern among other mathematics educators. One of them was Greeno (1994), who laid the blame squarely on misconceptions as to the nature of proof:

Regarding educational practice, I am alarmed by what appears to be a trend toward making proofs disappear from precollege mathematics education, and I believe that this could be remedied by a more adequate theoretical account of the epistemological significance of proof in mathematics. (pp. 270–271)

This chapter holds that none of the developments mentioned really undermines the value of proof, and that many of the assertions made in their wake are either simply wrong or based upon misunderstandings (primarily on the part of mathematics educators). It maintains that proof deserves a prominent place in the curriculum because it continues to be a central feature of mathematics itself, as the preferred method of verification, and because it is a valuable tool for promoting mathematical understanding.

The Influence of Developments in Mathematics

A number of recent developments in mathematical practice, most of them reflecting in some way the growing use of computers, have caused some mathematicians
and others to call into question the continuing importance of proof or indeed to announce its imminent death. John Horgan (1993), a staff writer of *Scientific American*, makes this prediction in his article “The death of proof” that appeared in its October 1993 issue.

*Computer Proofs and a Potential Semi-Rigorous Culture*

One of the developments that prompted Horgan’s announcement is the use of computers to create or validate enormously long proofs, such as the recently published proofs of the four-colour theorem (Appel and Haken) or of the solution to the party problem (Radziszowski and McKay). These proofs require computations so long they could not possibly be performed or even verified by a human being. Because computers and computer programs are fallible, then, mathematicians will have to accept that assertions proved in this way can never be more than provisionally true.

This is a limitation in principle, but computing also has practical limitations, for all its ever-increasing power. There will always be tasks that take too long or are thought too expensive. Computer proofs are no exception, and so mathematicians have explored the implications that these limitations might have for mathematical practice. One prediction is that mathematicians, in the face of impractical times or prohibitive cost, will come to settle for “semi-rigour.”

In an article published in 1993 in the *Notices of the American Mathematical Society* entitled “Theorems for a price: Tomorrow’s semi-rigorous mathematical culture,” the mathematician Doron Zeilberger predicts that with the advent of computer proofs a “new testament is going to be written.” As “absolute proof becomes more and more expensive,” he maintains, mathematicians will use proofs which are less complete, but cheaper. He points to the example of algorithmic proof theory for hypergeometric identities, where there is no lack of well-known algorithms. The problem is that some cases require computations which even on tomorrow’s computers would take so long that they would exhaust the budget, if not the lifetime, of the researcher. He concludes that mathematicians will choose to limit the amount of computation allocated even to theorems which, in principle, are easily provable, opting for a less costly “almost certainty.” Furthermore, he predicts that mathematicians as a whole will come to accept such “semi-rigour” as a legitimate form of mathematical validation.

A mathematical conjecture has always been considered no more than a conjecture until proven, so it is not surprising that Zeilberger’s comments were quickly challenged by another mathematician. In an article published in the *Mathematical Intelligencer* (1994) with the dismissive title: “The death of proof? Semi-rigorous mathematics? You’ve got to be kidding!” George Andrews maintains that Zeilberger’s evidence is simply not convincing. That certain algorithms may prove too expensive to execute, he says, does not mean that mathematicians will now give up the idea of absolute proof with its “concomitantly great insight and, dare I say it, beauty” (p. 17).

And others have already pointed out that cheaper, non-rigorous proofs may prove costly in the long run. Saunders MacLane (1996) reported that in Italy during
the years 1880–1920 several results in algebraic geometry were published without careful proving. The situation became so bad that “unverified rumour seems to have it that a real triumph for an Italian algebraic geometer consisted in proving a new theorem and simultaneously proposing a counter-example to the theorem” (p. 2). Italian results in algebraic geometry were discredited until several mathematicians, including Emmy Noether, cleared up the difficult points by applying much more rigorous standards of proof.

