Gila Hanna
Hans Niels Jahnke
Helmut Pulte
Editors

Explanation and Proof in Mathematics

Philosophical and Educational Perspectives

Springer

Explanation and Proof in Mathematics

Gila Hanna • Hans Niels Jahnke
Helmut Pulte
Editors

Explanation and Proof in Mathematics

Philosophical and Educational Perspectives

Springer

Editors
Gila Hanna
Ontario Institute for Studies
in Education (OISE)
University of Toronto
Toronto ON, Canada
ghanna@oise.utoronto.ca

Hans Niels Jahnke
Department of Mathematics
University of Duisburg-Essen
Essen
Germany
njahnke@uni-due.de

Helmut Pulte
Ruhr-Universität
Bochum
Germany
helmut.pulte@rub.de

ISBN 978-1-4419-0575-8 e-ISBN 978-1-4419-0576-5
DOI 10.1007/978-1-4419-0576-5
Springer New York Dordrecht Heidelberg London

Library of Congress Control Number: 2009933111

Printed on acid-free paper

Springer is part of Springer Science+Business Media (www.springer.com)

Contents

Part II Proof and Cognitive Development

Part III Experiments, Diagrams and Proofs

Contributors

Nicolas Balacheff
CNRS, Laboratoire d'Informatique de Grenoble (LIG),
CNRS/UJF/Grenoble-INP, France
Nicolas.Balacheff@imag.fr

Ed Barbeau
Department of Mathematics, University of Toronto, Canada
barbeau@math.utoronto.ca

Evelyne Barbin
Centre François Viète d'épistémologie et d'histoire des sciences
et des techniques, Université de Nantes, France
evelyne.barbin@univ-nantes.fr

Maria G. Bartolini Bussi
Department of Mathematics, University of Modena and Reggio Emilia, Italy
bartolini@unimore.it

Karine Chemla
REHSEIS, UMR 7219 SPHère, Université Paris Diderot & CNRS, Paris, France
chemla@univ-paris-diderot.fr

Gila Hanna
Ontario Institute for Studies in Education, Université of Toronto, Canada
ghanna@oise.utoronto.ca

Aiso Heinze
Department of Mathematics Education, Leibniz Institute for Science Education,
Kiel, Germany
heinze@ipn.uni-kiel.de

Hans Niels Jahnke
Department of Mathematics, University of Duisburg-Essen, Campus Essen,
Germany
njahnke@uni-due.de

Teun Koetsier
Department of Mathematics, Faculty of Science, Vrije Universiteit, Amsterdam, The Netherlands
t.koetsier@few.vu.nl

Brendan Larvor
School of Humanities, University of Hertfordshire, Hatfield, UK
b.p.larvor@herts.ac.uk

Mary Leng
Department of Philosophy, University of Liverpool, UK
mcleng@liv.ac.uk

Thomas Mormann
Department of Logic and Philosophy of Science, University of the Basque Country UPV/EHU, Donostia-San Sebastian, Spain
ylxmomot@sf.ehu.es

Alfred Nordmann
Institut für Philosohie, Technische Universität Darmstadt, Germay
nordmann@phil.tu-darmstadt.de

Kazuhiko Nunokawa
Department of Learning Support, Joetsu University of Education, Joetsu, Japan,
nunokawa@juen.ac.jp

Helmut Pulte
Institut für Philosophie, University of Bochum
helmut.pulte@rub.de

Juan Pablo Mejia Ramos
Department of Learning and Teaching, Graduate School of Education, Rutgers - The State University of New Jersey, Brunswick, NJ, USA
jpmejia@rutgers.edu

David Tall
Institute of Education, University of Warwick, Coventry CV4 7AL, UK
davidtall@mac.com

Michael de Villiers
School of Science, Mathematics & Technology Education,
University of KwaZulu-Natal, Durban, South Africa
profmd@mweb.co.zaContributorsContributors

Part I
Reflections on the Nature and Teaching of Proof

Chapter 1
Introduction

The essays collected in this volume were originally contributions to the conference *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives*, which was held in Essen in November 2006. The essays are substantially extended versions of those papers presented at the conference; each essay has been reviewed by two reviewers and has undergone criticism and revision.

The conference was organized by the editors of this volume and brought together people from the fields of mathematics education, philosophy of mathematics and history of mathematics. The conference organizers firmly believe that this interdisciplinary dialog on proof between scholars in these three fields will be fruitful and rewarding for each field for several reasons.

Developments in the *practice of mathematics* during the last 3 decades have led to new types of proof and argumentation, challenging the established norms in this area. These developments originated from the use of computers (both as heuristic devices and as means of verification), from a new quality in the relations of mathematics to its applications in the empirical sciences and technology (see the Jaffe–Quinn paper and the subsequent debate among mathematicians, for example), and from a stronger awareness of the social nature of the processes leading to the acceptance of a proof.

These developments reflect the philosophy of mathematics, partly *ex post facto*, and partly in *anticipation*. Philosophers have long sought to define the nature of mathematics, notably by focusing upon its *logical foundations* and its formal structure. Over the past 40 years, however, the focus has shifted to encompass epistemological issues such as visualization, explanation and diagrammatic thinking.

As a consequence, in the *philosophy and history of mathematics* the approach to understanding mathematics has changed dramatically. More attention is paid to mathematical practice. This change was first highlighted in the late 1960s by the work of Imre Lakatos, who pronounced mathematics a "quasi-empirical science." His work continues to be highly relevant for the philosophy of mathematics as well as for the educational aspects of mathematics.

The work of Lakatos and others gave rise to conceptions of mathematics in general, and of proof in particular, based on detailed studies of mathematical practice. Recently, these studies have been frequently combined with the epistemological points

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives*, DOI 10.1007/978-1-4419-0576-5_1,

of view and cognitive approaches commonly subsumed under the term “naturalism.” In this context, philosophers have come to a greater recognition of the central importance of mathematical understanding, and so have looked more closely at how understanding is conveyed and at what counts as explanation in mathematics. As might be expected given these two changes in focus, philosophers of mathematics have turned their attention more and more from the *justificatory* to the *explanatory* role of proof. Their central questions are no longer only why and how a proof makes a proposition true but also how it contributes to an adequate understanding of the proposition and what role is played in this process by factors that go beyond logic.

The computer has caused a radical change in *educational practices* as well. In algebra, analysis, geometry and statistics, for example, computer software already provides revolutionary capabilities for visualization and *experimentation*, and holds the promise of still more change. In sum, trends in the philosophy and history of mathematics, as well as in mathematics education, have lead to a diversity of notions of proof and explanation. These trends interact, as people in one field are sensitive to developments in the others. The tendencies in the different fields are not identical, however; each field retains its own peculiarities.

The present volume intends to strengthen, in particular, mutual awareness in the philosophy of mathematics and in mathematics education about these new developments and to contribute to a more coherent theoretical framework based upon recent advances in the different fields. This seems quite possible (even necessary) in light of the strong empirical and realistic tendencies now shared by philosophy of mathematics and mathematics education. More important, though they share a strong interest in these new understandings of mathematical explanation and proof, philosophers of mathematics and researchers in mathematics education usually work in different institutional settings and in different research programs. It is crucial that researchers in both fields take an interest in the problems and questions of the other. So, we invited philosophers and historians to reflect on which dimensions of mathematical proof and explanation could be relevant to the general culture and to broadly educated adults and asked people from didactics to specifically elicit the epistemological and methodological aspects of their ideas.

In preparing the conference we identified four subthemes to help organize this dialog between philosophers of mathematics and mathematics educators. They refer to central concerns of the two groups as well as designating issues on which both groups are currently working:

1.1 The Legacy of Lakatos

Lakatos’ conception of mathematics as a “quasi-empirical science” has proved influential for the philosophy of mathematics as well as for the educational context. Though the naïve idea that Lakatos’ concepts could be transferred directly into the classroom, in the hope that insights into the need for proof would arise immediately from classroom discussions, has been proven untenable, Lakatos’ work is still an inspiration for both philosophers and educators.

1.2 Diagrammatic Thinking

The term "diagrammatic thinking" was coined by C. S. Peirce to designate the fact that thinking cannot be explained by purely logical means but is deeply dependent upon the systems of symbols and representations that are used. Independently of Peirce and philosophical discourse, this idea plays a key role in the didactics of mathematics, particularly in relation to mathematical argumentation and proof.

1.3 Mathematical Proof and the Empirical Sciences

A number of authors conceive of *mathematics in its connection with the empirical sciences*, especially physics. One can designate this approach as a form of physicalism – albeit in the broad meaning of that term. This does not at all mean that mathematics itself is considered to be an empirical science in a strict epistemological sense. This position stresses, rather, that the contents, methods and meaning of mathematics are to be discussed under the point of view that mathematics contributes, via the empirical sciences, to our understanding of the world around us. Theoretical concepts of mathematics, such as group and vector space, are to be set on a par with theoretical concepts of physics, such as electron and electromagnetic wave.

1.4 Different Types of Reasoning and Proof

In the practice and teaching of mathematics, different forms of mathematical argumentation have evolved; some of these are considered as proofs proper and some as heuristic devices. Besides formal proofs, we mention the various forms of induction, analogy, enumeration, algebraic manipulation, visualization, computer experimentation, computer proof and modeling. The conference tried to understand these modes of argumentation better and in greater depth, and to analyse the different views of their acceptability and fruitfulness on the part of mathematicians, philosophers and mathematics educators.

As it turned out, the subthemes proved to be recurring issues which surfaced in various papers rather than suitable bases for grouping them. Hence, we decided to organize the essays for the book in three broad sections.

Part I, *"Reflections on the nature and teaching of proof,"* has seven papers belonging to the first, third and fourth main themes of the conference. Lakatos' philosophy of mathematics is discussed or applied mainly in Koetsier's and Larvor's articles. The function of proof and explanation in mathematics and the empirical sciences plays a more or less prominent role in Jahnke's and Mormann's papers. Different theoretical types of proofs and their practical implications are central to the papers of Leng and Hanna and Barbeau. Heinze's paper plays a special role, because it deals with mathematical proofs neither from the point of view of the philosopher or historian of mathematics nor from that of mathematical educators, but brings in the perspective of working mathematicians.

Hans Niels Jahnke's paper *"The Conjoint Origin of Proof and Theoretical Physics"* "triangulates" the historical, philosophical and educational aspects of the idea of mathematical proof in ancient Greece. Jahnke argues that the rise of mathematical proof cannot be understood solely as an outcome of social-political processes or of internal mathematical developments, but rather as the result of a fruitful interaction of both. Following mainly A. Szabó's path-breaking historical studies of the concept of proof, Jahnke argues that mathematical proof – at least in the early context of dialectic – was understood as a mode of rational discourse not restricted to the aim of securing "dogmatic" claims. It mainly served to defend plausible presuppositions and to organize mathematical knowledge in an axiomatic-deductive manner. Setting up axioms and deducing theorems therefore were by no means unique to mathematics proper (i.e., geometry and arithmetic) but were also applied in other fields of knowledge, especially in those areas later considered parts of theoretical physics (e.g., statics, hydrostatics, astronomy). Jahnke then integrates into his argument P. Maddy's distinction between "intrinsic" and "extrinsic" justification of axioms in order to show that (pure) mathematics in the twentieth century could not have evolved without an extrinsic motivation and justification of basic hypotheses of mathematics and therefore shows marked similarities to the early Greek tradition. Consequently, Jahnke argues for a "new" manner of discussing mathematical proof in the classroom not only by integrating elements of the "old" dialectical tradition, but also by rejecting excessive, outmoded epistemological claims about mathematical axioms and proofs.

Teun Koetsier's contribution *"Lakatos, Lakoff and Núnez: Towards a Satisfactory Definition of Continuity"* aims to integrate Lakatos' logic and methodology of mathematics, as highlighted in his famous *Proofs and Refutations*, and Lakoff and Núnez's theory of metaphorical thinking in mathematics. To do this, Koetsier introduces the evolution of the concept of continuity from Euclid to the late nineteenth century as a case study. He argues that this development can be understood as a successive transformation of conceptual metaphors which starts from the "Euclidean Metaphor" of geometry and ends (via Leibniz, Euler, Lagrange, Encontre, Cauchy, Heine and Dedekind) in a quite modern, though seemingly (also) *metaphorical* treatment of the intermediate-value principle of analysis. In this case study, Koetsier conventionally presents mathematics as a system of conceptual metaphors in Lakoff & Núnez's sense. At the same time, he proposes a Lakatosian interplay of analysis and synthesis as a motor of system-transformations and as a warrantor of mathematical progress: Conjectures are turned into propositions and are (later) rejected by means of analysis and synthesis. The subsequent application of these methods leads to a continued elaboration and refinement of mathematical concepts (metaphors?) and techniques.

Mary Leng's paper *"Pre-Axiomatic Mathematical Reasoning: An Algebraic Approach"* takes a different position with respect to mathematical proof and mathematical theorizing in general. Following G. Hellman's terminology, Leng introduces an "algebraic approach" in a partly metaphorical manner in order to characterize the view that axioms relate to mathematical objects analogously to how algebraic equations with unknown variables relate to their solutions (which may form different, varied systems). Leng contrasts this approach, which comes close to Hilbert's, with the "assertory" approach of Frege and others, which holds that axioms are assertions of truths about a particular set of objects given independently of the axioms. Leng gives an account of the pros and cons of both views with respect to the truth of axioms in general and to the reference of mathematical propositions. She pays special attention to the fact that a lot of "pre-axiomatised" mathematics is done: namely, mathematics that apparently refers to well-established mathematical objects not "given" by formal axioms. Leng defends a "liberal" algebraic view which can deal with pre-axiomatic mathematical theorizing without getting caught in the traps of traditional "algebraic" and "assertory" approaches to axiomatisation.

Thomas Mormann's *"Completions, Constructions and Corrollaries"* brings a "Kantian" perspective to mathematical proof and to the general formation and development of mathematical concepts. Mormann focuses on Cassirer's theory of idealization in relation to Kant's theory of intuition as well as to Peirce's so-called "theorematic reasoning." First, he outlines Kant's understanding of intuition in mathematics and its main function – controlling mathematical proofs by constructive step-by-step checks. Then, he presents Russell's logicism as the "anti-intuitive" opponent of the Kantian philosophy of mathematics. Despite this antagonism, Mormann posits that both positions argued for a fixed, stable framework for mathematics, rooted in intuition or relational logic respectively. In his reconstruction, Mormann considers Cassirer's "critical idealism" as a sublime synthesis of both precursors, which eliminates the sharp philosophical separation between mathematics and the empirical sciences: Cassirer's concept of idealization is an "overarching" principle, being effective in both mathematics and the empirical sciences. Further, Mormann argues, this procedure of idealization is basic for some "completions" in mathematics (like Hilbert's principle of continuity) which are not secured by a purely logical approach. Mormann presents Peirce's "theorematic reasoning" as a kind of complement in order to make Cassirer's completions work in mathematical practice. The "common denominator" of both approaches, according to Mormann, is a shift in the general understanding of philosophy of mathematics: Its main task is no longer to provide unshakable foundations for mathematics and science but to analyze the formation and transformation of general concepts and their functions in mathematical and scientific practice.

Brandon Larvor's contribution *"Authoritarian vs. Authoritative Teaching: Polya and Lakatos"* endeavors to understand and compare the two mathematicians' theories of mathematical education arising from their (common) "Hungarian" mathematical tradition that started with L. Fejer. Larvor shows that Lakatos' "critical" and "heuristic" approach to teaching, which later culminates in his *Proofs and Refutations*, is already present in his early statements on the role of education and science and might have been shaped by his mathematical teacher Sándor Karácsony. Lakatos' "egalitarian" understanding of teaching mathematics is rooted in a political distaste for authoritarianism. His appreciation of heuristic proofs at the expense of deductive proofs is perhaps the most visible result of this distaste. According to Larvor, however, Lakatos failed to develop a useful pedagogical model that takes into account the basic fact that students and teachers are not equal dialog partners. Polya, on the other hand, stressed earlier than Lakatos the distinction between deductive and heuristic presentations of mathematics and made explicit the "shaping" function of heuristics in mathematical proof. Contrary to Lakatos, he develops a model of teaching mathematics; his model is not egalitarian, but aims at a kind of "mathematical empathy" in the relation of experienced teacher and learning student. Polya also rejects mathematical fallibilism, which is important for Lakatos' philosophy of mathematics. Though both thinkers share important insights into the teaching of mathematics, Lakatos' understanding might be described as anti-authoritative, while Polya's can be described as "authoritative," though not as "authoritarian."

Gila Hanna's and Ed Barbeau's paper *"Proofs as Bearers of Mathematical Knowledge"* extends Yehuda Rav's thesis that mathematical proofs (rather than theorems) should be the main focus of mathematical interest: They are the primary bearers of mathematical knowledge, if this knowledge is not restricted to results and their truth but is understood as the ability to apply methods, tools, strategies and concepts. In the first part of the paper, Hanna and Barbeau present and analyze Rav's thesis and its further development in its original context of mathematical practice. Here, informal proofs – "conceptual proofs" instead of formal derivations – dominate mathematical argumentation and are of special importance. Among other arguments, Rav's thesis gains considerable support from the fact that mathematical theorems often are re-proven differently (J. W. Dawson), even if their "truth-preserving" function is beyond doubt. The second part of the paper aims at a *desideratum* of mathematical education in applying and transforming Rav's concept of proof to teaching mathematics. With special reference to detailed analysis of two case studies from algebra and geometry, Hanna and Barbeau argue that conceptual proofs deserve a major role in advanced mathematical education, because they are of primary importance for the teaching of methods and strategies. This kind of teaching proofs is not meant as a challenge to "Euclidean" proofs in the classroom but as a complement which broadens the view of mathematical proof and the nature of mathematics in general.

Aiso Heinze's *"Mathematicians' Individual Criteria for Accepting Theorems and Proofs: An Empirical Approach"* enlarges and concludes the "Reflections" of Part I through an empirical study on the working mathematician's views on proof. When is the mathematical community prepared to accept a proposed proof as such? The social processes and criteria of evaluation involved in answering this question are at the core of Heinze's explorative (though not representative) empirical investigation, which surveyed 40 mathematicians from southern Germany. The survey questions referred to a couple of possible criteria for the individual acceptance of proofs which belong to the participants' own research areas, to other research areas or to part of a research article which has to be reviewed. Some of the findings are hardly astonishing – a trust in peer-review processes and in the judgment of the larger mathematical community – but also the personal checking of a proof in some detail plays a major role. Particularly, senior mathematicians frequently do not automatically accept "second-hand" checks as correct. Apparently, a skeptical and individualistic attitude within the mathematical community goes hand in hand with the epistemological fact that a deeper understanding of proven theorems needs a reconstruction of the proof-process itself. Due to the lack of further empirical data, however, these and other conjectures are open to further discussion and investigation.