New Types of Proof

Doubts about proof as a whole have also been raised by new types of proof that have little in common with traditional forms. A particularly fascinating development is the recently introduced concept of zero-knowledge proof (Blum, 1986), originally defined by Goldwasser, Micali and Rackoff (1985). This is an interactive protocol involving two parties, a prover and a verifier. It enables the prover to provide to the verifier convincing evidence that a proof exists, without disclosing any information about the proof itself. As a result of such an interaction the verifier is convinced that the theorem in question is true and that the prover knows a proof, but the verifier has zero knowledge of the proof itself and thus is not in a position to convince others.

In principle a zero-knowledge proof may be carried out with or without a computer. In terms of our topic, however, the most significant feature of the zero-knowledge method is that it is entirely at odds with the traditional view of proof as a demonstration open to inspection. This clearly thwarts the exchange of opinion among mathematicians by which a proof has traditionally come to be accepted.

Another interesting innovation is that of holographic proof (Babai, 1994; Cipra, 1993). Like zero-knowledge proof, this concept was introduced by computer scientists in collaboration with mathematicians. It consists of transforming a proof into a so-called transparent form that is verified by spot checks, rather than by checking every line. The authors of this concept have shown that it is possible to rewrite a proof (in great detail, using a formal language) in such a way that if there is an error at any point in the original proof it will be spread more or less evenly throughout the rewritten proof (the transparent form). Thus to determine whether the proof is free of error one need only check randomly selected lines in the transparent form.

By using a computer to increase the number of spot checks, the probability that an erroneous proof will be accepted as correct can be made as small as desired (though of course not infinitely small). Thus a holographic proof can yield near-certainty, and the degree of near-certainty can be precisely quantified. Nevertheless, a holographic proof, like a zero-knowledge proof, is entirely at odds with the traditional view of mathematical proof, because it does not meet the requirement that every single line of the proof be open to verification.
Zero-knowledge proofs, holographic proofs and the creation and verification of extremely long proofs such as that of the four-colour theorem are feasible only because of computers. Yet even these innovative types of proof are traditional, in the sense that they remain analytic proofs. More and more mathematicians appear to be doing all their work outside the bounds of deductive proof, however, confirming mathematical properties experimentally. A case in point is the Geometry Center at the University of Minnesota, where mathematicians use computer graphics to examine the properties of four-dimensional hypercubes and other figures, or to study transformations such as the twisting and smashing of spheres.

Even today one does not usually associate mathematics with empirical investigations, yet mathematicians have long carried out experiments to formulate and test conjectures (knowing full well that such testing did not constitute proof). Earlier mathematicians, limited to testing a small number of cases, would undoubtedly have done even more extensive experimentation if they had had the means. Thus today’s experimental mathematics would not seem to differ in principle from what has been done all along.

What does seem to be new is that more and more mathematicians spend their time almost exclusively on experimentation, and so naturally wish to assert a claim to its importance in its own right. Horgan quotes several mathematicians who assert that experimental methods have acquired a new respectability. These methods have certainly received increased attention and funding following the growth of graphics-oriented fields such as chaos theory and non-linear dynamics.

Certainly many more mathematicians have come to appreciate the power of computers in communicating mathematical concepts. Some of them are going well beyond communication, however. In a clear departure from previous practice, some now see it as legitimate to engage in experimental mathematics as a form of mathematical justification. Horgan maintains that:

… some mathematicians are challenging the notion that formal proofs should be the supreme standard of proof. Although no one advocates doing away with proofs altogether, some practitioners think the validity of certain propositions may be better established by comparing them with experiments run on computers or with real-world phenomena. (p. 94)

The implication of such a view is that experimentation is not only a prestigious mathematical activity, but also an alternative to proof, an equally valid form of mathematical confirmation. This would seem to redefine “experimental mathematics” as a new discipline, one which is no longer subject to the criteria by which mathematical truth has traditionally been judged.