Part II of the book, *"Proof and cognitive development,"* consists of four papers. The first two investigate promising theoretical frameworks, whereas the last two use a well-established Vygotskian framework to examine results of empirical research.

In *"Bridging Knowing and Proving in Mathematics: A Didactical Perspective,"* Nicolas Balacheff begins by identifying two didactical gaps that confront new secondary school students. First, they have not yet learned that proof in mathematics is very different from what counts as evidence in other disciplines, including the physical sciences. Second, they have studied mathematics for years without being told about mathematical proof, but as soon as they get to secondary school they are abruptly introduced to proof as an essential part of mathematics and find themselves having to cope with understanding and constructing mathematical proofs.

These gaps make the teaching of mathematics difficult; in Balacheff's view, they point to the need to examine the teaching and learning of mathematical proof as a "mastery of the relationships among knowing, representing and proving mathematically." The bulk of his paper is devoted to developing a framework for understanding the didactical complexity of learning and teaching mathematical proof, in particular for analyzing the gap between knowing mathematics and proving in mathematics.

Seeking such a framework, Balacheff characterizes the relationship between proof and explanation quite differently from most contemporary philosophers of mathematics, who discuss the explanatory power of proofs on the premise that not all mathematical proofs explain and not all mathematical explanations are proofs. Balacheff, however, states that a proof is an explanation by virtue of being a proof.

He sees a proof as starting out as a text (a candidate-proof) that goes through three stages. In the first stage, the text is meant to be an explanation. In the second stage, this text (explanation) undergoes a process of validation (an appropriate community regards that text as a proof). Finally, in the third stage the text (now considered a proof by the appropriate community) is judged to meet the current standards of mathematical practice and thus becomes a legitimate mathematical proof. As Balacheff's Venn diagram shows, a proof is embedded in the class of explanation, that is, "mathematical proof $\subseteq$ proof $\subseteq$ explanation." Balacheff then arrives at a framework with three components: (1) action, (2) formulation (semiotic system), and (3) validation (control structure). He concludes that "This trilogy, which defines a conception, also shapes didactical situations; there is no validation possible if a claim has not been explicitly expressed and shared; and there is no representation without a semantic which emerges from the activity (i.e., from the interaction of the learner with the mathematical milieu)."

In *"The Long-term Cognitive Development of Reasoning and Proof,"* David Tall and Juan Pablo Mejia-Ramos use Tall's model of "three worlds of mathematics" to discuss aspects of cognitive development in mathematical thinking. In his previous research, Tall investigated for more than 30 years how children come to understand mathematics. His results, published in several scholarly journals, led him to define "three worlds of mathematics" – three ways in which individuals operate when faced with new learning tasks: (1) conceptual-embodied (using physical, visual and other senses); (2) proceptual-symbolic (using mathematical symbols as both processes and concepts, thus the term "procept"), and (3) axiomatic-formal (using formal mathematics).

Tall and Mejia-Ramos examine the difficult transition experienced by university students, from somewhat informal reasoning in school mathematics to proving within the formal theory of mathematics. Using Tall's "three worlds" model in combination with Toulmin's theory of argumentation, they describe how the three worlds overlap to a certain degree and are also interdependent. The first two worlds, those of embodiment and symbolism, do act as a foundation for progress towards the axiomatic-formal world. But the third, axiomatic-formal, world also acts as a foundation for the first two worlds, in that it often leads back to new and different worlds of embodiment and symbolism.

Tall and Mejia-Ramos argue that an understanding of formalization is insufficient to understand proof, since they have shown "how not only does embodiment and symbolism lead into formal proof, but how structure theorems return us to more powerful forms of embodiment and symbolism that can support the quest for further development of ideas."

The next two papers, "*Historical Artefacts, Semiotic Mediation, and Teaching Proof*" by Mariolina Bartolini-Bussi, and *"Proofs, Semiotics and Artefacts of Information Technologies"* by Alessandra Mariotti, also investigate the cognitive challenges in teaching and learning proof, but they do not aim at analyzing existing theoretical frameworks or developing new ones. Rather, they both use Vygotsky's framework, the basic assumptions of which are that the individual mind is an active participant in cognition and that learning is an essentially social process with a semiotic character, requiring interpretation and reconstruction of communication signs and artefacts. A key point of Vygotsky's theory is the need for mediation between the individual mind and the external social world. Bartolini-Bussi and Mariotti both explore the use of artefacts in the mathematics classroom and try to understand how these artefacts act as a means of mediation and how their use enables students to make sense of new learning tasks.

Bartolini-Bussi examines concrete physical artefacts: a pair of gear wheels meshed so that turning one causes the other to turn in the opposite direction, and mechanical devices for constructing parabolas. In the case of the gear wheels, the use of a concrete artefact proved to be helpful, in that students did come up with a postulate and a conviction that their postulate would be validated. In addition, the use the artefact seemed to have fostered a semiotic activity that encouraged the students to reason more theoretically about the functioning of gears.

In the case of the mechanical devices for constructing parabolas, Bartolini-Bussi notes that these concrete artefacts offered several advantages: (1) a context for historical reconstruction, for dynamic exploration and for the production of a conjecture, (2) continuous support during the construction of a proof framed by elementary geometry, and (3) a demonstration of the geometrical meaning of the parameter "p" that appears in the conic equation.

Mariotti examines two information technology artefacts: *Cabri-géomètre*, a dynamic geometry program, and *L'Algebrista*, a symbolic manipulation program. She uses the semiotic character of these specific artefacts to help students approach issues of validation and to teach mathematical proof. Mariotti gives an example of how the use of the Dynamic Geometry Environment artefact, *Cabri-géomètre*, carries semiotic potential and thus is useful a tool in teaching proof. This artefact enabled the teachers to structure classroom activities whereby students were engaged in (1) the production of a *Cabri* figure corresponding to a geometric figure, (2) a description of the procedure used to obtain the *Cabri* figure, and (3) a justification of the "correctness" of the construction. A second example, concerning the teaching of algebraic theory, uses a symbolic manipulator, *L'Algebrista*, as an artefact. Again, this artefact allowed a restructuring of classroom activities that enabled teachers to increase mathematical meanings for their students.

These two papers lend support to the idea that semiotic mediators in the form of artefacts, whether physical or derived from information technology, can be used successfully in the classroom at both the elementary and the secondary levels, not only to teach mathematics but to help students understand how one arrives at mathematical validation.

Part III, *"Experiments, Diagrams and Proofs,"* analyzes the phenomenon of proof by considering the interaction between processes and products. The first essay in this part, by a philosopher of mathematics, sets the stage with a fresh view of Wittgenstein's ideas on proof. Two essays on educational issues follow, which put proof in the broader context of experimentation and problem solving. Part III is completed by two historical case studies relating the process of proving to the way a proof is written down.

In *"Proof as Experiment in Wittgenstein,"* Alfred Nordmann reconstructs Wittgenstein's philosophy of mathematical proof as a complementarity between "proof as picture" and "proof as experiment." The perspectives designated by these two concepts are quite different; consequently, philosophers have produced bewilderingly different interpretations of Wittgenstein's approach. Using the concept of "complementarity," Nordmann invites the reader to consider these two perspectives as necessarily related, thus reconciling the seemingly divergent interpretations. However, he leaves open the question of whether every proof can be considered in both these ways.

"Proof as picture" refers to a proof as a written product. For Wittgenstein, it is exemplified by a *calculation* as it appears on a sheet of paper. Such a calculation comprises, line-by-line, the steps which lead from the initial assumptions to the final result. It shows two features: It is (1) surveyable and (2) reproducible. On the one hand, only the proof as a surveyable whole can tell us what was proved.

On the other hand, the proof can also be reproduced "with certainty in its entirety" like copying a picture wholesale and "once and for all."

"Proof as experiment" relates to the productive and creative aspects of proof. In an analogy to scientific experiments, the term refers to the experience of undergoing the proof. Wittgenstein's paradigm case for this view is the *reductio ad absurdum* or negative proof. In this case, a proof does not add a conclusion to the premises but it changes the domain of what is imaginable by rejecting one of the premises. Hence, going through the proof involves us in a process at the end of which we see things differently. For example, proving that trisection of an angle by ruler and compass is impossible changes our idea of trisection itself. The proof allows us to shift from an old to a new state, from a wrong way of seeing the world to a right one.

Nordmann argues that the opposition between pictures and experiments elucidates what is vaguely designated by opposing static vs. dynamic, synchronic vs. diachronic, and justificatory vs. exploratory aspects of proof. Proof as picture and proof as experiment are two ways of considering proof rather than two types of proof. They cannot be distinguished as necessary on the one hand and empirical on the other. The experiments of the mathematician and of the empirical scientist are similar in that neither experimenter knows what the results will be, but differ in that the mathematicians' experiment immediately yields a surveyable picture of itself, so that showing something and showing its paradigmatic necessity can collapse into a single step.

In *"Experimentation and Proof in Mathematics,"* Michael de Villiers discusses the substantial importance of experimentation for mathematical proof and its limitations. The paper rests on a wealth of historical examples and on cases from de Villiers's personal mathematical experience.

De Villiers groups his considerations around three basic subthemes: (1) the relation between conjecturing on the one hand and verification/conviction on the other; (2) the role of refutations in the process of generating a (final) proof; and (3) the interplay between experimental and deductive phases in proving.

De Villiers writes that conjecturing a mathematical theorem often originates from experimentation, numerical investigations and measurements. A prominent example is Gauss's 1792 formulation of the Prime Number Theorem, which Gauss based on a great amount of numerical data. Hence, even in the absence of a rigorous proof of the theorem, mathematicians were convinced of its truth. Only at the end of the nineteenth century was an actual proof produced that was generally accepted.

Hence, conviction often precedes proof and is, in fact, a prerequisite for seeking a proof. Experimental evidence and conviction play a fundamental role. On the other hand, this is not true in every case. Sometimes it might be more efficient to look for a direct argument in order to solve a problem rather than trying a great number of special cases.

The role of refutations in the genesis of theorems and proofs, be they global or heuristic, is a typical Lakatosian motive. De Villiers gives several examples and shows that the study of special cases and the search for counter-examples, even after a theorem has been proved, are frequently very efficient in arriving at a final, mature formulation of a theorem and its proof. Thus, this strategy belongs to the top-level methods of mathematical research and should be explicitly treated in the classroom. In this context, de Villiers argues against a radical fallibilist philosophy of mathematics by making clear that its implicit assumption that the process of proof and refutations can carry on infinitely is erroneous.

Finally, de Villiers analyses the complementary interplay between mathematical experimentation and deduction, citing several thought-provoking examples.

In *"Proof, Mathematical Problem-Solving, and Explanation in Mathematics Teaching,"* Kazuhiko Nunokawa discusses the relation between proof and exploration by analyzing concrete processes of problem solving and proof generation which he observed with students. The paper focuses on the relationships among the problem solvers' explorations, constructions of explanations and generations of understanding. These three mental activities are inseparably intertwined. Explorations facilitate understanding, but the converse is also true. Exploration is guided by understanding and previously generated (personal) explanations. Problem solvers use implicit assumptions that direct their explorative activities. They envisage prospective explanations, which in the process of exploration become real explanations (or not). An especially interesting feature of the processes of exploration and explanation is the generation of new objects of thought, a process of abstraction which eliminates nonessential conditions, leads to a generalization of the situation at hand and opens the eyes to new phenomena and theorems.

A central theme for Nunokawa is the fundamental role of diagrams and their stepwise modification in the observed problem-solving processes. Hence, at the end of his paper, Nunokawa rightly remarks that most teachers have the (bad) habit to present so-to-speak final versions of diagrams to their students, whereas it would be much more important and teachable "to investigate how the final versions can emerge through interactions between explorations and understandings and what roles the immature versions of diagrams play in that process."

Evelyne Barbin's paper "*Evolving Geometric Proofs in the 17th Century: From Icons to Symbols*" is the first of two historical case studies that conclude the volume. The wider context of her study is a reform or transformation of elementary geometry which took place in the course of the scientific revolution of the seventeenth century and might be termed "arithmetization of geometry." In the seventeenth century, a widespread anti-Euclidean movement criticized Euclid's *Elements* as aiming more at certainty than at evidence and as presenting mathematical statements not in their "natural order." Hence, some mathematicians worked on a reform of elementary geometry and tried to organize the theory in a way that not only convinced but enlightened. Two of them, Antoine Arnauld and Bernard Lamy, wrote textbooks on elementary geometry in the second half of the seventeenth century.

Barbin analyses and compares five different proofs of a certain theorem on proportions: two ancient proofs by Euclid and three proofs from the seventeenth century by Arnauld and Lamy. In the modern view, the theorem consists of a simple rule for calculating with proportions, which says that in a proportion the product of the middle members is equal to the product of the external members. In her analysis, Barbin follows a rigorous method, making one of the rare and successful attempts to apply Charles Sanders Peirce's semiotic terminology to concrete mathematical texts. Barbin explains Peirce's concepts of symbol, diagram, icon, index and representation and applies them to the different proofs. Thus, she consistently elucidates the proofs' differences and specificities of style. The result of this analysis is that the seventeenth century authors not only produced new proofs of an ancient theorem but brought about a new conception or style of proof.

In *"Proof in the Wording: Two Modalities from Ancient Chinese Algorithms,"* Karine Chemla analyses the methods that early Chinese mathematicians used for proving the correctness of algorithms they had developed. The manuscripts she considers were in part recovered through excavations of tombs in the twentieth century; others have come down to us via the written tradition of Chinese mathematics. These manuscripts contain mainly algorithms; thus, it is a fundamental issue whether they contain arguments in favor of the algorithms' correctness and, if so, how these arguments are presented. Hence, in Chinese mathematics proof apparently takes a form distinctly different from the Western tradition. Nevertheless, there are certain points of similarity: Some parts of Western mathematics, for example in the seventeenth century, are presented as problems and algorithms for their solution.

In her analysis, Chemla uses a specific framework, to take into account that on the one hand most of the algorithms presuppose and refer to certain material calculating devices. Thus, it is an important question whether the algorithms present the operations step by step in regard to the calculating device. On the other hand, she has to consider in general how detailed the description of an algorithm is; thus, she writes of the "grain of the description." One of her most important results is her finding that proofs for the correctness of an algorithm are mainly given by way of semantics: that is, the Chinese authors often very carefully designated the meaning of the magnitudes calculated at each step in the course of an algorithm. In addition, the Chinese mathematicians might use a "coarser grain" of description – collapsing certain standard procedures – or change the order of operations in order to enhance the transparency of a proof.

Both these historical case studies show convincingly that proof and how it is represented strongly depend on the "diagrams" available in a certain culture and at a certain time.

In conclusion, we trust that this volume shows that much can be learned from an interdisciplinary approach bringing together perspectives from the fields of mathematics education, philosophy of mathematics, and history of mathematics. We also hope that the ideas embodied in this collection of papers will enrich the ongoing discussion about the status and function of proof in mathematics and its teaching, and will stimulate future cooperation among mathematical educators, philosophers and historians.

Acknowledgments We thank the "Deutsche Forschungsgemeinschaft" (DFG) for the generous funding of the Essen conference in 2006. We also thank the participants and authors for intense and fruitful discussions during the conference and for their generous spirit of cooperation in the preparation of this volume. A multitude of colleagues from history of mathematics, mathematics education and philosophy of mathematics refereed the various papers and thereby helped ensure the quality of this volume. We extend many thanks to them, too. We wish to thank John Holt for his stylistic polishing of much of the manuscript and for his helpful editorial advice and suggestions overall. We are also very grateful to Sarah-Jane Patterson and Stephanie Dick, graduate students in the Institute for History and Philosophy of Science and Technology at the University of Toronto, for their invaluable help in the preparation of the manuscript.

Gila Hanna, Toronto

Hans Niels Jahnke, Essen

Helmut Pulte, Bochum

Chapter 2
The Conjoint Origin of Proof and Theoretical Physics

Hans Niels Jahnke

2.1 The Origins of Proof

Historians of science and mathematics have proposed three different answers to the question of why the Greeks invented proof and the axiomatic-deductive organization of mathematics (see Szabó 1960, 356 ff.).

(1). The *socio-political thesis* claims a connection between the origin of mathematical proof and the freedom of speech provided by Greek democracy, a political and social system in which different parties fought for their interests by way of argument. According to this thesis, everyday political argumentation constituted a model for mathematical proof.
(2). The *internalist thesis* holds that mathematical proof emerged from the necessity to identify and eliminate incorrect statements from the corpus of accepted mathematics with which the Greeks were confronted when studying Babylonian and Egyptian mathematics.
(3). The *thesis of an influence of philosophy* says that the origin of proof in mathematics goes back to requirements made by philosophers.

Obviously, thesis (1) can claim some plausibility, though there is no direct evidence in its favor and it is hard to imagine what such evidence might look like.

Thesis (2) is stated by van der Waerden. He pointed out that the Greeks had learnt different formulae for the area of a circle from Egypt and Babylonia. The contradictory results might have provided a strong motivation for a critical re-examination of the mathematical rules in use at the time the Greeks entered the scene. Hence, at the time of Thales the Greeks started to investigate such problems by themselves in order to arrive at correct results (van der Waerden 1988, 89 ff.).

H.N. Jahnke (✉)
FB Mathematik, Universität Duisburg-Essen, 45117, Campus Essen, Essen, Germany
e-mail: njahnke@uni-due.de

[1]I would like to thank Gila Hanna and Helmut Pulte for their valuable advice.

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_2,

Thesis (3) is supported by the fact that standards of mathematical reasoning were broadly discussed by Greek philosophers, as the works of Plato and Aristotle show. Some authors even use the term “Platonic reform of mathematics.”