The founding of Experimental Mathematics in 1991 might be seen as a portent of such a new and independent discipline. This new quarterly does differ markedly from traditional journals, in that it publishes the results of computer explorations rather than theorems and proofs. But does this mean that its editors think proof is dead? This would not seem to be the case. In their paper “Experimentation and
proof in mathematics” the editors of Experimental Mathematics, Epstein and Levy, first point out the enhanced potential of experimentation in the age of the computer: “the use of computers gives mathematicians another view of reality and another tool for investigating the correctness of a piece of mathematics through investigating examples” (1995, p. 674). They then go on, however, to make very clear how they believe experimentation fits into the mathematical scheme of things:

Note that we do value proofs: experimentally inspired results that can be proved are more desirable than conjectural ones…. The objective of Experimental Mathematics is to play a role in the discovery of formal proofs, not to displace them (p. 671)…. We believe that, far from undermining rigor, the use of computers in mathematics research will enhance it in several ways. (p. 674)

A New Division of Labour within Mathematics?

Many mathematicians are nevertheless very concerned that the recognition of experimentation as a valid full-time mathematical activity may obscure the fact that its results cannot be considered to have been proven. They do not agree on what, if anything, should be done about this. Some propose separation: that heuristic results be isolated as a clearly separate category.

Jaffe and Quinn (1993), for example, in their paper “Theoretical mathematics: Toward a cultural synthesis of mathematics and theoretical physics,” stress how important it is to distinguish unequivocally between results based on rigorous proof and those based on heuristic arguments. They even suggest labels for the two activities, proposing the former be called “rigorous mathematics” and the latter “theoretical mathematics,” by which they mean heuristic or speculative.

Jaffe and Quinn are motivated by a concern for standards of rigour, which they propose to preserve by isolating rigorous from non-rigorous mathematics through a new division of labour. They suggest that non-rigorous mathematics (“theoretical mathematics”) be considered a valid branch of mathematics in its own right, and that mathematicians be evaluated by the standards of the branch to which they choose to belong.

The suggestion that mathematicians be divided into two camps brought swift and varied reactions, sixteen of them in the Bulletin of the American Mathematical Society (1994). William Thurston, for example, responded in an eighteen-page essay entitled “On proof and progress in mathematics,” in which he opposes the division suggested by Jaffe and Quinn. In his view the important question is not “how do mathematicians prove theorems?” or “how do mathematicians make progress in mathematics?” but how they “advance human understanding of mathematics,” and accordingly he believes it wrong to split mathematics on the basis of standards of rigour. Though he does not question the role of proof in validation, he sees its main value in its ability to communicate ideas and generate understanding. Accordingly he proposes to mathematicians, who have traditionally gained
recognition among their peers primarily by proving theorems, that they all undertake to recognize and value the entire range of activities that advance understanding in their common discipline.

Fifteen other prominent mathematicians gave shorter responses. Most rejected the proposal put forward by Jaffe and Quinn to recognize two separate branches of mathematical activity (Atiyah et al., 1994). James Glimm wrote that if mathematics is to cope with the “serious expansion in the amount of speculation” it will need to adhere to the “absolute standard of logically correct reasoning [which] was developed and tested in the crucible of history” (p. 184).

Though driven, as were Jaffe and Quinn, by the growth of experimental mathematics and by a concern for rigour, it is clear that Glimm has come to precisely the opposite conclusion. While Jaffe and Quinn seem to believe that identifying and welcoming heuristic mathematics as a separate (though perhaps lesser) discipline would prevent it from establishing itself as a method of mathematical confirmation equal in value to rigorous proof, Glimm appears to fear that such isolation would have the opposite effect of allowing heuristics to stake this parallel claim.

But the responses also revealed differing views on the role of rigorous proof. Saunders MacLane stated that “mathematics does not need to copy the style of experimental physics. Mathematics rests on proof—and proof is eternal” (p. 193), while Atiyah conceded that “Perhaps we now have high standards of proof to aim at but, in the early stages of new developments, we must be prepared to act in more buccaneering style” (p. 178). And, not surprisingly, Mandelbrot asserted that rigour is “besides [sic] the point and usually distracting, even when possible.”