This paper considers in detail a fourth thesis which in a certain sense constitutes a combination of theses (1) and (3). It is based on a study by the historian of mathematics Árpád Szabó[2] (1960), who investigated the etymology of the terms used by Euclid to designate the different types of statements functioning as starting points of argumentation in the “Elements.”

Euclid divided the foundations of the “Elements” into three groups of statements: (1) Definitions, (2) Postulates and (3) Common Notions (Heath 1956). Definitions determine the objects with which the Elements are going to deal, whereas Postulates and Common Notions entail statements about these objects from which further statements can be derived. The distinction between postulates and common notions reflects the idea that the postulates are statements specific to geometry whereas the common notions provide propositions true for all of mathematics. Some historians emphasize that the postulates can be considered as statements of existence.

In the Greek text of Euclid handed down to us (Heiberg’s edition of 1883–1888) the definitions are called ὅϱοι, the postulates ἀιτήματα and the common notions κοιναί ἔννοιαι. In his analysis, Szabó starts with the observation that Proclus (fifth century AD), in his famous commentary on Euclid’s elements, used a different terminology (for an English translation, see Proclus 1970). Instead of ὅϱοσ (definition) Proclus applied the concept of ὑπόθεσις (hypothesis) and instead of κοιναί ἔννοιαι (common notions) he used ἀξιώμα (axiomata). He maintained the concept of ἀιτήματα (postulates) as contained in Euclid. Szabó explains the differing terminology by the hypothesis that Proclus referred to older manuscripts of Euclid than the one which has led to our modern edition of Euclid.

Szabó shows that ὑπόθεσις (hypothesis), ἀιτήματα (postulate) and ἀξιώμα (axiom) were common terms of pre-Euclidean and pre-Platonic dialectics, which is related both to philosophy and rhetoric. The classical Greek philosophers understood dialectics as the art of exchanging arguments and counter-arguments in a dialog debating a controversial proposition. The outcome of such an exercise might be not simply the refutation of one of the relevant points of view but rather a synthesis or a combination of the opposing assertions, or at least a qualitative transformation (see Ayer and O’Grady 1992, 484).

The use of the concept of hypothesis as synonymous with definition was common in pre-Euclidean and pre-Platonic dialectics. In this usage, hypothesis designated the fact that the participants in a dialog had to agree initially on a joint definition of the topic before they could enter the argumentative discourse about it. The Greeks, including Proclus, also used hypothesis in a more general sense, close to its meaning today. A hypothesis is that which is underlying and consequently can be used as a foundation of something else. Proclus, for example, said: ”Since this

[2] For a discussion of the personal and scientific relations between Szabó and Lakatos, see Maté (2006). I would like to thank Brendan Larvor for drawing my attention to this paper.

science of geometry is based, we say, on hypothesis (ἔξ ὑποθέσεως ἑιναι), and proves its later propositions from determinate first principles … he who prepares an introduction to geometry should present separately the principles of the science and the conclusions that follow from the principles, …" (Proclus 1970, 62).

According to Szabó, the three concepts of hypothesis, aitema (postulate) and axioma had a similar meaning in the pre-Platonic and pre-Aristotelian dialectics. They all designated those initial propositions on which the participants in a dialectic debate must agree. An initial proposition which was agreed upon was then called a "hypothesis". However, if participants did not agree or if one declared no decision, the proposition was then called aitema (postulate) or axioma (Szabó 1960, 399).

As a rule, participants will introduce into a dialectic debate hypotheses that they consider especially strong and expect to be accepted by the other participants: numerous examples of this type can be found in the Platonic dialogs. However, it is also possible to propose a hypothesis with the intention of critically examining it. In a philosophical discourse, one could derive consequences from such a hypothesis that are *desired* (plausible) or *not desired* (implausible). The former case leads to a strengthening of the hypothesis, the latter to its weakening. The extreme case of an undesired consequence would be a logical contradiction, which would necessarily lead to the rejection of the hypothesis. Therefore, the procedure of indirect proof in mathematics can be considered as directly related to common customs in philosophy. According to Szabó (1960) this constitutes an explanation for the frequent occurrence of indirect proofs in the mathematics of the early Greek period.

The concept of common notions as a name for the third group of introductory statements needs special attention. As mentioned above, this term is a direct translation of the Greek κοιναί ἔννοιαι and designates "the ideas common to all human beings". According to Szabó, the term stems from Stoic philosophy (since 300 BC) and connotes a proposition that cannot be doubted justifiably. Proclus also attributes the same meaning to the concept of ἀξιώμα, which he used instead of κοιναί ἔννοιαι. For example, he wrote at one point: "These are what are generally called indemonstrable axioms, inasmuch as they are deemed by everybody to be true and no one disputes them" (Proclus 1970, 152). At another point he even wrote, with an allusion to Aristotle: "…whereas the axiom is as such indemonstrable and everyone would be disposed to accept it, even though some might dispute it for the sake of argument"(Proclus 1970, 143). Thus, only quarrelsome people would doubt the validity of the Euclidean axioms; since Aristotle, this has been the dominant view.

Szabó (1960) shows that the pre-Aristotelean use of the term axioma was quite similar to that of the term aitema, so that axioma meant a statement upon which the participants of a debate agreed or whose acceptance they left undecided. Furthermore, he makes it clear that the propositions designated in Euclid's "Elements" as axioms or common notions had been doubted in the early period of Greek philosophy, namely by Zenon and the Eleatic School (fifth century BC). The explicit compilation of the statements headed by the term axioms (or common notions) in the early period of constructing the elements of mathematics was motivated by the intention of rejecting Zeno's criticism. Only later, when the philosophy of the Eleates had been weakened, did the respective statements appear as unquestionable for a healthy mind.

In this way, the concept of an axiom gained currency in Greek philosophy and in mathematics. Its starting point lay in the art of philosophical discourse; later it played a role in both philosophy and mathematics. More important for this paper, it underwent a concomitant change in its epistemological status. In the early context of dialectics, the term axiom designated a proposition that in the beginning of a debate could be accepted or not. However, axiom's later meaning in mathematics was clearly that of a statement which itself cannot be proved but is absolutely certain and therefore can serve as a fundament of a deductively organized theory. This later meaning became the still-dominant view in Western science and philosophy.

Aristotle expounded the newer meaning of axiom at length in his "Analytica posteriora":

> I call "first principles" in each genus those facts which cannot be proved. Thus the meaning both of the primary truths and the attributes demonstrated from them is assumed; as for their existence, that of the principles must be assumed, but that of the attributes must be proved. E. g., we assume the meaning of "unit", "straight" and "triangular"; but while we assume the existence of the unit and geometrical magnitude, that of the rest must be proved. (Aristotle 1966, I, 10)

Aristotle also knew the distinction between postulate (aitema) and axiom (common notions) as used in Euclid:

> Of the first principles used in the demonstrative sciences some are special to particular sciences, and some are common; . . . Special principles are such as that a line , or straightness, is of such-and-such a nature; common principles are such as that when equals are taken from equals the remainders are equals. (Aristotle 1966, I, 10)

Thus, Szabó's study leads to the following overall picture of the emergence of mathematical proof. In early Greek philosophy, reaching back to the times of the Eleates (ca. 540 to 450 BC), the terms axioma and aitema designated propositions which were accepted in the beginning of a dialog as a basis of argumentation. In the course of the dialog, consequences were drawn from these propositions in order to examine them critically and to investigate whether the consequences were desired. In a case where the proposition referred to physical reality, "desired" could mean that the consequences agreed with experience. If the proposition referred to ethics, "desired" could mean that the consequences agreed with accepted norms of behavior. Desired consequences constituted a strong argument in favor of a proposition. The most extreme case of undesired consequence, a logical contradiction, led necessarily to rejecting the proposition. Most important, in the beginning of a dialog the epistemic status of an axioma or aitema was left indefinite. An axiom could be true or probable or perhaps even wrong.

In a second period, starting with Plato and Aristotle (since ca. 400 BC) the terms axioma and aitema changed their meaning dramatically; they now designated propositions considered absolutely true. Hence, the epistemic status of an axiom was no longer indefinite but definitely fixed. This change in epistemic status followed quite natural because at that time mathematicians had started building theories. Axioms were supposed true once and for all, and mathematicians were interested in deriving as many consequences from them as possible. Thus, the emergence of the classical view that the axioms of mathematics are absolutely true was inseparably linked to the fact that mathematics became a "normal science" to use T. Kuhn's term. After Plato and Aristotle, the classical view remained dominant until well into the nineteenth century.

Natural as it might have been, in the eyes of modern philosophy and modern mathematics this change of the epistemic status of axioms was nevertheless an unjustified dogmatisation. The decision to build on a fixed set of axioms and not to change them any further is epistemologically quite different from the decision to declare them absolutely true.

On a more general level, we can draw two consequences: First, Szabó's (1960) considerations suggest the thesis that the *practice of a rational discourse* provided a model for the organization of a mathematical theory according to the axiomatic-deductive method; in sum, proof is rooted in communication. However, this does not simply support the socio-political thesis, according to which proof was an outcome of Greek democracy. Rather, it shows a connection between proof and dialectics as an *art of leading a dialog*. This art aimed at a methodically ruled discourse in which the participants accept and obey certain rules of behavior. These rules are crystallized in the terms hypothesis, aitema and axiom, which entail the participants' obligation to exhibit their assumptions.

The second important consequence refers to the *universality* of dialectics. Any problem can become the subject of a dialectical discourse, regardless of which discipline or even aspect of life it involves. From a problem of ethics to the question of whether the side and diagonal of a square have a common measure, all problems could be treated in a debate. Different persons can talk about the respective topic as long as they are ready to reveal their suppositions. Analogously, the possibility of an axiomatic-deductive organization of a group of propositions is not confined to arithmetic and geometry, but can in principle be applied to any field of human knowledge. The Greeks realized this principle at the time of Euclid, and it led to the birth of theoretical physics.

2.2 Saving the Phenomena

During the Hellenistic era, within a short interval of time Greek scientists applied the axiomatic-deductive organization of a theory to a number of areas in natural science. Euclid himself wrote a deductively organized optics, whereas Archimedes provided axiomatic-deductive accounts of statics and hydrostatics.

In astronomy, too, it became common procedure to state hypotheses from which a group of phenomena could be derived and which provided a basis for calculating astronomical data. Propositions of quite a different nature could function as hypotheses. For example, Aristarchos of Samos (third century BC) began his paper "On the magnitudes and distances of the sun and the moon" with a hypothesis about how light rays travel in the system earth-sun-moon, a hypothesis about possible positions of the moon in regard to the earth, a hypothesis giving an explanation of the phases of the moon and a hypothesis about the angular distance of moon and sun at the time of half-moon (a measured value). These were the ingredients Aristarchos used for his deductions.

In the domain of astronomy, the Greeks discussed, in an exemplary manner, philosophical questions about the relation of theory and empirical evidence. This discussion started at the time of Plato and concerned the paths of the planets. In general, the planets apparently travel across the sky of fixed stars in circular arcs. At certain times, however, they perform a retrograde (and thus irregular) motion. This caused a severe problem; since the Pythagoreans, the Greeks had held a deeply rooted conviction that the heavenly bodies perform circular movements with constant velocity. But this could not account for the irregular retrograde movement of the planets.

Greek astronomers invented sophisticated hypotheses to solve this problem. The first scientist who proposed a solution was Eudoxos, the best mathematician of his time and a close friend of Plato's. Though the phenomenon of the retrograde movement of the planets was well known, it did not figure in the dialogs of Plato's early and middle period. Only in his late dialog "Nomoi" ("Laws") did Plato mention the problem. In this dialog, a stranger from Athens (presumably Eudoxos) appeared, who explained to Clinias (presumably Plato) that it only seems that the planets "wander" (i.e., perform an irregular movement), whereas in reality precisely the opposite is true: "Actually, each of them describes just one fixed orbit, although it is true that to all appearances its path is always changing" (Plato 1997, 1488). Thus, in his late period Plato acknowledged that we have to adjust our basic ideas in order to make them agree with empirical observations.

I will illustrate this principle by a case simpler than the paths of the planets but equally important in Greek astronomy. In the second century BC, the great astronomer and mathematician Hipparchos investigated an astronomical phenomenon probably already known before his time, the "anomaly of the sun." Roughly speaking, the term referred to the observation that the half-year of summer is about 1 week longer than the half-year of winter. Astronomically, the half-year of summer was then defined as the period that the sun on its yearly path around the earth (in terms of the geocentric system) needs to travel from the vernal equinox to the autumnal equinox. Analogously, the half-year of winter is the duration of the travel from the autumnal equinox to the vernal equinox. Vernal equinox and autumnal equinox are the two positions of the sun on the ecliptic at which day and night are equally long for beings living on the earth. The two points, observed from the earth, are exactly opposite to each other (vernal equinox, autumnal equinox and center of the earth form a straight line). Since the Greek astronomers supposed that all heavenly bodies move with constant velocity in circles around the center of the earth; it necessarily followed that the half-years of summer and winter would be equal.

The Greek astronomers needed to develop a hypothesis to explain this phenomenon. Hipparchos proposed a hypothesis placing the center of the sun's circular orbit not in the center of the earth but a bit outside it (Fig. 2.1). If this new center is properly placed, then the arc through which the sun travels during summer,

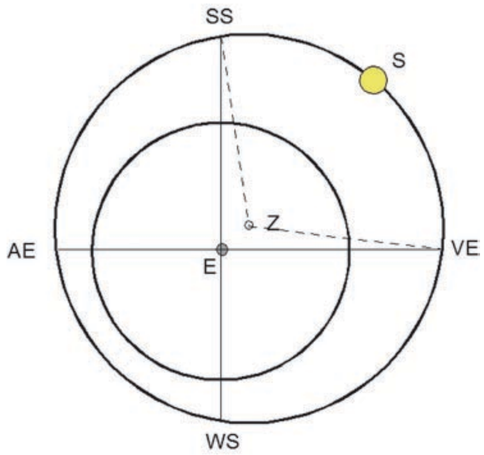


Fig. 2.1 Eccentric hypothesis

observed from the earth, is greater than a half-circle; the anomaly of the sun is explained. Later, Hipparchos' hypothesis was called by Ptolemaios the "eccentric hypothesis" (Toomer 1984, 144 pp).

Another hypothesis competing with that of Hipparchos was the "epicyclic hypothesis" of Appolonios of Perge (third century BC; see Fig. 2.2). It said that the sun moves on a circle concentric to the center of the universe, however "not actually on that circle but on another circle, which is carried by the first circle, and hence is known as the epicycle" (Toomer 1984, 141). Hence, the case of the anomaly of the sun confronts us with the remarkable phenomenon of a *competition of hypotheses*. Both hypotheses allow the derivation of consequences which agree with the astronomical phenomena. Since there was no further reason in favor of either one, it didn't matter which one was applied. Ptolemaios showed that, given an adequate choice of parameters, both hypotheses are mathematically equivalent and lead to the same data for the orbit of the sun. Of course, physically they are quite different; nevertheless, Ptolemaios did not take the side of one or the other.

Hence the following situation: The Greeks believed that the heavenly bodies moved with constant velocity on circles around the earth. These two assumptions (constancy of velocity and circularity of path) were so fundamental that the Greeks were by no means ready to give them up. The retrograde movement of the planets and the anomaly of the sun seemed to contradict these convictions. Consequently, Greek astronomers had to invent additional hypotheses which brought the theory into accordance with the phenomena observed. The Greeks called the task of inventing such hypotheses "saving the phenomena" ("σώζειν τὰ φαινόμενα").

The history of this phrase is interesting and reflects Greek ideas about how to bring theoretical thinking in agreement with observed phenomena (see Lloyd 1991; Mittelstrass 1962). In written sources the term "saving the phenomena" first appears in the writings of Simplikios, a Neo-Platonist commentator of the sixth

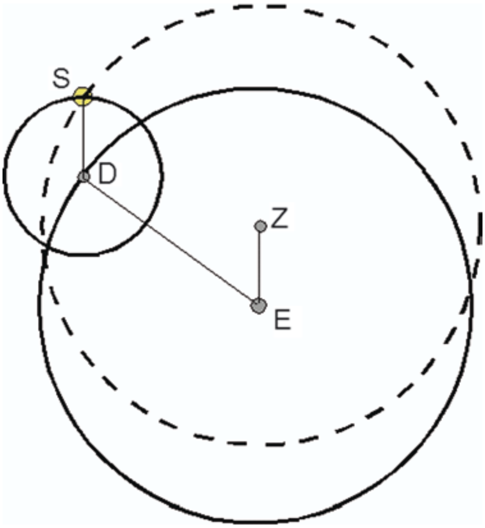


Fig. 2.2 Epicyclic hypothesis

century AD, a rather late source. However, the phrase probably goes back to the time of Plato. Simplikios wrote that Plato made "the saving of phenomena" a task for the astronomers. But we have seen that Plato hit upon this problem only late in his life; it is much more probable that he learnt about it from the astronomers (e.g., Eudoxos) than vice versa. It seems likely that the phrase had been a terminus technicus among astronomers since the fourth century BC.

A number of philosophers of science, the most prominent being Pierre Duhem (1908/1994), have defended the thesis that the Greeks held a purely conventionalist view and did not attribute any claim of truth to astronomical hypotheses. They counted different hypotheses, like the excentric or epicyclic hypotheses as equally acceptable if the consequences derived from them agreed with the observed phenomena. However, the Greeks in fact never questioned certain astronomical assumptions, namely the circularity of the paths and the constancy of velocities of the heavenly bodies, attributing to them (absolute) truth.

Mittelstrass (1962), giving a detailed analysis of its history, shows that the phrase "saving the phenomena" was a terminus technicus in ancient astronomy and stresses that it was used only by astronomers and in the context of astronomy (Mittelstrass 1962, 140 ff). He questions Simplikios' statement that Plato posed the problem of "saving the phenomena" and contradicts modern philosophers, such as Natorp (1921, p. 161, 382, 383), who have claimed that the idea of "saving the phenomena" was essential to the ancient Greek philosophy of science. According to Mittelstrass, only Galileo first transferred this principle to other disciplines and made it the basis of a general scientific methodology, which by the end of the nineteenth century was named the "hypothetico-deductive method."