Mandelbrot also takes exception in his response to the customary practice of awarding credit only to those who prove conjectures, slighting those who came up with them in the first place. Indeed, one cannot ignore that the recent controversies over the place of experimentation and other heuristic approaches may be motivated as much by a concern for professional recognition as by disagreement over the nature of mathematical truth.

Certainly in these controversies the issue of the importance and prestige of heuristics has become intertwined, often confusingly, with the issue of the role of proof as the arbiter of mathematical truth. In the recent discussion triggered by Jaffe and Quinn, however, there is a perhaps surprising degree of agreement. All the participants would seem to agree with Albert Schwartz that heuristic mathematics is an important and legitimate part of their discipline. But none suggested that mathematicians carry out their work without a view to the ultimate test of proof. Those who agreed, as most did, that mathematicians should accord more recognition to those who come up with interesting and productive heuristic results, were nevertheless of the opinion that such results remain conjectures until validated by proof.

THE INFLUENCE OF LAKATOS

Mathematics educators in North America have been propelled in the direction of a diminished role for proof in the curriculum, however, not only by the recent
developments in mathematical practice discussed above, but also by interpretations
given to the work of Imre Lakatos. His thinking, published first as a dissertation in
1961 and finally as Proofs and refutations in 1976, provoked much discussion
among philosophers, and in particular among philosophers of mathematics (Agassi,
1981; Feyerabend, 1975; Hacking, 1979; Lehman, 1980; Steiner, 1983). Whatever
their assessment of his claims as a whole, they tended to accept Lakatos’ principal
insight that the critique of mathematical results by others has been the motive force
in the growth of mathematical knowledge.

Practising mathematicians were impressed by his work as well, in particular by
his detailed study of how the proof of Euler’s theorem had evolved over time. This
study shed light upon many previously unappreciated aspects of mathematical ac-
tivity, and for many mathematicians Lakatos’ account of the dynamics of mathe-
matical discovery rang true.

Lakatos’ ideas were brought to the attention of North American mathematics
educators primarily by Davis and Hersh (1981) in their book The Mathematical
Experience. Their enthusiastic exposition of Lakatos’ approach gained for it broad
acceptance among these educators, who assumed this approach to be more widely
applicable in mathematics itself than in fact it is.

It is not surprising that such a fascinating new way of looking at mathematical
discovery diverted attention from its weaknesses. The method of proof analysis is
admittedly engaging, but the case for it as a general method rests upon two exam-
iples, one of which is the study of polyhedra—an area in which it is relatively easy
to suggest the counterexamples required. This method does not even begin to ex-
plain some important cases of mathematical discovery, however. It has nothing to
say about set-theory research and the acceptance of the Zermelo-Fraenkel axioms,
or about the emergence of non-standard analysis, or in fact about the many mathe-
matical discoveries that did not start with a primitive conjecture.

It is not difficult, in fact, to cite cases in which a proof was found or a mathe-
matical discovery made in a way radically different from the process of heuristic
refutation described in Proofs and refutations. Even in the proof of Euler’s theorem
cited by Lakatos, for example, refutation is redundant; as soon as adequate defini-
tions have been formulated the theorem can be proved for all possible cases with-
out further discussion. Indeed, whenever mathematicians work with adequate
definitions (or an adequate “conceptual setting,” to use Bourbaki’s term), the proc-
ess of proof is not one of heuristic refutation. In “A renaissance of empiricism in
the recent philosophy of mathematics” (1978, p. 36), Lakatos himself says:

Not all formal mathematical theories are in equal danger of heuristic refuta-
tions. For instance, elementary group theory is scarcely in any danger; in this
case the original informal theory has been so radically replaced by the axio-
matic that heuristic refutations seem to be inconceivable.

In Proofs and Refutations Lakatos defines proof as a “thought experiment…a de-
composition of the original conjecture into subconjectures or lemmas” (p. 9). For
example, in his interpretation of the history of Euler’s theorem for a polyhedron
(V−E+F=2, where V is the number of vertices, E the number of edges, and F the
number of faces), Lakatos describes a thought experiment in which one imagines stretching a rubber polyhedron and observing the effects of its manipulation. He goes on, however, to describe a broader process which allows proofs and refutations to interact, generates counter-examples and “informal falsifiers,” gives rise to happy guesses, and ends with a well-formulated result.

This approach can be viewed as an attempt to examine mathematics from Popper’s point of view, to erect a critique of deductivism in mathematics parallel to Popper’s critique of inductivism in the physical sciences. Taking “induction” to mean the verification of general laws on the basis of observational data, Popper hoped to show that “empirical science does not really rely upon a principle of induction” (Putnam, 1987). Similarly, Lakatos hoped to show that verification in mathematics does not rely on “Euclidean deductivism.” In describing the heuristic process, Lakatos constantly attacks what he calls the “Euclidean programme,” which in his opinion aims at making mathematics “certain and infallible.”

But the truth is, first of all, that when mathematicians have undertaken the heuristic method which Lakatos describes, or one similar to it, it has almost always been for the purpose of arriving at certainty. In the case of Euclid’s theorem, for example, the long heuristic process did lead, in fact, to a proof which satisfies the accepted criteria of mathematical certainty. As Ian Hacking (1979) put it: “Critical discussion can enable a conjecture to evolve into logical truth. In the beginning Euler’s theorem was false; in the end it is true. The theorem has been ‘analytified.’”

Secondly, the concept of fallibilism would seem to be a red herring. Mark Steiner has shown that in the eyes of present-day topologists Euler’s theorem is “not about a polyhedron so much as about the underlying space the polyhedron divides” (p. 514). (He also shows that the modern proof is more explanatory than the one from the 19th century which Lakatos studied.) Steiner comes to the conclusion that the history of Euler’s theorem in the 20th century not only provides a case in which Lakatos’ model does not work, but, more importantly, demonstrates that we “can have progress without fallibilism” (p. 521). He also states that “despite the title of his book, Lakatos’ mathematical realism can be profitably disengaged, not only from his fallibilism, but from the method of proofs and refutations itself!” (p. 510).

John Conway has remarked recently that Lakatos’ Proofs and Refutations “is a very interesting book, but I fear is definitely misleading as regards mathematics in general” (Sept. 1995, request for advice, www.forum.swarthmore.edu). And in words which seem to sum up the present discussion, Conway adds:

It is misleading to take this example (Euler’s) as typical of the development of mathematics. Most mathematical theorems do get proved, and stay proved; the original proof may not be quite satisfactory according to later standards of proof, but that is a fairly trivial matter. In many cases there has been a significant omission or error in the first attempt at a proof, which later had to be corrected; but there have been very few cases like Euler’s theorem, in which the discussion continued for several centuries.
Let us now turn to the manner in which Lakatos’ ideas have come to influence the curriculum, at least in North America. Lakatos chose, perhaps with good reason, to put some of his ideas rather dramatically. Some mathematics educators would seem to have taken such assertions literally and sought to translate them directly into classroom practice (Dawson, 1969; Lampert, 1990).

Lakatos dismissed certainty and infallibility with the dramatic assertion “we never know, we only guess,” for example, and this has led some to consider mathematical knowledge to be provisional. Ernest (1996), for example, stated that “mathematics knowledge is understood to be fallible and eternally open to revision, both in terms of its proofs and its concepts” (p. 808). In addition, the very terms “informal falsifier” and “fallibility” of mathematics seem to have led many mathematics educators to propose downplaying “formal” mathematics in the curriculum (Dossey, 1992; Hersh, 1986).