Mittelstrass is surely right in denying that "saving the phenomena" was a general principle of Greek scientific thinking. He can also prove that the phrase was used explicitly only in astronomy. Greek scientific and philosophic thinking was a mixture of different ideas and approaches; there was no unified "scientific method." Nevertheless, Mittelstrass goes too far in strictly limiting to astronomy the idea that a hypothesis is evaluated through the adequacy of its consequences. As Szabó (1960) has shown, such trial was common practice in Greek dialectics and was reflected in early meanings of the terms aitema, axioma and hypothesis, meanings that the terms kept until the times of Plato and Aristotle. The procedure of supposing a hypothesis as given and investigating whether its consequences are desired abounds in Plato's dialogs. Thus, the idea underlying the phrase "saving the phenomena" had a broader presence in Greek scientific and philosophical thinking than Mittelstrass supposes. Besides, Mittelstrass did not take into account Szabó's (1960) study, though it had already been published.

Hence, I formulate the following thesis: The extension of the axiomatic procedure from geometry to physics and other disciplines cannot be imagined without the idea that an axiom is a hypothesis which may be justified not by direct intuition but by the adequacy of its consequences, in line with the original dialectical meaning of the terms aitema, axioma and hypothesis.

The Greeks set up a range of possible hypotheses in geometry and physics with a variety of epistemological justifications. For example, Euclid's geometrical postulates were considered from antiquity up to the nineteenth century as evident in themselves and absolutely true. Only the parallel postulate couldn't claim a similar epistemological status of direct evidence; this was already seen in antiquity. A possible response to this lack would have been to give up the epistemological claim that the axioms of geometry are evident in themselves, but the Greeks didn't do that. Another way out would have been to deny the parallel postulate the status of an axiom. The Neo-Platonist commentator Proclus did exactly that, declaring the parallel postulate a theorem whose proof had not yet been found. However, this tactic was motivated by philosophical not mathematical reasons, though the problem was a mathematical one. Besides, Proclus lived 700 years after Euclid; we do not even know how Euclid himself thought about his parallel postulate. Perhaps Euclid as a mathematician was more down-to-earth and was less concerned about his postulate's non-evidency.

A second example concerns statics. In the beginning of "On the equilibrium of planes or the centers of gravity of planes," Archimedes set up seven postulates; the first reads as:

> I postulate the following: 1. Equal weights at equal distances are in equilibrium, and equal weights at unequal distances are not in equilibrium but incline towards the weight which is at the greater distance." (Heath, 1953, 189)

This postulate shows a high degree of simplicity and evidence and, in this regard, is like Euclid's geometric postulates. The other postulates on which Archimedes based his statics are similar; they appear as unquestionable. Statics therefore seemed to have the same epistemological status as geometry and from early times up to the nineteenth century was considered a part of mathematics. However, during the nineteenth century statics became definitively classified as a subdiscipline of physics. Meanwhile, the view that the natural sciences are founded without exception on experiment became dominant. Hence, arose a problem: Statics had the appearance of a science that made statements about empirical reality, but was founded on propositions apparently true without empirical evidence. Only at the end of the nineteenth century did E. Mach (1976) expose, in an astute philosophical analysis, the (hidden) empirical assumptions in Archimedes' statics, thus clarifying that statics is an empirical, experimental science like any other.

As a final example, consider the (only) hypothesis which Archimedes stated at the beginning of his hydrostatics ("On floating bodies"):

> Let it be supposed that a fluid is of such a character that, its parts lying evenly and being continuous, that part which is thrust the less is driven along by that which is thrust the more; and that each of its parts is thrust by the fluid which is above it in a perpendicular direction if the fluid be sunk in anything and compressed by anything else. (Heath, 1953, 253)

Archimedes derived from this hypothesis his famous "law of upthrust" ("principle of buoyancy") and developed a mathematically sophisticated theory about the balance of swimming bodies. Obviously, the hypothesis does not appear simple or beyond doubt. To a historically open-minded reader, it looks like a typical assumption set up in a modern situation of developing a mathematical model for some specific aim – in other words, a typical hypothesis whose truth cannot be directly judged. It is accepted as true insofar as the consequences that can be derived from it are desirable and supported by empirical evidence. No known source has discussed the epistemological status of the axiom and its justification in this way. Rather, Archimedes' hydrostatics was considered as a theory that as a whole made sense and agreed with (technical) experience.

Considered in their entirety, the axiomatic-deductive theories that the Greeks set up during the third century BC clearly rest on hypotheses that vary greatly in regard to the justification of their respective claims of being true. Some of these hypotheses seem so intuitively safe that a "healthy mind" cannot doubt them; others have been accepted as true because the theory founded on them made sense and agreed with experience.

In sum, ancient Greek thinking had two ways of justifying a hypothesis. First, an axiom or a hypothesis might be accepted as true because it agrees with intuition. Second, hypotheses inaccessible to direct intuition and untestable by direct inspection were justified by drawing consequences from them and comparing these with the data to see whether the consequences were desired; that is, they agreed with experience or with other statements taken for granted. Desired consequences led to strengthening the hypothesis, undesired consequences to its weakening. Mittelstrass (1962) wants to limit this second procedure to the narrow context of ancient astronomy. I follow Szabó (1960) in seeing it also inherent in the broader philosophical and scientific discourse of the Pre-Platonic and Pre-Aristotelean period.

2.3 Intrinsic and Extrinsic Justification in Mathematics

From the times of Plato and Aristotle to the nineteenth century, mathematics was considered as a body of absolute truths resting on intuitively safe foundations. Following Lakatos (1978), we may call this the *Euclidean* view of mathematics (in M. Leng's chapter, this volume, this is called the "assertory approach"). In contrast, modern mathematics and its philosophy would consider the axioms of mathematics simply as statements on which mathematicians agree; the epistemological qualification of the axioms as true or safe is ignored. At the end of the nineteenth century, C. S. Peirce nicely expressed this view: "… all modern mathematicians agree … that mathematics deals exclusively with hypothetical states of things, and asserts no matter of fact whatever." (Peirce 1935, 191). We call this modern view the *Hypothetical* view.

Mathematical proof underwent a foundational crisis at the beginning of the twentieth century. In 1907, Bertrand Russell stated that the fundamental axioms of mathematics can only be justified not by an absolute intuition but by the insight that one can derive the desired consequences from them (Russell 1924; see Mancosu 2001, 104). In discussing his own realistic (or in his words "Platonistic") view of the nature of mathematical objects, Gödel (1944) supported this view:

> The analogy between mathematics and a natural science is enlarged upon by Russell also in another respect … He compares the axioms of logic and mathematics with the laws of nature and logical evidence with sense perception, so that the axioms need not necessarily be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these "sense perceptions" to be deduced: … I think that … this view has been largely justified by subsequent developments. (Gödel 1944, 210)

On the basis of Gödel's "Platonistic" (realistic) philosophy, the American philosopher Penelope Maddy has designated justification of an axiom by direct intuition as "intrinsic", and justification by reference to plausible or desired consequences as "extrinsic" (Maddy 1980). According to Maddy, Gödel posits a faculty of mathematical intuition that plays a role in mathematics analogous to that of sense perception in the physical sciences:

> … presumably the axioms [of set theory: Au] force themselves upon us as explanations of the intuitive data much as the assumption of medium-sized physical objects forces itself upon us as an explanation of our sensory experiences. (Maddy, 1980, 31)

For Gödel the assumption of sets is as legitimate as the assumption of physical bodies, Maddy argues. Gödel posited an analogy of intuition with perception and of mathematical realism with common-sense realism. If a statement is justified by referring to intuition Maddy calls the justification *intrinsic*. But this is not the whole story. As Maddy puts it:

> Just as there are facts about physical objects that aren't perceivable, there are facts about mathematical objects that aren't intuitable. In both cases, our belief in such 'unobservable' facts is justified by their role in our theory, by their explanatory power, their predictive success, their fruitful interconnections with other well-confirmed theories, and so on. (Maddy, 1980, 32)

In other words, in mathematics as in physics, one can justify some axioms by direct intuition (intrinsic), but others only by referring to their consequences. The acceptance of the latter axioms depends on evaluating their fruitfulness, predictive success and explanatory power. Maddy calls this type of justification *extrinsic justification*.

Maddy enlarged on the distinction between intrinsic and extrinsic justification in two ways. First, she discussed perception and intuition (1980, 36–81), trying to sketch a cognitive theory that explains how human beings arrive at the basic intuitions of set theory. There she attempted to give concrete substance to Gödel's rather abstract arguments. Second, she elaborated on the interplay of intrinsic and extrinsic justifications in modern developments of set theory (1980, 107–150). Mathematical topics treated are measurable sets, Borel sets, the Continuum hypothesis, the Zermelo–Fraenkel axioms, the axiom of choice and the axiom of constructability. She found that as a rule there is a mixture of intrinsic and extrinsic arguments in favor of an axiom. Some axioms are justified almost exclusively by extrinsic reasons. This raises the question of which modifications of axioms would make a statement like the continuum hypothesis provable and what consequences such modifications would have in other parts of mathematics. Here questions of weighing advantages and disadvantages come into play; these suggest that in the last resort extrinsic justification is uppermost.

Maddy succinctly stated the overall picture which emerges from her distinction:

> … the higher, less intuitive, levels are justified by their consequences at lower, more intuitive, levels, just as physical unobservables are justified by their ability to systematize our experience of observables. At its more theoretical reaches, then, Gödel's mathematical realism is analogous to scientific realism.

Thus Gödel's Platonistic epistemology is two-tiered: the simpler concepts and axioms are justified intrinsically by their intuitiveness; more theoretical hypotheses are justified extrinsically, by their consequences. (Maddy 1980, 33)

In conclusion, until the end of the nineteenth century, mathematicians were convinced that mathematics rested on intuitively secure intrinsic hypotheses which determined the inner identity of mathematics. Extrinsic hypotheses could occur and were necessary only outside the narrower domain of mathematics. This view dominated by and large the philosophy of mathematics. Then, non-Euclidian geometries were discovered. The subsequent discussions about the foundations of mathematics at the beginning of the twentieth century resulted in the decisive insight that pure mathematics cannot exist without hypotheses (axioms) which can only be justified extrinsically. Developments in mechanics from Newton to the nineteenth century enforced this process (see Pulte 2005).

Today, there is a general consensus that the axioms of mathematics are not absolute truths that can be sanctioned by intuition: rather, they are propositions on which people have agreed. A formalist philosophy of mathematics would be satisfied with this statement: however, modern realistic or naturalistic philosophies go further, trying to analyse scientific practice inside and outside of mathematics in order to understand how such agreements come about.

2.4 Implications for the Teaching of Proof

As we have seen, the "Hypothetical" view of modern post-Euclidean mathematics has a high affinity with the origins of proof in pre-Euclidean Greek dialectics. In dialectics, one may suppose axioms or hypotheses without assigning them epistemological qualification as evident or true. Nevertheless, at present the teaching of proof in schools is more or less ruled by an implicit, strictly Euclidean view. When proof is mentioned in the classroom, the message is above all that proof makes a proposition safe beyond doubt. The message that mathematics is an edifice of absolute truths is implicitly enforced, because the hypotheses underlying mathematics (the axioms) are not explicitly explained as such. Therefore, the hypothetical nature of mathematics remains hidden from most pupils.

This paper pleads for a different educational *approach to proof based on the modern Hypothetical view* while taking into account its affinity to the early beginnings in Greek dialectics and Greek theoretical science. This approach stresses the *relation between a deduction and the hypotheses* on which it rests (cf. Bartolini Bussi et al. 1997 and Bartolini 2009). It confronts pupils with situations in which they can *invent* hypotheses and *experiment* with them in order to understand a certain problem. The problems may come from within or from outside mathematics, from combinatorics, arithmetic, geometry, statics, kinematics, optics or real life situations. Any problem can be become the subject of a dialog or of a procedure in which hypotheses are formed and consequences are drawn from them. Hence, from the outset pupils see proof in the context of the hypothetico-deductive method.

There are mathematical and pedagogical reasons for this approach. The *mathematical* reasons refer to the demand that instruction should convey to the pupils an *authentic and adequate image of mathematics* and its role in human cognition. In particular, it is important that the pupils understand the differences and the connections between mathematics and the empirical sciences, because frequently proofs are motivated by the claim that one cannot trust empirical measurements. For example, students are frequently asked to measure the angles of a triangle, and they nearly always find that the sum of the angles is equal to 180°. However, they are then told that measurements are not precise and can establish that figure only in these individual cases. If they want to be sure that the sum of 180° is true for all triangles they have to prove it mathematically. However, for the students (and their teachers) that theorem is a statement about real (physical) space and used in numerous exercises. As such, the theorem is true when corroborated by measurement. Only if taking into account the fundamental role of measurement in the empirical sciences, can the teacher give an intellectually honest answer to the question of why a mathematical proof for the angle sum theorem is urgently desirable. Such an answer would stress that in the empirical sciences proofs do not replace measurements but are a means for building a network of statements (laws) and measurements.

The *pedagogical* reasons are derived from the consideration that the teaching of proof should explicitly address two question: (1) What is a proof? (2) Where do the axioms of mathematics come from?

Question (1) is not easy and cannot be answered in one or two sentences. I shall sketch a genetic approach to proof which aims at explicitly answering this question (see Jahnke 2005, 2007). The overall frame of this approach is the notion of the hypothetico-deductive method which is basic for all sciences: by way of a deduction, pupils derive consequences from a theory and check these against the facts. The approach consists of three phases, a first phase of *informal thought experiments* (Grade 1+); a second phase of *hypothetico-deductive thinking* (Grade 7+); and a third phase of *autonomous mathematical theories* (upper high school and university). Students of the third phase would work with closed theories and only then would "proof" mean what an educated mathematician would understand by "proof."

The first phase would be characterized by informal argumentations and would comprise what has been called "preformal proofs" (Kirsch 1979), "inhaltlich-anschauliche Beweise" (Wittmann and Müller 1988) and "proofs that explain" in contrast to proofs that only prove (Hanna 1989). These ideas are well-implemented in primary and lower secondary teaching in English-speaking countries as well as in Germany.

In the second phase the instruction should make the concept of proof an explicit theme – a major difficulty and the main reason why teachers and textbook authors mostly prefer to leave the notion of proof implicit. There is no easy definition of the very term "proof" because this concept is dependent on the concept of a theory. If one speaks about proof, one has to speak about theories, and most teachers are reluctant to speak with seventh graders about what a theory is.

The idea in the second phase is to build local theories; that is small networks of theorems. This corresponds to Freudenthal's notion of "local organization" (Freudenthal 1973, p. 458) but with a decisive modification. The idea of measuring should not be dispersed into general talk about intuition; rather we should build small networks of theorems based on empirical evidence. The networks should be manageable for the pupils, and the deductions and measurements should be organically integrated. The "small theories" comprise hypotheses which the students take for granted and deductions from these hypotheses.

For example, consider a teaching unit about the angle sum in triangles exemplifying the idea of a network combining deductions and measurements (for details, see Jahnke 2007). In this unit the alternate angle theorem is introduced as a hypothesis suggested by measurements. Then a series of consequences about the angle sums in polygons is derived from this hypothesis. Because these consequences agree with further measurements the hypothesis is strengthened. Pupils learn that a proof of the angle sum theorem makes this theorem not absolutely safe, but dependent on a hypothesis. Because we draw a lot of further consequences from this hypothesis which also can be checked by measurements, the security of the angle sum theorem is considerably enhanced by the proof.

Hence, the answer to question (1) consists in showing to the pupils by way of concrete examples the relation between hypotheses and deductions; exactly this interplay is meant by proof.

Question (2) is answered at the same time. The students will meet a large variety of hypotheses with different degrees of intuitiveness, plausibility and acceptability.

They will meet basic statements in arithmetic which in fact cannot be doubted. They will set up by themselves ad-hoc-hypotheses which might explain a certain situation. They will also hit upon hypotheses which are confirmed by the fact that the consequences agree with the phenomena. This basic approach is common to all sciences be they physics, sociology, linguistics or mathematics. We have seen above that the Greeks already had this idea and called it "saving the phenomena." The students' experience with it will lead them to a realistic image of how people have set up axioms which organize the different fields of mathematics and science. These axioms are neither given by a higher being nor expressions of eternal ideas; they are simply man made.

References

Aristotle. (1966). *Posterior Analytics*. English edition by Hugh Tredennick. London/Cambridge: William Heinemann/Harvard University Press.

Ayer, A. J., & O'Grady, J. (1992). *A dictionary of philosophical quotations*. Oxford: Blackwell Publishers.

Balacheff, N. (1991). The benefits and limits of social interaction: The case of mathematical proof. In A. J. Bishop, E. Mellin-Olsen, & J. van Dormolen (Eds.) *Mathematical knowledge: Its growth through teaching* (pp. 175–192). Dordrecht: Kluwer.

Bartolini Bussi, M. G. (2009). Experimental mathematics and the teaching and learning of proof. In *CERME 6 Proceedings*, to appear.

Bartolini Bussi, M. G., Boero, P., Mariotti, M. A., Ferri, F., & Garuti, R. (1997). Approaching geometry theorems in contexts: from history and epistemology to cognition. In *Proceedings of the 21st conference of the international group for the psychology of mathematics education (Lahti)* (Vol. 1, pp. 180–195).

Duhem, P. (1994). *Sozein ta Phainomena. Essai sur la notion de théorie physique de Platon à Galilée*. Paris: Vrin (Original publication 1908).

Freudenthal, H. (1973). *Mathematics as an educational task*. Dordrecht: Reidel.

Gödel, K. (1944). Russell's Mathematical Logic. In P. A. Schilpp (Hrsg.) *The Philosophy of Bertrand Russell* (pp. 125–153). New York: Tudor. Quoted according to: P. Benacerraf, & H. Putnam (Eds.) Philosophy of mathematics, Englewood Cliffs 1964.

Hanna, G. (1989). Proofs that prove and proofs that explain. In *Proceedings of the thirteenth conference of the international group for the psychology of mathematics education* (Vol. II, pp. 45–51). Paris: PME.

Heath, T. L. (Ed. and Transl.) (1953). *The works of Archimedes*. New York: Dover.

Heath, T. L. (Ed. and Transl.) (1956). *The thirteen books of Euclid's elements*. Second edition revised with additions. New York: Dover.

Jahnke, H. N. (2005). A genetic approach to proof. In M. Bosch (Ed.) *Proceedings of the Fourth Congress of the European Society for Research in Mathematics Education, Sant Feliu de Guíxols 2005*, 428–437.

Jahnke, H. N. (2007). Proofs and Hypotheses. *Zentralblatt für Didaktik der Mathematik (ZDM), 39*(2007), 1–2, 79–86.

Kirsch, A. (1979). Beispiele für prämathematische Beweise. In W. Dörfler, & R. Fischer (Eds.) *Beweisen im Mathematikunterricht* (pp. 261–274). Klagenfurt: Ho¨lder-Pichler-Temspsky.

Lakatos, I. (1978). A renaissance of empiricism in the recent philosophy of mathematics. In I. Lakatos (Ed.) *Philosophical papers* (Vol. 2, pp. 24–42). Cambridge: Cambridge University Press.

Lloyd, G. E. R. (1991). Saving the appearances. In G. E. R. Lloyd (Ed.), *Methods and problems in Greek science* (pp. 248–277). Cambridge: Cambridge University Press.

Lloyd, G. E. R. (2000). Der Beweis und die Idee der Wissenschaft. In J. Brunschwig, & G. E. R. Lloyd (Eds.) *Das Wissen der Griechen. Eine Enzyklopädie* (pp. 238–258). München: Wilhelm Fink.

Mach, E. (1976). *Die Mechanik. Historisch-kritisch dargestellt* (Unveränderter Nachdruck der 9. Auflage, Leipzig 1933, 1. Auflage 1883). Darmstadt: Wissenschaftliche Buchgesellschaft.

Maddy, P. (1990). *Realism in mathematics*. Oxford: Clarendon Press.

Máté, A. (2006). Árpád Szabó and Imre Lakatos, or the relation between history and philosophy of mathematics. *Perspectives on Science, 14*(3), 282–301.

Mittelstrass, J. (1962). *Die Rettung der Phänomene. Ursprung und Geschichte eines antiken Forschungsprinzips*. Berlin: Walter de Gruyter & Co.

Mancosu, P. (2001). Mathematical Explanation: Problems and Prospects. *Topoi, 20*, 97–117.

Natorp, P. (1921). *Platos Ideenlehre. Eine Einführung in den Idealismus*. Leipzig: Meiner. 1921[2].

Peirce, C. S. (1935). The essence of mathematics. In C. Hartshorne, P. Weiss (Eds.) *Collected Papers of Charles Sanders Peirce* (Vol. III, pp. 189–204). Cambridge: Harvard University Press.

Cooper, J. M. (Ed.) (1997). *Plato: Complete works*. Cambridge/Indianapolis: Hackett Publishing Company.

Proclus (1970). *A Commentary on the First Book of Euclid's Elements*. Translated, with Introduction and Notes, by Glenn R. Morrow. Princeton: Princeton University Press.

Pulte, H. (2005). *Axiomatik und Empirie. Eine wissenschaftstheoriegeschichtliche Untersuchung zur Mathematischen Naturphilosohie von Newton bis Neumann*. Darmstadt: Wissenschaftliche Buchgesellschaft.

Russell, B. (1924). Logical Atomism. In J. M. Muirhead (Ed.), *Contemporary British Philosophy, first series* (pp. 357–383). London: George Allen & Unwin.

Szabó, Á. (1960). *Anfänge des Euklidischen Axiomensystems*. Archive for History of Exact Sciences 1, 38–106. Page numbers refer to the reprint in O. Becker (Ed.) (1965) Zur Geschichte der griechischen Mathematik (pp. 355–461). Darmstadt: Wissenschaftliche Buchgesellschaft.

Toomer, G. J. (Ed. and Transl.) (1984). *Ptolemy's Almagest*. London: Duckworth.

van der Waerden, B. L. (1988). *Science awakening I, Paperback edition*. Dordrecht: Kluwer.

Wittmann, E. C., & Müller, G. (1988). Wann ist ein Beweis ein Beweis. In P. Bender (Ed.) *Mathematikdidaktik: Theorie und Praxis. Festschrift für Heinrich Winter* (pp. 237–257). Berlin: Cornelsen.

Chapter 3
Lakatos, Lakoff and Núñez: Towards a Satisfactory Definition of Continuity

Teun Koetsier

3.1 Introduction

In Thomas Heath's translation of Pappus' words the heuristic method of analysis and synthesis to prove a conjecture is described as follows:

> …assume that which is *sought* as if it were (already) done, and we inquire what it is from which this results, and again what is the antecedent cause of the latter, and so on, until *by retracing our steps we come upon something* already known or belonging to the class of first principles, and such a method we call analysis as being solution *backwards*. But in synthesis, reversing the process, we take as already done that which was last *arrived at* in the analysis and, by arranging in their natural order as consequences what were before antecedents, and successively connecting them one with another, we *arrive* finally at the construction of what was *sought*; and this we call synthesis.[1]

In this well-known quotation, we are obviously dealing with metaphorical language (italicized by me – T. K.). The imagery is derived from the way we orient ourselves in space: we are looking for something, we retrace our steps, we come upon something, we go backwards, we arrive somewhere, and so forth.

Of course, one could argue that the metaphor concerns merely the methodology of mathematics and not the ontology. From a Platonist point of view, the metaphor describes a mental search in a realm independent of the human mind. Platonism, however, is vexed by an essential problem: the way in which the mind has access to that realm is a mystery. Platonists seem to be inclined to simply accept as given the fact that our mind has this access. Lakoff and Núñez (2000) have defended a completely different position. Their view is that all of mathematics is a conceptual system created by the human mind on the basis of the ideas and modes of reasoning grounded in the sensory motor system. This creation takes place through conceptual metaphors.

T. Koetsier (✉)
Department of Mathematics, Faculty of Science, Vrije Universiteit, Amsterdam, The Netherlands
e-mail: t.koetsier@few.vu.nl

Parts of this paper are based on Koetsier (1995).

[1] Quoted in Lakatos (1961/1973, p. 73).

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_3,

Let us consider a simple example. A man buys a present for his wife because he has decided to "invest more in his relationship with her." In principle, this metaphor from the world of business (the source domain of the metaphor) generates a whole new way of viewing the man's private life (its target domain). Lakoff and Núñez have argued that such metaphors are much more than merely ways of adding color to our language. Not only can they add to existing target domains but they can create new conceptual domains as well. According to Lakoff and Núñez, for example, elementary arithmetic is a conceptual system generated by four grounding metaphors: object collection, object construction, the measuring stick and motion along a path. Moreover, as the Pappus quotation above illustrates, our investigation of the conceptual domains thus created is based on metaphors as well.

In this paper I wish to explore the views of Lakoff and Núñez by means of some remarks on the history and prehistory of the intermediate value theorem in analysis[2]. The notion of continuity plays an essential role in the story. Elsewhere, I have studied this history from a Lakatosian point of view (Koetsier 1995). In this paper I will combine the two points of view. I view mathematics as a conceptual system generated by means of conceptual metaphors. This system is subsequently subjected to processes of explicitation and refinement, which take place along the lines sketched by Imre Lakatos in *Proofs and Refutations* (1976).

Following Lakatos, I will stretch the meaning of the notions of analysis and synthesis, in the sense that I will include the possibility that the analysis may lead to new notions or new definitions of already existing notions or to new axioms of Koetsier (1991).

3.2 Proposition I of Book I of Euclid's *Elements*

3.2.1 Proposition I of Book I and the Euclidean Metaphor

The intermediate value theorem in analysis is related to the first proposition of Book I of Euclid's *Elements*. This proposition concerns the well-known construction of an equilateral triangle on a given segment: the top of the triangle is constructed by intersecting two circles C_1 and C_2. The construction is based on Postulates 1 and 3 of Book I, which guarantee the possibilities to draw the straight line segment that connects two points and to draw the circle that has a given point as center and passes through another given point.

It is possible to explain the Postulates 1 and 3 to a pupil by means of rope-stretching. Given two points, I can stretch a rope between those two points (Postulate 1). If I keep one of the two points fixed and rotate the other endpoint keeping the rope stretched, I can also draw the circle of which the existence is guaranteed by Postulate 3. This rope-stretching is an activity belonging to everyday reality.

If Lakoff and Núñez are right, the step from this everyday reality (the source domain) to the situation in Euclid's *Elements* (the target domain) is an example of

[2] Spalt (1988) also deals with the history of the intermediate value theorem.

a metaphor: we lift a conceptual system from its context, modify it, and thus create a new domain. In the Euclidean target domain, points have no parts and lines have no width; moreover, all possible real-life problems that could in reality disturb the rope-stretching are excluded. I will call the metaphor involved in the creation of the Euclidean conceptual system the *Euclidean Metaphor*.

3.2.2 The Basic Metaphor of (Actual) Infinity (BMI)

Imagine we want to teach arithmetic to a child. We give the child a huge bag full of marbles and tell the child that any individual isolated marble is called "One." Two, Three, Four … are defined as the names of the corresponding ordered sets of marbles. Once this is clear we say: "You now know what numbers are; the collection of all numbers that can be created in this way is precisely the collection of all the ordered sets of marbles that you can create in this way."

Of course, in reality the bag contains a finite number of marbles and a clever child might say: "There is a biggest number; it is the number consisting of all the marbles in the bag!" Let us, however, assume that the bag is a magical bag that never gets empty. Then we can create arbitrarily big numbers.

As soon as we consider as given the infinite set of numbers that we can create in this way, we apply what Lakoff and Núñez (2000) have called the *Basic Metaphor of Infinity* (BMI). Thus abstracting ourselves from all kinds of possible disturbing circumstances, we are then ready to do number theory with an actually infinite set of natural numbers. When we discuss infinite sequences and their limits, we will need the BMI. The essence of the BMI is that a process that can be indefinitely iterated, and of which the completion is beyond imagination, is nevertheless understood as completed. This is remarkable but not in itself problematic: in a sense, points without parts and lines without width cannot be imagined either. Yet, the application of the BMI in practice can easily lead to contradictions. Lovely examples of such contradictions occur in the area of so-called supertasks: What happens if infinitely many well-defined tasks are all executed in a finite period of time? Cf. Allis and Koetsier (1991, 1995, 1997).

The occurrence in the past of the paradoxes of infinity in set theory only supports my point that, if we view mathematics as a conceptual system generated by means of metaphors like the BMI, we should be aware of the fact that the conceptual system is subsequently subjected to processes of explicitation and refinement as described by Lakatos.

3.2.3 The "Tacit Assumption"

The construction and proof of Proposition I of Book I were not questioned for many centuries. Postulate 3 guarantees the possibility to generate circles by means of motion. The generating motion is so obviously continuous that the thought of circumferences with holes in them does not even occur. The existence of the point of intersection of the two circles created by rope-stretching is automatically carried over in the Euclidean Metaphor. Yet, from a modern point of view there is a tacit assumption involved in the construction. A possible explicitation of this assumption is the following: if the circumference of a circle C_1 partly lies inside another circle C_2 and partly outside that other circle, the circumference of C_1 will intersect the circumference of C_2. This assumption is part of the prehistory of the intermediate value theorem in analysis.

3.3 Leibniz' Proof Generated Definition of a Continuum[3]

In a paper presumably written in 1695[4] and posthumously published, *Specimen geometriae luciferae* (1695), Leibniz phrases the general mathematical fact involved as follows: "And in general: if some continuous line lies in some surface, in such a way that part of it is inside and part of it is outside a part of the surface, then this [line] will intersect the periphery of that part."[5] The fundamental notion involved is the notion of continuity, which Leibniz defines in this way:

> The continuum is a whole with the property that any two parts (that together make up the whole) do have something in common, and in such a way that if they do not overlap, which means that they have no part in common, or if the totality of their size equals the whole, they at least possess a common border.[6]

In Leibniz's opinion, the hidden assumption in Euclid's argument can be proved trivially on the basis of this definition. His argument, accompanied by a figure, runs as follows:

> However, we can also express this with a kind of calculation. Let $\underline{Y}$ be part of some manifold (See Fig. 3.1) and let every individual point that falls within this part $\underline{Y}$ be called with the general name Y; every point, however, of this manifold that falls outside this part will be called with the general name Z, and everything belonging to the manifold outside of $\underline{Y}$ will be called $\underline{Z}$.

[3] One of the referees pointed out that the universal validity of the intermediate value principle was debated long before Leibniz in connection with the concept of angle. Horn-like or cornicular angles, like the one between a tangent and the circumference of a circle, can be increased indefinitely by decreasing the radius of the circle. Yet, in comparison with 'ordinary' angles their behavior is paradoxical: although they can be increased indefinitely they are nevertheless smaller than any acute angle (cf. Heath, 1956, pp. 37–43). The same referee wrote that there is a clear resemblance between Leibniz' definition of continuity and Aristotle's view: "things are called continuous when the touching limits of each become one and the same and are, as the word implies, contained in each other: continuity is impossible if these extremities are two" (*Physics* V 3, Translated by R. P. Hardie and R. K. GayeCf. http://classics.mit.edu/Aristotle/physics.html). This is absolutely correct and suggests that Leibniz may have been influenced by Aristotle.

[4] Cf. Müller and Kronert (1969, p. 136).

[5] Leibniz (1695, p. 284).

[6] Leibniz (1695, p. 284).

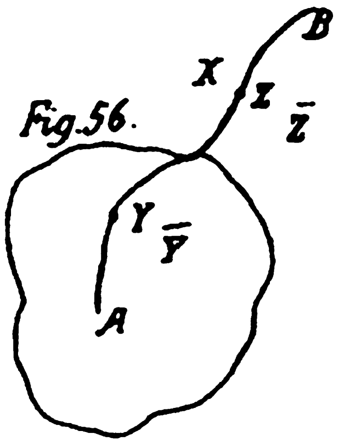


Fig. 3.1 Leibniz put bars above the letters, instead of underneath them, as I do

It is then clear that the points that are on the boundary of the part $\underline{Y}$, belong both to $\underline{Y}$ and to $\underline{Z}$, which means that one can say that a certain Y is a Z and a certain Z is a Y. The whole manifold consists in any case of $\underline{Y}$ and $\underline{Z}$ together, or is equal to $\underline{Y}$+$\underline{Z}$,[7] so that every point is either a Y or a Z, be it that some points are both Y and Z. Let us now assume that there is given another, new manifold, for example AXB, that lies in the given manifold $\underline{Y}$+$\underline{Z}$ and let us call this new manifold generally $\underline{X}$. It is a priori clear that every X is a Y or a Z. However, if on the bases of the data it can be established that a certain X is a Y (e.g. A falls within $\underline{Y}$) and on the other hand some X is Z (e.g. B falls outside of $\underline{Y}$ and consequently in $\underline{Z}$), then it follows that some X is simultaneously a Y and a Z.[8]

Briefly, the argument seems to be: Let W be a continuum and let in accordance with the definition, Y and Z represent a split of W into two parts without overlap, with a common border B_1 consisting of all points that belong both to Y and to Z. Let moreover a continuum X be a part of W that has points in common with both Y and Z. Then X consists of two parts, a part in Y and a part in Z. Those two parts possess on the basis of the definition of a continuum a common border B_2 in X and B_2 must be part of B_1. In the first proposition of Euclid's *Elements* B_1 is the circumference of a circle and X is the circumference of another circle. B_2 is a point belonging to X but also to B_1.

Leibniz' argument is a nice attempt to deduce the tacit assumption in Proposition I of Book I from a general definition of a continuum. This definition was probably "proof-generated": Leibniz found the definition in an analysis that started from what he wanted to prove.

Leibniz underestimated the problem of the characterization of the common border of the two parts of a continuum. Yet, against the background of the seventeenth

[7] Leibniz wrote Y, Z and Y+Z, in this sentence, but must have meant $\underline{Y}$, $\underline{Z}$ and $\underline{Y}$+$\underline{Z}$.

[8] Leibniz (1695, pp. 284-285)

century, his is a remarkable attempt and it would take more than a century before others would do something similar.

Leibniz had a great interest in logic. In the view of mathematics defended by Lakoff and Núñez, logic is based on metaphors as well. Consider an everyday situation: There is a bottle in a barrel and there are ants in the bottle; then we know that the ants are in the barrel. Our conceptual system concerning positions in space yields: if an object is in A and A is in B, then the object is in B. Logicians and mathematicians metaphorically use this scheme, for example, in the form of the syllogism Barbara: If all X are Y and all Y are Z, then all X are Z. They apply Barbara and other rules of deduction to the domains that they have created through the application of other metaphors, like the Euclidean Metaphor or the Basic Metaphor of Infinity.

Leibniz' paper shows some characteristics of seventeenth-century mathematics. His use of the word "calculation" (another metaphor) with respect to the proof is revealing. The idea of reducing thought to calculation was very much in the air. However, solving the problem of the characterization of the common border of two nonoverlapping parts of a continuum would require much more complicated metaphors involving limits of point sequences. The solution would require quite a journey in analysis, as we will see.

3.4 The Intermediate Value Theorem in Analysis in the Eighteenth Century

3.4.1 Euler

In order to discuss the intermediate value theorem in analysis from the perspective of Lakoff and Núñez (2000), I need to make some remarks on analytical geometry. The Euclidean Metaphor gives us Euclidean geometry. In order to get to analytical geometry, we need two number lines that enable us to map points, lines and curves on pairs of numbers and equalities between algebraic expressions.

Lakoff and Núñez call the number line a conceptual blend: it is a blend of the Euclidean line and the real numbers. The Euclidean Metaphor gives us the Euclidean line. The concept of the number line arises from the activity of measuring in real life. The basic idea is that with any unit of measure, be it a foot or a thumb, any line has a length that can be measured as a positive number. However the rational numbers are not enough to measure any line, the measurement must include irrational numbers. The conceptual blend of line and numbers, initiated by Fermat and Descartes, created an intimate link between geometry and algebraic expressions. First, the algebraic expressions could not exist without geometry. Euler (and Lagrange) turned algebra plus calculus into "analysis" and attempted to liberate this from geometrical elements. However, even until after the middle of the nineteenth century analysis would continue to contain geometrical elements. Our story nicely illustrates this.

In Section 33 of the *Introductio in Analysin Infinitorum* (1748), Euler describes the intermediate value property for polynomials:

> If an integer function Z [i. e. a polynomial - T.K.] assumes for z=a the value A and for z=b the value B, then this function can assume all values that lie between A and B by putting for z values that are between a and b.

Euler does not prove the property; he finds it obvious.