Echoes of Lakatos’ thinking can be heard quite clearly in the curriculum guidance developed in the United States by the National Council of Teachers of Mathematics. In response to wide-spread concern for the quality of the mathematics curriculum, the NCTM has published *Standards for curriculum and evaluation* and *Professional standards for teaching mathematics*, covering the entire range from kindergarten through Grade 12 (NCTM, 1989, 1991). There is no national curriculum in the United States or Canada, where education is the responsibility of each state or province, but the NCTM *Standards*, though not binding, are very influential in both countries.

The authors of these guidelines, in their desire to reflect a modern view of mathematics, incorporated into them a position of relativism. According to John Dossey (1992), president of NCTM at the time the *Standards* were being drafted, “[T]he leaders and professional organisations in mathematics education are promoting a conception of mathematics that reflects a decidedly relativistic view of mathematics” (p. 45).

It may have been these views that led the NCTM *Standards* to give short shrift to proof, avoiding almost all mention of the term. The only explicit reference to proof, in fact, is in the context of preparation for post-secondary education, where the document states that “… college-intending students can … construct proofs for mathematical assertions, including indirect proofs and proofs by mathematical induction.” The implication is that students who do not intend to pursue post-secondary studies need not encounter the concept of proof.

Some of its recommendations (such as the development of short sequences of theorems, and the use of deductive arguments expressed orally and in sentence form) do offer a faint glimmer of proof (pp. 126–127). But the NCTM explicitly recommended decreased attention to proof even in the geometry curriculum, suggesting, as topics to be de-emphasized, two-column proofs, proofs of incidence, proofs of betweenness theorems, and Euclidean geometry as an axiomatic system.

This tendency to downplay the role of proof in mathematics is surely misguided. In the first place, formal proof arose as a response to a persistent concern for justification, a concern reaching back to Aristotle and Euclid, through Frege and Leibniz. There has always been a need to justify new results (and often previous results
as well), not always in the limited sense of establishing their truth, but rather in the broader sense of providing adequate grounds for their plausibility. Formal mathematical proof has been and remains one quite useful answer to this concern for justification.

Secondly, it is a mistake to think that the curriculum would be more reflective of mathematical practice if it were to limit itself to the use of informal counterexamples. The history of mathematics clearly shows that it is not the case, as Lakatos seems to have implied, that only heuristics and other “informal” mathematics are capable of providing counterexamples. Indeed, formal proofs themselves have often provided counterexamples to previously accepted theories or definitions. For instance, as Mark Steiner (1983, p. 502-521) points out, Peano provided a counterexample to the definition of a curve as the path of a continuously moving point by showing formally that a moving point could fill a two-dimensional area.

Gödel’s famous incompleteness proofs are another example, with an interesting and ironic twist. In this case formal proofs were employed to demonstrate that the axiomatic method itself has inherent limitations. Gödel could not have produced these proofs without using a comprehensive system of notation for the statements of pure arithmetic and a systematic codification of formal logic, both developed in the *Principia* for the purpose of arguing the Frege–Russell thesis that mathematics can be reduced to logic. His proofs could certainly not have been produced in informal mathematics or reduced to direct inspection.

Nor does it seem reasonable to assume that Gödel’s conclusions could have been arrived at through a discovery of counterexamples (“monster-barring”) followed by a denial (“monster-adjusting”), or by finding unexplained exceptions (“exception-barring”) or unstated assumptions (“hidden lemmas”). Curiously enough, however, when some educators make a case that formal proof and rigour should be downplayed in the curriculum they rest their case on Gödel’s most formal proof.

THE INFLUENCE OF SOCIAL VALUES

In the minds of many mathematics educators in North America the status of proof has also been called into question by the claim put forward, primarily by other educators, that it is a key element in an authoritarian view of mathematics (Confrey, 1994; Ernest, 1991; Nickson, 1994). This claim owes something to Lakatos (1976), who not only challenged the “Euclidean programme” for an “authoritative, infallible, irrefutable mathematics,” as noted, but also wrote of the dangers of elitism in mathematics. But it surely owes its prominence and its degree of acceptance primarily to the prevailing wind of “relativism” that seems to dominate the North American “intellectual” climate.