In his *Introductio,* Euler needed the intermediate value property while dealing with roots of polynomials. He also needed it for his *Recherches sur les racines imaginaires des équations* (1751), in which he gave a proof of the fundamental theorem of algebra. The proof is preceded by a theorem representing a particular case of the intermediate value theorem: Polynomials of odd degree possess at least one real root. In his proof, Euler interprets the polynomial as a curve. He establishes the fact that one part of the curve is above the axis and another part is below it. This fact, he argues, implies that the curve necessarily cuts the axis. It is interesting that Euler in the end uses the geometrical Euclidean Metaphor in order to prove an algebraic result.

3.4.2 Lagrange

Euler's proof in fact rested upon an element alien to analysis as Euler saw it: the geometrical intermediate value property. However, at the end of the eighteenth century, in a treatise on algebraic equations, Lagrange came up with a simple proof of the intermediate value theorem for polynomials, based on the fundamental theorem of algebra. His proof, which seemed at first sight satisfactory and independent of geometry, runs as follows: He writes a polynomial as a product of linear terms. If substitution of p and of q in

$$(x-a)(x-b)(x-c)....= 0$$

gives values of different sign, then at least two corresponding factors like $(p-a)$ and $(q-a)$ must have different sign, which means that at least one root must be between p and q.[9] We find this same proof in Klügel (1805, p. 447).

Later, Lagrange remarked that the objection that he had not considered imaginary roots in his proof is not serious, because the imaginary roots correspond to positive quadratic factors, so that there is no problem there. However, this proof of the intermediate value theorem is unacceptable for quite another reason. In the second edition of his treatise, Lagrange added a note in which he rejected his first proof and came up with a new proof,[10] because the earlier proof was based upon the fundamental theorem of algebra, while the proof of the fundamental theorem of algebra that Lagrange accepted used the intermediate value theorem: so Lagrange's first proof is circular.

[9] Lagrange (1808, pp. 1–2)

[10] Lagrange (1808, pp. 101–102)

Lagrange proceeded to give another proof, which runs as follows: He represents the equation involved by:

$$P\text{-}Q=0.$$

P is the sum of the terms with a positive sign and Q is the sum of the terms with a negative sign. Substitution of the values p and q in the equation results in values of opposite sign. We must prove that there exists a value between p and q for which the equation vanishes. Lagrange now distinguishes several cases. It is enough to discuss only the first case, in which p and q are positive and in which, moreover, for $x=p$ the value of $P-Q$ is negative and for $x=q$ the value of $P-Q$ is positive. He then argues that when x increases from p to q "par tous les degrés insensibles" both P and Q will increase, also "par les degrés insensibles," but P increases more than Q, because for $x=p$ the value of P is smaller than Q and for $x=q$ the value of P is bigger than Q. According to Lagrange, there exists then necessarily between p and q a value for which P equals Q. In order to support the argument, Lagrange compares the situation to two moving objects that cover the same line in the same direction. If one of the two objects is first behind the other and afterwards in front of the other object then they must meet somewhere in between. This same proof was also given in 1797 by Clairaut (1797, pp. 251–253).

The proofs described so far illustrate the Lakatosian pattern: a conjecture is proved repeatedly by means of analysis and synthesis; the old proofs are (implicitly) criticized, rejected and replaced by new ones. Cleary, in the end Euler and Lagrange saw themselves forced to base the intermediate value theorem upon its geometrical counterpart in the Euclidean Metaphor.

3.4.3 Encontre: Continuity Related to Converging Sequences

Most eighteenth-century proofs of the intermediate value theorem were extensively criticized by Bolzano (1817), who also did give the first proof more satisfactory from a modern point of view. For reasons of space, I will not discuss that proof here. Bolzano's work in this respect was not representative of his time. He was a highly exceptional man, ahead of his time; his work, however, did not exert much influence.

Instead, we will have a look at a paper by a minor French mathematician, Encontre. Encontre first describes Lagrange's second proof, but does not criticize it on mathematical grounds. On the contrary, he even thinks that it is very rigorous. He has, however, didactic objections. He complains that his students find it difficult to compare two functions to two moving objects. The students also object to the use in some sense of infinitely small quantities, the use of which had been forbidden to them elsewhere in mathematics (Encontre 1813/14, p. 203). It is amusing to see how Encontre presents mathematical objections as didactic objections. Encontre then attempts to give an elaboration of Lagrange's proof that makes it independent of geometry. He shows in terms of inequalities that the increment of a polynomial with positive coefficients is smaller than an arbitrary given value if the increment of the independent variable remains within certain bounds. In fact, he constructs a sequence $a_1, a_2, a_3, \ldots$ converging to a limit α such that $P(a_i) - Q(a_i)$ converges to zero (P and Q are the polynomials from Lagrange's proof), from which he draws the conclusion that α is the required root.

Encontre's paper is interesting because, although he denies that he disagrees with the great Lagrange, he in fact points out a gap in Lagrange's proof and attempts to fill it using converging sequences which are in their turn handled by means of inequalities. Encontre lacked the genius to release himself from Lagrange's proof, but he seems to have been one of the first to relate continuity to the limits of converging sequences.

3.5 The Intermediate Value Theorem in the Nineteenth Century

3.5.1 The Notion of Limit and the Basic Metaphor of Infinity (BMI)

The essence of the BMI is that the set of all terms of an infinite sequence can be considered as a given existing whole. We can localize the values of such sequences (of rational or real numbers) as points on a line.

Lakoff and Núñez have shown how these aspects of the BMI have contributed to the generation of the notion of the limit of a convergent sequence (2000, Chap. 9). In informal mathematics, metaphors like "the terms approach the limit as n approaches infinity" are quite common. Tawny called this metaphor "fictive motion": lines meet at a point; functions reach a minimum, and so on. The fictive motion metaphor with respect to infinity is not without risks. For example the assumption that the limit of a series is actually reached in a process of an infinite summation means that this summation has been interpreted as a *supertask*: the execution of infinitely many acts in a finite time. The notion of supertask easily leads to contradicting assumptions as to the state reached after its execution, as Koetsier and Allis (1991, 1995, 1997) have shown.

3.6 Cauchy

Cauchy's *Cours d'analyse* (1821) played a major role in the considerable transformation of analysis in the nineteenth century[11]; the intermediate value theorem played only a minor role. As a matter of fact, in the *Cours d'analyse* Cauchy

[11] In the following reconstruction I will interpret some of Cauchy's results in accordance with the traditional view of his work. A good presentation of this view is in Grabiner (1981). Grabiner nicely shows that the idea that the notion of limit is related to methods of approximation played an important heuristic role in Cauchy's foundational work. For a rather different view of Cauchy's foundational work in analysis see Spalt (1996).

proves the theorem twice. The first proof simply uses the geometrical analog (Chapter 2). The second proof occurs in an appendix and it is related to Cauchy's definition of continuity. In Cauchy's research program, a function is no longer merely a formula but a formula that expresses the value of a dependent variable in terms of an independent variable. A formula f(x) that becomes a divergent series for all values of x is no function in Cauchy's mathematics. Cauchy precisely distinguishes continuous functions from functions possessing local discontinuities, using the following definition: A function f(x) is continuous on an interval if for all x the numerical value of the difference $f(x+\alpha) - f(x)$ decreases indefinitely with the value of α. We do not precisely know where Cauchy found this definition of continuity. However, it could be the result of an analysis given in order to prove the intermediate-value theorem in the appendix to the *Cours d'Analyse*. There Cauchy deals with methods to approximate roots of equations (1821, pp. 378–380) and he gives his second proof of the intermediate value theorem: Cauchy takes a real continuous function f(x) on an interval $x_0 < x < X$, which is such that $f(x_0)$ and f(X) have opposite signs, and proves the existence of a root as follows. He divides the interval into *m* equal parts and, going from left to right, determines (in thought) the first sub-interval $x_1 < x < X^1$ for which $f(x_1)$ and $f(X^1)$ again have opposite signs. With this sub-interval, he repeats the process. He divides it in *m* equal parts and so on. All $f(x_i)$ have the same sign as do all $f(X^i)$. In this way, he obtains sequences x_i and X^i with

$$x_0 < x_1 < x_2 < \ldots\ldots < x_n < X^n < X^{n-1} < \ldots\ldots < X^3 < X^2 < X^1 < X$$

while $X^i - x_i < (1/m)^i(X - x_0)$. The going from left to right to determine the next subinterval guarantees that all $f(x_i)$ have the same sign, as do all $f(X^i)$, but that the two signs are opposite. Because the terms of the decreasing sequence "will finish by differing as little as one would want" from the terms of the increasing sequence, one can conclude that "the general terms of the series [...] will converge to a common limit" (See the remark at the end of this section). Cauchy calls this limit a. Cauchy's definition of continuity implies that $f(a + (x_i - a)) - f(a)$ converges to zero because $(x_i - a)$ converges to zero and also that $f(a + (X^i - a)) - f(a)$ converges to zero because $(X^i - a)$ converges to zero. Therefore

$$\lim_{x_i \to a} f(x_i) = f(a) \text{ and } \lim_{X^i \to a} f(X^i) = f(a)$$

Because all $f(x_i)$ have the same sign and all $f(X^i)$ the same opposite sign, it is obvious that f(a) must be 0.

Quite possibly, Cauchy's definition of continuity in terms of converging sequences was born when he found this proof. Cauchy's point of departure was the method, well-known in his time, of finding a good approximation to a root by means of repeated subdivisions of an interval. I suggest that Cauchy applied that method and wondered what property of the function f guaranteed the existence of the root. The required property appeared to be that for all sequences $\{x_i\}$ with lim

$x_i = a$, we must have $\lim f(x_i) = f(a)$. A modern definition of continuity was born.[12] If this is what happened, Cauchy generated his definition of continuity in a Pappussian analysis. But even if he did not do it in precisely this way; the relatively sophisticated nature of the definition makes it highly probable that he generated the definition was generated in another context in a similar way. It is interesting that Cauchy actually turned a well-known method to approximate a root into a definition of continuity.

Remark: When Cauchy argues that "the general terms of the series [...] will converge to a common limit" he applies the criterion that says that every Cauchy-series converges, which occurs in the *Cours d'analyse* in the last sentence of the following quotation:

> It is necessary also [for the series to converge], for increasing values of n that [...] the sums of the quantities u_n, u_{n+1}, u_{n+2}, ..., taken, from the first, in whatever number we wish, finish by constantly having numerical values less than any assignable limit. Conversely, when these diverse conditions are fulfilled, the convergence of the series is assured.[13]

Cauchy does not prove the criterion; he finds it obviously true. Incidentally, Cauchy may have generated the criterion by means of an analysis, in the sense of Pappus, when trying to prove the convergence of $1 - 1/2 + 1/3 - 1/4 + 1/5$ etc. using its telescoping property (1821, pp. 130–131).

3.6.1 Heine

Heine's *Die Elemente der Functionenlehre* (1872), based upon Weierstrass' oral teaching, represents the next phase in the history of the intermediate value theorem. Cauchy still took the completeness of the real numbers for granted. Heine doesn't. Heine gives the definition of the real numbers in the nowadays well-known way by means of Cauchy-sequences of rational numbers, and proves the completeness by showing that repetition of the process does not lead to new numbers. Heine's proof of the intermediate value theorem is similar to Cauchy's, with one major difference: Where Cauchy uses implicitly the completeness of the real number system Heine bases himself upon his definition of the real numbers; the sequence that he constructs is the root he looks for.

With Heine's proof we have reached the kind of proof that could still appear in a modern textbook (perhaps slightly rephrased). The proof also shows how criticism led to analyses reaching further back towards the foundation of the real number system. From the peripheral position it had in the eighteenth century, the intermediate value theorem moved towards a much more central position in nineteenth-century analysis.

---

[12] I am not the first one to suggest that Cauchy's definition of continuity was born here. Cf. Daval and Guilbaud (1945, p. 117).

[13] Cauchy (1821, pp. 115–116)

Heine's definition of the real numbers is mathematically equivalent to Dedekind's definition by means of Dedekind-cuts. Yet, metaphorically it is very different. Heine characterizes irrational numbers by means of converging sequences. Dedekind did it differently. Dedekind records that he created his definition in 1858, when he attempted to characterize the "essence of the continuity" of a straight line. After he had read Heine's paper, he decided to publish his results. Dedekind had noticed that each point of a straight line yields a split of the line into two parts, left and right, such that each point of the left part lies to the left of each point of the right part. He defined the essence of the continuity of the straight line by turning the statement around:

> If all points of a straight line are separated into two classes in such a way that every point of the first class is on the left side of every point of the second class, then there exists one point, and only one, which brings about this separation into two classes, this cutting of the straight line into two pieces (1927, p. 11).

Heine and Dedekind separated the theory of the real numbers from geometry. Lakoff and Núñez (2000) have given an interesting, detailed description of the way in which they feel metaphors guided Dedekind in his work. The line is viewed as consisting of points (the "Spaces are sets of Points" metaphor; Lakoff & Núñez). We create a correspondence between the rational numbers and points of the line (the "Numbers are Points on a line" metaphor, Lakoff & Núñez). In this way, certain points on the line that do not represent rational numbers are "gaps" (actually, gaps in the rational number system). Dedekind then transfers the above characterization of the continuity of the line to the rational numbers. The gaps in the rational numbers are filled by defining the real numbers as pairs of sets of rational numbers.

Clearly, Heine and Dedekind continued where Leibniz stopped in 1795. We saw how Leibniz struggled with the notion of "continuum." Leibniz' definition is based on the idea that any split without overlap in two parts that make up the whole should be such that the two parts possess a common border. The notion of "border" and with it the notions of "inside" and "outside" remain vague in Leibniz reasoning. The problem that Leibniz attacked was solved for the straight line by Dedekind, using the possibility of ordering the points on a line. However, the general solution was given by means of the notions of modern topology; converging sequences of points play an important role. It is remarkable that the solution of an originally geometrical problem, attempted by Leibniz, required a long metaphorical detour via the development of analysis.

3.7 Conclusion

Lakoff and Núñez' view – that all of mathematics is a conceptual system created through conceptual metaphors by the human mind on the basis of the ideas and modes of reasoning grounded in the sensory motor system – deserves more attention. By interpreting things that mathematicians do all the time, like abstraction and idealization in terms of metaphors, Lakoff and Núñez connect those notions to the conceptual apparatus of cognitive psychology and linguistics in a promising way.

True, many questions remain unanswered and we may have to consider other views on what metaphors are and do (cf. e.g., Black, 1962, Way, 1991); nevertheless the basic idea is sound.

Yet, mathematical knowledge is very different from other kinds of knowledge: That is what needs further investigation. One referee for this paper wrote: "We choose metaphors according to rhetorical need, but we do not seem to have this latitude in most mathematical cases." This is correct, but I would argue that the certainty of mathematics is related to the occurrence of Lakatosian processes of refinement in the development of mathematics. Mathematics is a system of conceptual metaphors refined in a process of proofs and refutations until a rigid structure is reached. The fact that this is possible is highly remarkable, although it is no serious argument against Lakoff and Núñez' views.

Acknowledgments I am grateful to Brendan Larvor and to two anonymous referees for commenting on an earlier version of this paper.

References

Allis, V., & Koetsier, T. (1991). On some paradoxes of the Infinite. *British Journal Philosophy of Science, 42*(1991): 187–194.

Allis, V., & Koetsier, T. (1995). On some Paradoxes of the Infinite II. *British Journal Philosophy of Science, 46*(1995): 235–247.

Koetsier, T., & Allis, V. (1997). Assaying Supertasks. *Logique & Analyse, 40*(1997): 291–313.

Black, M. (1962). *Models and Metaphors*. Ithaca: Cornell University Press.

Bolzano, B. (1817). Rein analytischer Beweis des Lehrsatzes, daß zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege. Gottlieb Haase, Prag (Reprint in Czechoslovak Studies in the History of Science 12 (1981). 'Bernard Bolzano, Early Mathematical Works'. Special Issue: 417–476).

Cauchy, A. (1821). *Cours d'analyse*, Paris, Oeuvres, second series, Vol. 3.

Clairaut. (1797). *Élémens d'Algèbre par Clairaut*, 5ième édition, Tome Second, Paris An. V.

Daval, R., & Guilbaud, G.-T. (1945). *Le raisonnement mathématique.* Paris: Presses universitaires de France.

Dedekind, R. (1927). Stetigkeit und irrationale Zahlen, 5. Auflage, Braunschweig.

De Swart, H. C. M., & Bergmans, L. J. M. (eds.) (1995). *Perspectives on Negation, Essays in honour of Johan J. de Iongh on his 80th birthday*. Tilburg: Tilburg University Press.

Encontre, D. (1813/1814). Mémoire sur les principes fondamentaux de la théorie générale des équations. *Annales de Mathématiques Pures et Appliquées* Tome IV: 201–222. Available digitally: http://archive.numdam.org.

Euler, L. (1748). *Introductio in analysin infinitorum* (Vol. I). Lausanne (Euler's *Opera Omnia*, Series I, Vol. 8).

Euler, L. (1751). Recherches sur les racines imaginaires des équations. *Opera Omnia*, Series I, *6*: 78–147.

Grabiner, J. V. (1981). *The origins of Cauchy's rigorous calculus*. Cambridge, London: MIT Press.

Heath, T. (1956). *The thirteen books of the elements*, 2nd ed. New York: Dover Publications.

Heine, E. (1872). Die Elemente der Functionenlehre. *Journal für die reine und angewandte Mathematik Band 74*: 172–188.

Klügel, G. S. (1805). *Mathematisches Wörterbuch*, Band II, Leipzig.

Koetsier, T. (1991). *Lakatos' Philosophy of Mathematics*. Amsterdam: Elsevier.

Koetsier, T. (1995). Negation in the development of mathematics: Plato, Lakatos, Mannoury and the history of the intermediate-value theorem in analysis. In H. C. M. De Swart & L. Bergmans (eds.): 105–121.

Lagrange, J. L. (1808). *Traité de la résolution des équations numériques de tous les degrés*. Paris: Courcier.

Lakatos, I. (1976). *Proofs and Refutations, The Logic of Mathematical Discovery*. Ed. by J. Worrall & E. Zahar. Cambridge: Cambridge University Press.

Lakatos, I. (1961/1973). The method of analysis-synthesis. In Lakatos (1978). *Mathematics, Science and Epistemology: Philosophical Papers Volume 2*. Cambridge University Press, Cambridge 1978: 70–103.