Indeed, supporters of this claim would say that the “Euclidean” view is in conflict with the present values of society, which dictate not only that one need not defer to authority, but also that one should not regard any knowledge as infallible or irrefutable. Some even appear to see proof in general, and rigorous proof in particular, as a mechanism of control wielded by an authoritarian establishment to
help impose upon students a body of knowledge that it does regard as infallible and irrefutable.

It must be stressed that such views are not in the first instance a protest against authoritarianism in the classroom, but rather a projection upon the curriculum debate of attitudes that have their origins in the popular culture of the United States. Discussing these attitudes, the philosopher of science Larry Laudan says:

The displacement of the idea that facts and evidence matter by the idea that everything boils down to subjective interests and perspectives is—second only to American political campaigns—the most prominent and pernicious manifestation of anti-intellectualism in our time. (1990, p. x)

Of course mathematics has sometimes been taught in an authoritarian way, as have other subjects, but one could hardly maintain that there has been a recent consensus among educators that it should be. One can only despair to find that proof has become the target of what would seem to be no more than a misguided desire to impose a sort of “political correctness” on mathematics education.

It is not easy to refute such a view of mathematics. In the first place, it is not easy to understand what it means to say that mathematics or a mathematical proof is “authoritative.” Certainly a proof offered by a very reputable mathematician would initially be given the benefit of the doubt, and in that sense the fact that this mathematician is considered an “authority” by other mathematicians would play some role in the eventual acceptance of the proof. But the claim seems to be that the very use of proof is authoritarian, and this claim is hard to fathom.

In fact the opposite is true. A proof is a transparent argument, in which all the information used and all the rules of reasoning are clearly displayed and open to criticism. It is in the very nature of proof that the validity of the conclusion flows from the proof itself, not from any external authority. Proof conveys to students the message that they can reason for themselves, that they do not need to defer to authority. Thus the use of proof in the classroom is, if anything, actually anti-authoritarian.

In the second place, it is hard to understand how the use of proof strengthens the idea that mathematics is infallible. Looking at the issue first from the point of view of theory, it is clear that any mathematical truth arrived at through a proof or series of proofs is contingent truth, rather than absolute truth, in the sense that its validity hinges upon other assumed mathematical truths and rules of reasoning. Nor would infallibility seem to be an issue from the point of view of mathematical practice. Mathematicians are as prone to making errors as almost anyone else, in proof and elsewhere. The history of mathematics can supply many examples of erroneous results which had to be subsequently corrected. Thus the concept of “infallibility” would seem to be irrelevant to the teaching of mathematics in general and the teaching of proof in particular.

The use of proof in the classroom has also been called into question on the grounds that it would encourage the idea that mathematics is an a priori science. The supporters of this claim see a conflict between this idea and their own view that mathematics is “socially constructed” (Ernest, 1991). Though their use of the
term a priori is not entirely clear, it would seem that what they reject is not that mathematics is a priori in the sense of being analytic (non-empirical), but rather that it is a priori in the sense of given, pre-existing, waiting to be discovered. Of course this is a view of mathematics that they might well see as standing in opposition to “socially constructed.”

On this point, however, Kitcher (1984) is surely right when he says that the pursuit of proof and rigour in mathematics does not carry with it a commitment to looking at mathematics as a body of a priori knowledge. Nor need it do so in mathematics education. As Kitcher put it: “To demand rigor in mathematics is to ask for a set of reasonings which stands in a particular relation to the set of reasonings which are currently accepted” (p. 213). Whether the set of reasonings currently accepted is regarded as given a priori or as socially constructed has no bearing on the value of proof in the classroom.