Lakoff, G., & Núñez, R. E. (2000). *Where mathematics comes from, How the embodied mind brings mathematics into being*. New York: Basic Books.

Leibniz, G. W. (1695). Specimen geometriae luciferae. In C. I. Gerhardt (Hrsg.) *Mathematische Schriften*, Band 7, New York, 1971 (reprint of 1863 edition).

Müller, K., & Kronert, G. (1969). *Leben und Werk von Leibniz. Eine Cronologie*. Frankfurt.

Spalt, D. D. (1988). Das Unwahre des Resultatismus, Eine historische Fallstudie aus der Analysis. *Mathematische Semesterberichte, 35*(1988): 6–36.

Spalt, D. D. (1996). *Die Vernunft im Cauchy-Mythos*. Frankfurt/M: Deutsch.

Way, E. C. (1991). *Knowledge representation and metaphor*. Dordrecht: Kluwer.

Chapter 4
Preaxiomatic Mathematical Reasoning: An Algebraic Approach

Mary Leng

My interest, in this paper, is in how we should understand preaxiomatic mathematical theorizing.[1] A great deal of important mathematical theorizing clearly happens prior to the formulation of specific mathematical axioms for a given theory (after all, it was 1889 before Peano published his axioms for arithmetic, and yet plenty was known about the numbers prior to this breakthrough). But there is a view of mathematics according to which mathematical axioms contextually define their subject matter, so that what it *means* to be a natural number system is to be a system of objects satisfying the (second order) Peano axioms. This view might simply seem implausible once one recognizes the existence of perfectly meaningful preaxiomatic mathematical theorizing. Since, however, I think that the view that axioms define their subject matter has its attractions, I wish to argue that something like this view can be extended so as to deal with meaningful preaxiomatic theorizing.

Before considering preaxiomatic mathematical theorizing, it will be helpful to get clearer on what is meant by the view that axioms are really contextual definitions. Following Geoffrey Hellman (2003), we may describe the view of axioms as contextual definitions as taking an "algebraic" approach to axiomatic theories. The label "algebraic" here comes from seeing axioms as analogous to systems of equations with various unknowns. The axioms are held to "define" their primitive terms as whatever would be needed to make those axioms true, in the same way as systems of equations "define" their unknown values as whatever would be needed to make those equations hold. On this view, then, axioms are not straightforward truths about an independently specified subject matter: it would be a mistake to ask whether the axioms for, say, group theory have things right about their primitive terms G, +, and 0, just as it would be a mistake to ask whether the equation $x^2+3x+2=0$ has

M. Leng (✉)
Department of Philosophy, University of Liverpool, 7 Abercromby Square, Liverpool, L69 7WY, UK
e-mail: mcleng@liv.ac.uk

[1] The label 'preaxiomatic' may suggest the expectation that such theories will eventually receive an axiomatization. There is, however, scope for important mathematical reasoning that never becomes axiomatized. I take the considerations of this paper to apply to such theories as well as to theories that eventually succumb to axiomatization.

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives*, DOI 10.1007/978-1-4419-0576-5_4,

things right about x. The primitive terms act as place holders for the many possible "solutions" to the "equations" set by the axioms.

In order to understand the motivation for the view that *all* axioms are to be understood "algebraically," it will be helpful to compare the "algebraic" understanding of axiom systems to an alternative role that axioms may be thought to have. While it is fairly uncontroversial to view the axioms of group theory as contextual definitions which tell us what would have to be true of any system of objects in order for it to count as a group, it is not so clear that all axiom systems should be viewed as contextual definitions in this manner. A standard view of axiom systems for arithmetic or set theory, for example, is that these axioms should be viewed as assertions of truths about a particular subject matter – *the* natural numbers, or *the* sets – not just defining what would have to be true of a system of objects to count as an example of a natural number system or a system of ZFC-sets. Again, borrowing from Hellman, we can label the view of axioms as assertions of truth as an "assertory" approach to axiomatic theories.

A natural view of axiomatic mathematical theories is, then, that while for *some* such theories (such as group theory) the axioms are only to be read algebraically, as defining their primitive terms, in at least some important cases (such as number theory or set theory), the primitive terms should be viewed as understood independently of the axiomatization, and the axioms should be taken as assertions expressed in those previously understood terms, which may be true or false of their intended interpretation. Indeed, so natural is this dual-view of the role of axioms that it might be hard to see why anyone would wish defend one of the two views as *the* unique, correct view of all kinds of mathematical axioms. Nevertheless, looking at the history of discussion of the nature of axioms, we do find this issue under debate, most prominently in the famous correspondence between Frege and Hilbert over the nature of axioms (reprinted in Gabriel et al. 1980).

Writing to Hilbert, Frege takes a recognizably assertory view of axioms, arguing that an algebraic approach to axioms is incoherent. According to Frege,

> axioms and theorems can never try to lay down the meaning of a sign or word that occurs in them, but it must already be laid down. (Frege to Hilbert 27/12/1899)

(Insofar as apparent axiom systems, such as the axioms for group theory, are best understood as contextual definitions, then, presumably on this view they should be turned into explicit definitions expressed against the backdrop of a well-understood assertory theory, e.g., ZFC set theory.) On the other hand, Hilbert defends an algebraic approach to *all* axiom systems, holding that axioms provide the only possible way of fixing the meaning of mathematical concepts.

> In my opinion a concept can be fixed logically only by its relation to other concepts. These relations, formulated in certain statements, I call axioms, thus arriving at the view that axioms (perhaps together with propositions assigning names to concepts) are the definitions of the concepts. (Hilbert to Frege 22/9/1900)

Even in the case of a "backdrop" theory such as ZFC set theory, within which other mathematical concepts can be defined, the meaning of the basic notions (in this case, set and member) is ultimately, on Hilbert's view, given contextually by their role in axioms.

These two perspectives on mathematics carry with them two quite different views on the nature of (pure) mathematical practice. On a Fregean assertory view, in proving mathematical theorems from axioms we are attempting to establish truths about particular mathematical objects, using concepts that are graspable independently of any axiomatization we may have. And although axioms may express the results of our most basic intuitions about the concepts and objects they concern, there is room for error in this regard: so long as we view axioms as attempted assertions of truth about particular objects, it is at least theoretically possible for us to be mistaken in holding those axioms to be true. On the other hand, if axioms are, as a global algebraic approach would claim, contextual definitions of their primitive terminology, defining what would have to be true of any system of objects for it to count as, for example, a natural number system, then it does not make sense to ask whether our axioms are really true, so the truth of axioms can never be at issue in pure mathematical practice. So long as a system of axioms are consistent, those axioms will suffice to characterize some concept or other, and in proving a mathematical theorem from those axioms, all that we establish is that, *if* some system of objects satisfies the axioms, the theorem proved will likewise be true when interpreted as talking about those objects. While we may still be concerned about the interest/applicability of a concept characterized by axioms, the question of the absolute "truth"[2] of theorems proved concerning concepts so-characterized, divorced from these questions of interest/applicability, will be ill-formed.[3] Our interest, in proving mathematical theorems, is not in discovering truths about any particular objects, but rather, in discovering the consequences of our mathematical assumptions, which tell us what would have to be true of any particular objects that did happen to satisfy our axioms.

This said, we can now consider why a global algebraic picture, if sustainable, might be attractive as a view of mathematics. There are two features of the assertory view of axioms that make that view vulnerable to some important difficulties (presented in their most well known formulations by Paul Benacerraf in his influential (1965) and (1973) papers). First of all, the view of axioms as assertions about some particular subject matter leads us to ask, precisely *which* objects are the proper subject matter of a given axiomatic theory? Take the Peano axioms for arithmetic. As Benacerraf (1965) points out, there are many different systems of objects which satisfy these axioms. Yet, on a standard assertory view, only *one* such system can count as the proper subject matter about which the Peano axioms assert truths. But there seems to be nothing in our mathematical practices that would allow us to

[2]As opposed to truth relative to a particular interpretation of the theory's axioms.

[3]For this reason, one should be wary of Hilbert's rather misleading characterization of consistent axioms as true by definition. According to Hilbert, "if the arbitrarily given axioms do not contradict each other with all their consequences, then they are true and the things defined by the axioms exist. This is for me the criterion of truth and existence." (Hilbert to Frege, 29/12/1899) The concepts of truth and existence in mathematics are, for Hilbert, quite different from the concepts of truth and existence as we know them elsewhere. So different that I think it is preferable to speak of consistency and existence-according-to-a-consistent-theory, rather than hijacking terminology that to most minds has more substantial implications than this.

make any nonarbitrary choice about *which*, amongst the many candidate ω-sequences that are available to us, we mean to pick out when we talk about *the* natural numbers. So on an assertory view of the Peano axioms, while it is claimed that singular terms such as 0 refer to unique objects, it appears entirely mysterious how this reference is achieved, and the question of which objects such terms refer to seems forever beyond our grasp.

The second difficulty is a result of the room that the assertory view leaves for error. Since axioms are, on this view, attempts to assert truths about an independently existing subject matter, it makes sense to ask: How can we know that those axioms are indeed true? And since the kinds of objects that our axioms are aiming to characterize are, on most views, *abstract*, it even makes sense to ask: How can we know *anything* about such objects? Benacerraf (1973) presents this worry in the context of a causal theory of knowledge: since abstract objects are acausal, the causal theory would seem to make knowledge of such objects impossible. But even if we abandon the causal theory, a worry remains for those who wish to adopt an assertory view. Defenders of such a view would need to say *something* about the link between our mathematical beliefs (as expressed in axioms) and their intended subject matter, in order to provide grounds for their assumption that those beliefs reliably reflect their subject matter (this way of putting the worry is due to Hartry Field (1989)). But, at least on the standard (negative) characterization of mathematical objects as abstract (nonspatiotemporal, mind- and language-independent), there is very little room for any positive account of how this link may be achieved.

These worries, of course, are well known, and have set the stage for a great deal of recent work in the philosophy of mathematics. What is interesting, from the perspective of the distinction we have drawn between algebraic and assertory approaches to mathematics, is how little they affect the algebraic approach. Take Benacerraf's first worry. The problem of saying precisely *which* objects our axiomatic mathematical theories are talking about is avoided completely: insofar as the axioms of a given theory can be interpreted as truths about a particular system of objects, that system will count as just one of many potential instances of the concept contextually defined by the axioms. There is no need, on an algebraic view, to interpret axioms as talking about any particular system of objects. Indeed, given that the algebraic view sees axiom systems as analogous to systems of equations with several unknowns, it should not be surprising that such systems are amenable to multiple adequate systems of solutions.

The second worry is also easily dealt with on an algebraic approach. Since, on this view, axioms contextually define their subject matter, there is no substantial question of whether the intended subject matter of an axiom system actually satisfies the axioms. If a system of objects *didn't* satisfy the axioms of a given theory, then those axioms wouldn't have been talking about those objects in the first place. And this is true whatever the nature of the objects in question. If there are any abstract objects, then there may be ones which satisfy the Peano axioms, and if so, the Peano axioms automatically assert truths about those objects. But if there are no abstract objects, or none which satisfy the Peano axioms, this makes no difference to the algebraist's ability to view the axioms of the theory as contextual definitions which place conditions on what would have to be true of any system of objects in order to count as a natural number system. There can be perfectly meaningful job descriptions even if *no one* exists who fits the role.

If a global algebraic view of mathematical theories were plausible, then, we could avoid two of the major difficulties which have plagued the philosophy of mathematics in recent years. This is reason enough, I think, to consider whether such a view is plausible. But despite its advantages, there are some significant difficulties that any algebraic approach to mathematics would have to deal with.

Amongst these difficulties are those which arise when one considers cases where we might want to make genuine assertions about mathematical objects. According to the algebraic approach, when we *appear* to make assertions about mathematical objects in our mathematical theorizing, we are actually just developing our characterization of a mathematical concept. For example, when we appear to prove that there are infinitely many prime numbers, all that is really shown is that, if a system of objects satisfies the Peano axioms, it will contain infinitely many objects satisfying the description "prime number," something which we can say while remaining agnostic about whether there are any such systems. (To avoid potential triviality, the "if" here must be read as the modal "if" of implication, rather than the bare material conditional.) But perhaps there are some mathematical claims we will wish to assert categorically, rather than making do with these hypothetical alternatives.

Take, for example, the "if…then" claims which we have said are all that are established by mathematical proofs, on the algebraic view. We have said that an algebraist will wish to *assert* that the axioms of a theory imply its theorems. But on one understanding of implication, this should really be understood as a claim about models of the axioms: in any (set theoretic) model in which the axioms are true, the theorems are also true. So (categorical) assertions about implication become (categorical) assertions about mathematical objects after all, and hence global algebraism fails. In order to avoid this difficulty, algebraists have to reject the reduction of modal claims about implication to non-modal claims about set-theoretic models, thus introducing modal primitives into their theory.

Another case where we might think we ought to view our mathematical claims at face vale, as genuine categorical assertions, rather than as elliptic for hypothetical claims, comes when we consider our empirical scientific theories, where mixed mathematical/empirical statements are ubiquitous. In our empirical theorizing, surely our aim is to assert truths, not simply to speak hypothetically about what would be the case were there any objects that happened to fit our theory. But if so, then does this not commit us to viewing the mathematically expressed statements of our empirical theories as genuine assertions? Defenders of the algebraic view would, it seems, have to respond by either (a) explaining how we can make sense of empirical scientific theorizing even if we view the mathematically stated claims of those theories as elliptic for hypothetical claims (see, e.g., Hellman (1989)); (b) showing that the content of our empirical theories can be expressed non-mathematically (e.g., Field (1980)); or (c) reject the assumption that the aim of our empirical theories is to assert truth (e.g., Leng (forthcoming)). At any rate, there is substantial work to be done in defending global algebraism in the light of applications of mathematics.

My view is that these difficulties can be dealt with in a way that still renders the algebraic approach preferable to the assertory alternative, given the intractable worries raised by Benacerraf for that view. But aside from these concerns about whether global algebraism is possible as a view of axiomatic mathematical theories, there remains the initial worry we mentioned about the plausibility of this view in the light of the evident prominence of meaningful preaxiomatic theorizing in the history of mathematics. If it is *axioms* which give meaning to mathematical concepts, then surely preaxiomatic mathematical reasoning is, strictly, meaningless. Yet, historically, a great deal of mathematical reasoning has happened prior to axiomatization, and even in theories that have never received a formal axiomatization. Perfectly coherent mathematical explanations and even proofs have been given outside of the axiomatic setting: Euler didn't need the Peano axioms to prove that there were infinitely many prime numbers. If an algebraic approach to mathematics cannot account for this preaxiomatic mathematical theorizing as a meaningful activity, then surely, despite its advantages as a response to Benacerraf's problems, this view of mathematics is indefensible.

In glossing Hilbert's version of the view, I said earlier that Hilbert held that axiom systems were the *only* way of fixing mathematical concepts. If this gloss is correct, then the existence of perfectly meaningful preaxiomatic mathematical reasoning concerning mathematical concepts would seem to present a counterexample to this view. However, it should be noted that Hilbert himself was certainly sensitive to the importance of preformal, preaxiomatic reasoning. Indeed, in his early correspondence with Frege, Hilbert actually *agrees* with Frege's claim that formal, symbolic theorizing is not, or ought not be, the start of the story, but rather, is best introduced as the natural development of a previously understood subject matter:

> I agree especially that the symbolism must come later and in response to a need, from which it follows, of course, that whoever wants to create or develop a symbolism must first study these needs. (HILBERT to FREGE, 4/10/1895)

Isn't Hilbert then actually *agreeing* that the role of axioms is to assert truths of previously understood concepts?

Of course, one explanation of this agreement is the early date: perhaps Hilbert's view on the centrality of formal theories hardened, as he became more convinced of the inability to capture concepts through nonaxiomatic characterizations. Certainly, Hilbert despaired of some nonaxiomatic ways of picking out mathematical concepts:

> If one is looking for other definitions of a 'point', e.g., through paraphrase in terms of extensionless, etc., then I must indeed oppose such attempts in the most decisive way; one is looking for something one can never find because there is nothing there; and everything gets lost and becomes tangled and degenerates into a game of hide-and-seek. (HILBERT to FREGE, 29/12/1899)

But, whether or not Hilbert's later view did signify a change in his own attitude to preaxiomatic theorizing, there are, I think, ways of reconciling his early agreement with Frege with the spirit of the algebraic view. And since I think it would be a mistake to play down the importance of preaxiomatic mathematical thought, it is the possibility of reconciling the algebraic approach with the meaningfulness of preaxiomatic thought that I will consider here.

The algebraic approach says that *mathematical* meaning is fixed by axioms. One way of reconciling this idea with the existence of apparently meaningful preaxiomatic reasoning is to appeal to a meaning the concepts we use may have that is not strictly speaking mathematical. For example, in the case of geometry, the notions of "point" and "straight line" have an empirical usage, in picking out, for example, edges and vertices in diagrams we draw. When we reason outside of the axiomatic context about points and straight lines, we can be thought of as trying to characterize approximate truths about these ordinary objects (approximate truths, since they are only true to the extent that these objects approach some ideal), with candidate "axioms" as assertions of the more basic (approximate) truths about such things. At some point, though, the picture switches and we think of ourselves as talking about ideal points and straight lines themselves, and not the physically located points and lines we can draw in the sand. A plausible approach for an algebraist to take is to view that switch as the move to the mathematical subject-matter proper, holding that, at that point, *all* that can give meaning to the notion of an "ideal" point or straight line is what is present in our axioms. Insofar as our subject matter is approximate and empirical, our axioms and theorems can be assertory and meaningful, but once we switch to pure mathematics, we only have the axioms to go by and must at that point accept as a potential subject matter anything that fits the axioms.

This dual approach will work for some preaxiomatic reasoning, in cases where mathematical theories are recognizably developed through abstraction and idealization on empirical concepts. In such cases, nothing of the original algebraic view needs to be sacrificed. Insofar as our subject matter is *mathematical*, meaning is fixed by axioms. And insofar as we may think that there is any question about the *truth* of those axioms, this must be because we are comparing our axiomatic theories to our prior nonmathematical subject matter: Euclidean geometry can be questioned as not being true to points and straight lines in physical space, but this in no way falsifies the mathematical theory, which simply defines what it is to be a Euclidean point or straight line. But not all mathematical theories are straightforward idealizations on a previously understood empirical subject matter. Does the algebraic view have room for preaxiomatic theorizing when the subject matter is not recognizably empirical?