Those who challenge the use of proof in general would challenge even more strongly the use of rigorous proof in particular. Yet in mathematical practice the level of rigour is often a pragmatic choice. Kitcher states that it is quite rational to accept unrigorous reasoning when it proves its worth in solving problems, as it has in physics. Mathematicians worry about defects in rigour, he adds, only when they “come to appreciate that their current understanding is so inadequate that it prevents them from tackling the urgent research problems that they face” (p. 217). When is it rational to replace non-rigorous with rigorous reasoning? Kitcher’s answer is: “when the benefits it [rigorization] brings in terms of enhancing understanding outweigh the costs involved in sacrificing problem-solving ability.” (Mathematics educators, whose goal is surely to enhance understanding, would be well advised to adopt this guideline.)

Rigour is a question of degree in any case. In the classroom one need provide not absolute rigour, but enough rigour to achieve understanding and to convince. An argument presented with sufficient rigour will enlighten and convince more students, who in turn may convince their peers. It is the teacher who must judge when it is worthwhile insisting on more careful proving to promote the elusive but most important classroom goal of understanding.

CODA: PROOF IN THE CLASSROOM

With today’s stress on making mathematics “meaningful,” teachers are being encouraged to focus on the explanation of mathematical concepts and students are being asked to justify their findings and assertions. This would seem to be precisely the right climate to make use of proof, not only in its role as the ultimate form of mathematical justification, but also as an explanatory tool. But for this to succeed, students must be made familiar with the standards of mathematical argumentation; in other words, they must be taught proof. (The value of proof as an explanatory tool has been explored in some detail by a number of authors, among them Hanna (1990, 1995), who discusses “proofs that explain;” Wittmann and Müller (1990), who talk about the “inhaltlich-anschaulicher Beweis;” and Blum and Kirsch (1991), who emphasize the use of “preformal proofs.”)
Teaching students to both recognize and produce valid mathematical arguments is certainly a challenge. We know all too well that many students have difficulty following any sort of logical argument, much less a mathematical proof. But we cannot avoid this challenge. We need to find ways, through research and classroom experience, to help students master the skills and gain the understanding they need. Our failure to do so will deny us a valuable teaching tool and deny our students access to a crucial element of mathematics.

NOTES

REFERENCES


Gila Hanna

*Ontario Institute for Studies in Education
University of Toronto*
INTRODUCTION
THEOREMS IN SCHOOL: AN INTRODUCTION

WHY THIS BOOK?


The Forum Presentation on “Theorems in school” by some of the authors of this book, and related discussions involving other authors, showed that there were suitable conditions to start preparing a book that meant to support the renewed interest for proof and proving in mathematics education.

In the meantime, reconsideration of the importance of proof in mathematics education was leading to important changes in the orientations for curricula in different countries all over the world. In particular, this movement led, in the NCTM Standards published in 2000, to revalue proof and proving in mathematics curricula, and to recommendations to develop proof-related skills since the beginning of primary school.

The general reasons for these changes are presented in the chapter written by Gila Hanna, the Preface of this book.

But how to approach the development of proving skills (by students) and teach proof in school?

Old teaching models (essentially based on learning and repetition of proofs of relevant theorems as they are written in textbooks) do not fit the current needs of students and teachers. Moreover those models showed their inefficiency in the attempt to understand the role of proof in mathematics and the development of skills related to the production of conjectures and the construction of proofs by students. Such inefficiency was one of the reasons for getting rid of proof or reducing its importance in secondary school curricula in some countries, like the USA at the beginning of the last decade, or Sweden, Italy and other European countries in the last two decades.

Therefore entirely new approaches are needed. And these approaches must take into account the actual complexity of the subject: it is not wise to replace the old, structured teaching of proof with naive alternatives; the unavoidable bad results would bring teachers back to old methods!

This book, addressing mathematics educators, teacher-trainers and teachers, is published as a contribution to the endeavour of renewing the teaching of proof (and theorems) on the basis of historical-epistemological, cognitive and didactical considerations.

What led us to choose such a broad scope, embracing so different disciplines and perspectives? How does this choice affect and shape the plan of this book?