If we cannot rely on a prior empirical meaning to give a sense to reasoning concerning theoretical concepts prior to axiomatization, where else can we look? At this point, I think, we need to reconsider the essence of the algebraic view to see whether it can make and room for the existence of *mathematical*, rather than *empirical*, meaning prior to axiomatization. According to Hilbert, the reason for viewing axioms as contextual definitions is, as we have said, because he thinks that "a concept can be fixed logically only by its relations to other concepts." But perhaps there are nonaxiomatic characterizations of mathematical concepts that can also do the job of pinning down mathematical concepts in relation to other concepts we already grasp? Take arithmetic: we tend to characterize the natural numbers as "0, 1, 2, …," where the going on in the same way of the "…" indicates that each successive number is distinct from what has gone before and differs from its immediate successor by 1, such that any number can be reached from 0 by following a finite number of steps in this sequence. This is not an axiomatization, but it is arguable that we have a strong enough "prior" understanding of "…" for this preaxiomatic characterization to fully characterize the concept of an ω-sequence, as anything that fits this characterization. If the essence of the algebraic view is that mathematical concepts are fixed only contextually, as, if you like, "solutions" to equations in several "unknowns," then surely this essence remains even if we allow the context to be given by means other than axioms?

Indeed, if the algebraic view is extended to allow for the possibility of pinning down concepts by means of nonaxiomatic characterizations, then this makes room for what I think is quite a plausible view of some cases of mathematical theory development. For the analogy which views mathematical concepts as solutions to equations in several unknowns can be extended to formal axiomatic theories themselves: such theories are often the "solutions" found to the "equations" set by preaxiomatic desiderata.

To see how this can be, consider the example of W. R. Hamilton's discovery of the quaternions. Announced with the famous equations "$i^2=j^2=k^2=-1$," excitedly carved into Brougham Bridge in Dublin, Hamilton tells us how he "felt a *problem* to have been at that moment *solved*" (Tait 1866: 57). Hamilton had set himself the problem to find a three-dimensional analog of the two-dimensional complex numbers, preserving the usual properties of addition and multiplication (associativity, commutativity, law of the moduli, …). It turns out that this problem has no solution: something has to give. Moving to four dimensions and dropping commutativity led Hamilton to the quaternions, the closest solution "in the ball park" of what he was looking for. The formal theory of quaternions followed, but rather than being a starting point, it is itself a solution to some preaxiomatic desiderata. If we extend the algebraic view to allow for preformal, nonaxiomatic characterizations of mathematical concepts, then the example of the quaternions is grist to the algebraist's mill. For the view of mathematical proof as inquiry into what *would have to be the case* were certain assumptions also the case, can now be extended to this kind of preaxiomatic reasoning. The formal characterization of the quaternions results from inquiring into what would have to be true of a formal system so as to satisfy as many as possible of Hamilton's original desiderata, and so a place is made within the algebraic approach for mathematical practice that takes place in the run up to the development of formal axiomatic theories, as well as axiomatic mathematical theorizing.

For somewhat less glamorous examples of this kind of phenomenon, we can look to the many examples of axiomatizations which first appear in the context of *theorems*. Just as the formal characterization of the quaternions came about as a solution to a problem posed in a preaxiomatic context, formal axiom systems are likewise often presented as solutions to problems raised in the context of ordinary mathematical theorizing. To take just one example, consider the Gelfand–Naimark axioms for C^*-algebras. In their original paper presenting these axioms, Gelfand and Naimark define a *-ring (later to become known as a C^*-algebra) as follows:

A normed ring R will be called a *-ring if to every $x \in R$ there corresponds an element $x^* \in R$ satisfying the following conditions:

1′. $(\lambda x + \mu y)^* = \bar{\lambda} x^* + \bar{\mu} y^*$;
2′. $x^{**} = x$;
3′.$(xy)^* = y^* x^*$;
4′. $\|x^* x\| = \|x^*\|.\|x\|$;
5′. $\|x^*\| = \|x\|$;
6′. $x^* x + e$ possesses a two-sided inverse element for all $x \in R$.
(Gelfand & Naimark 1943: 4)

In modern terminology, a *normed ring* is now known as a Banach algebra.[4] Conditions 1′–3′ state that the operation * is an involution on R, and therefore that R is, in modern terminology, a Banach *-algebra. The contemporary definition of a C^*-algebra, by contrast, is as a Banach *-algebra such that $\|x^* x\| = \|x\|^2$ and therefore differs from the original definition in replacing conditions 4′–6′ with the single criterion that $\|x^* x\| = \|x\|^2$. So, where did the original Gelfand–Naimark axiomatic definition of a *-ring come from, and why does it differ from the contemporary characterization of a C^*-algebra?

Interestingly, in a footnote to their original definition, Gelfand and Naimark state that,

> The authors suppose the last two axioms to be corollaries of 1′–4′, but they have not succeeded in proof of this fact. We also note that the axioms 4′, 5′ may be replaced by the axiom: $\|x^* x\| = \|x\|^2$.
> (Gelfand & Naimark 1943: 4)

In effect, then, Gelfand and Naimark had suggested the contemporary axiomatic definition of a C^*-algebra, with some extra clauses which they suspected were redundant. Over the next 17 years, the cumulative work of several mathematicians proved Gelfand and Naimark to be correct about the redundant clauses.

But why all this hard work, if axioms are to be viewed as contextual definitions? Why not simply replace conditions 4′–6′ from the start with the single condition $\|x^* x\| = \|x\|^2$, rather than waiting for 17 years and a lot of mathematical hard work to make this replacement? After all, so long as the resulting axiom system was consistent (which it would have to be, if the axioms Gelfand and Naimark in fact plumped for were), the axioms would have succeeded in characterizing some mathematical concept, and starting with these axioms, it would be possible then to go on to consider what would have to be true of any system of objects satisfying these axioms.

The reason Gelfand and Naimark were not free to do this is clear from the central theorem of their paper, which proves that every normed *-ring is isomorphic to a subring of the set B(H) of bounded linear operators on a Hilbert space H. The axioms for a *-ring are chosen as a solution to a problem that arose in the preaxiomatic setting: how to characterize a class of mathematical objects that have

[4] That is, a Banach space which is also an algebra with norm satisfying $\|xy\| \leq \|x\|.\|y\|$.

already been encountered (and indeed which had been discovered to be important for the mathematical machinery of quantum mechanics).[5] The Gelfand–Naimark axiomatization was the best solution they could come up with to that problem, even though they suspected that some conditions were redundant.

Where does this discussion leave the "algebraic" view of axioms? One might think that the fact that Gelfand and Naimark were trying, with their axioms, to characterize a previously encountered system of mathematical objects, suggests an assertory rather than algebraic approach to these axioms: the axioms are evaluated by their success in asserting truths about the subrings of B(H). But the algebraic approach can be preserved in two respects. Firstly, as with the case of quaternions, we can see the preaxiomatic setting as containing enough of a preformal characterization of the concepts to be axiomatized for the axioms in question to be presented as a solution to a problem set by this preformal characterization – the axioms are neither to be viewed as asserting truths about independently existing objects nor as simply defining a new concept from out of nowhere, but rather as pinning down an appropriate definition of a concept that was already preaxiomatically grasped. But secondly, as in the case of Euclidean geometry, we can see the axioms taking on a life of their own once they are specified. Although C^*-algebras were originally characterized with the collection of all norm-closed selfadjoint algebras of bounded operators on complex Hilbert spaces in mind, much contemporary work on C^*-algebras takes place within the axiomatic setting without reference to this specific "concrete" interpretation. Just as, as a mathematical theory, Euclidean geometry can be investigated in an axiomatic setting entirely divorced from its original physical interpretation, similarly, C^*-algebras can be investigated in their own right in an axiomatic setting entirely divorced from the particular mathematical structures they were originally introduced to characterize. Having found the axioms for C^*-algebras, they can now be viewed as defining their subject matter, so that questions about C^*-algebras become questions to be answered within the new axiomatic setting.

As we have noted, a broadly "algebraic" approach to mathematical proof from axioms holds potential as a way out of Benacerraf's difficulties with the traditional "assertory" view of mathematics. Theorems, once proved, need not be viewed as asserting truths about specific objects, but rather, when we prove a theorem we establish what would have to be true of any objects satisfying the concepts fixed on in our axioms. However, at first glance, the "algebraic" approach has more difficulty than does the traditional "assertory" view in accounting for preaxiomatic theorizing. According to the "assertory" view, axioms express our intuitive grasp of

[5]In fact, on seeing the Gelfand-Naimark axiomatization of C^*-algebras, Irving E. Segal was able to argue convincingly that C^*-algebras provided the perfect basis for quantum field theory. Indeed, according to Segal, C^*-algebras "rendered quantum mechanics no less intuitive than classical mechanics once one became familiar with them." (Segal 1994: 58), while along the same lines, Edward G. Effros tells us that, "Physicists have viewed quantization as a mysterious process that ultimately cannot be explained. Feynman remarked that "it is safe to say that no one understands quantum mechanics". Owing in large part to the Gelfand-Naimark theorem, this is certainly not the case in mathematics." (Effros 1994: 100).

previously understood mathematical concepts, and so it makes perfect sense on this account that a great deal of meaningful mathematical theorizing could happen prior to axiomatization. On the other hand, if axioms fix our mathematical concepts, this would seem to leave little room for theorizing prior to the point of axiomatization. I have argued that the spirit of the algebraic view can be developed to deal with preaxiomatic theorizing in two ways. First, we can allow for cases of genuinely "assertory" preaxiomatic theorizing that concerns meaningful *empirical* concepts (which can then be set adrift from their empirical meaning once an axiomatization has been fixed upon). And second, we can allow for preformal characterizations of mathematical concepts that fall short of axiomatizations. Indeed, given a preformal characterization of a mathematical concept, an axiomatization may on this view be understood as a "solution" to the problem set by the preaxiomatic characterization, as the closest formalization in the ball-park of the concept at which our preaxiomatic characterization was aiming. Once axioms have been given, mathematical theorizing can proceed in the axiomatic setting cut loose from the preaxiomatic characterization – so that questions about the mathematical concepts so-characterized become questions about what does and does not follow from our axioms, as is central to the algebraic view of mathematical practice. Despite apparent difficulties with understanding preaxiomatic reasoning, then, adopting an algebraic approach to mathematical proof and theorizing still holds some promise as an account of the nature of mathematics that avoids some traditional problems.

References

Benacerraf, P. (1965). What numbers could not be. *Philosophical Review, 74*: 47–73.

Benacerraf, P. (1973). Mathematical truth. *Journal of Philosophy, 70*: 661–80.

Doran, R. S. (1994). *C*-Algebras: 1943–1993; A fifty year celebration (1993: San Antonio, Texas).* Vol. 167 of *Contemporary Mathematics*. Providence, RI: American Mathematical Society.

Effros, E. G. (1994). Some quantizations and reflections inspired by the Gelfand-Naimark theorem. In Doran (1994): 99–114.

Field, H. (1980). *Science without numbers: a defence of nominalism*. Princeton, NJ: Princeton University Press.

Field, H. (1989). *Realism, mathematics, and modality*. Oxford: Blackwell.

Gabriel, G., Hermes, H., Kambertel, F., Thiel, C., & Veraat, A. (eds.) (1980). *Gottlob Frege: philosophical and mathematical correspondence*. Chicago: University of Chicago Press. Abridged from the German edition by Brian McGuinness.

Gelfand, I. M., & Naimark, M. A. (1943). On the imbedding of normed rings into the ring of operators in Hilbert space. *Mat. Sbornik*, *12*: 197–213. Reprinted in Doran (1994): 3–20.

Hellman, G. (1989). *Mathematics without numbers: towards a modal-structural interpretation*. Oxford: Clarendon Press.

Hellman, G. (2003). Does category theory provide a framework for mathematical structuralism? *Philosophia Mathematica, 11*(2): 129–157.

Leng, M. (forthcoming). *Mathematics and reality*. Oxford: Oxford University Press.

Tait, P. G. (1866). Sir William Rowan Hamilton. *North British Review, 14*: 35–54.

Chapter 5
Completions, Constructions, and Corollaries

Thomas Mormann

5.1 Introduction

In his paper *A Renaissance of Empiricism in the Recent Philosophy of Mathematics?* (Lakatos 1978), Lakatos painted the history of Western epistemology with a broad brush:

> Classical epistemology has for two thousand years modeled its ideal of a theory [...] on the conception of Euclidean geometry. The ideal theory is a deductive system with an indubitable truth-injection at the top (a finite conjunction of axioms) – so that truth, flowing down from the top through the safe truth-preserving channels of valid inferences, inundates the whole system. (Lakatos 1978: 28)

The Euclidean perspective, as Lakatos defined it, has not much to say about proofs beyond the well-known characterization that they are deductively valid arguments that necessarily lead from true premises to true conclusions. In the case of Euclidean geometry, this means that the axioms of Euclidean geometry logically imply the theorems of Euclidean geometry. Today we take this assertion as a triviality. Philosophically, it might be less trivial than one thinks at first view. According to the founding father of modern epistemology – Kant – the just-mentioned "triviality" is no triviality but a blatant falsehood. More precisely, Kant proposed the thesis that the axioms of Euclidean geometry do *NOT* logically imply the theorems of Euclidean geometry. This sounds a bit surprising, to say the least. But Kant insisted that proof needs something more than just pure logic: namely, pure intuition.

If this is true, then Kant does not belong to the tradition of Euclidean epistemology as Lakatos defined it. Hence the question, "Whom else we can pick out as a good example of Lakatos's 'Euclidean tradition'?" A good choice would be Bertrand Russell, who vigorously argued for the Anti-Kantian thesis:

T. Mormann (✉)
Department of Logic and Philosophy of Science, University of the Basque Country UPV/EHU, Donostia-San Sebastian, Spain
e-mail: ylxmomot@sf.ehu.es

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives,* DOI 10.1007/978-1-4419-0576-5_5,

The axioms of Euclidean geometry do logically imply the theorems of Euclidean geometry. More generally, proofs in mathematics must not contain any nonlogical ingredients. (Russell 1903, § 5)

Let's call this Russell's thesis. The first time Russell presented it was in *The Principles of Mathematics* (Russell 1903). *The Principles* are heavily influenced by the logical and mathematical achievements of Peano, Cantor, and Frege, but Russell may be credited as the first professional philosopher who argued for this logicist thesis. If one accepts Russell's thesis, the philosophy of mathematics and the philosophy of the empirical sciences become neatly separated: On the side of the empirical sciences, one has a variety of procedures to obtain scientific knowledge, ranging from deductive and inductive arguments to experiments of various kinds. On the other hand, mathematics has only one method of producing knowledge: proving theorems through using arguments of deductive logic. Not everybody subscribed to this neat "apartheid" between philosophy of mathematics and philosophy of empirical science. Among the dissenters, one may mention (1) Peirce's Semiotic Pragmatism, (2) Cassirer's Critical Idealism, and (3) Lakatos's Quasi-empiricism.

I'll say nothing about Lakatos but will concentrate on Cassirer, with some occasional glances at Peirce. I do not aim at elucidating the relation between Peirce's and Cassirer's philosophies in general; rather, I'd like to concentrate on one pertinent issue, namely the role in both of intuition and symbolic constructions for mathematical knowledge. Both accounts may be characterized as attempts to do justice to Kant's philosophy of mathematics and at the same time to overcome the limitations of the traditional Kantian account of pure intuition in the realm of mathematical proofs. Both meant to withstand Russell's radical logicist stance, according to which anything like intuition is completely obsolete for modern mathematical and scientific knowledge. In particular, his emphasis on the role of idealization[1] in mathematics *and* the sciences may be interpreted as an attempt to revive something like Kant's pure intuition, or so I want to argue. The outline of my paper is as follows:

1. The Role of Intuition in Mathematics according to Kant
2. Russell's Logicist Expulsion of Intuition
3. Cassirer's Critical Idealism
4. Idealizations, Constructions and Corollaries
5. Concluding Remarks

[1]"Idealization" points to the more general topic of the "symbolic" character of scientific and mathematical knowledge, a huge issue that involves epistemology, philosophy of science, and other disciplines. It cannot be adequately treated in a short paper like this; for further information, the reader may consult the following: Ferrari and Stamatescu (2002), Ihmig (1996, 1997) Rudolph and Stamatescu (1997), Ryckman (1991).

5.2 The Role of Intuition in Mathematics According to Kant

First we have to deal with Kant's claim that the axioms of Euclidean geometry do not logically imply the theorems of Euclidean geometry. Indeed, Kant contended that the theorem that the sum of the angles of a triangle is two right angles (180°) is *not* implied the Euclidean axioms. First I'll give the textual evidence, then explain why Kant made such a claim and why it is correct – even from our more modern point of view.

Kant's "antilogical" thesis is expressed most clearly in the "Discipline of Pure Reason in Its Dogmatic Employment" in the *Critique of Pure Reason*, where Kant contrasted philosophical with mathematical reasoning:

Philosophy confines itself to general concepts; mathematics can achieve nothing by concepts alone but hastens at once to intuition, in which it considers the concept in concreto, although still not empirically, but only in an intuition which it presents a priori, that is, which it has constructed, and in which whatever follows from the general conditions of the construction must hold, in general for the object of the concept thus constructed.

Suppose a philosopher be given the concept of a triangle and he be left to find out, in his own way, what relation the sum of its angle bears to a right angle. He has nothing but the concept of a figure enclosed by three straight lines, along with the concept of just as many angles. However long he meditates on these concepts, he will never produce anything new. He can analyze and clarify the concept of a straight line or of an angle or of the number three, but he can never arrive at any properties not already contained in these concepts. Now let the geometer take up this question. He at once begins by constructing a triangle. Since he knows that the sum of all the adjacent angles which can be constructed from a single point on a straight line, he prolongs one side of the triangle and obtains two adjacent angles which together equal two right angles. He then divides the external angle by drawing a line parallel to the opposite side of the triangle, and observes that he has thus obtained an external adjacent angle which is equal to an internal angle and so on. In this fashion, through a chain of inferences guided throughout by intuition, he arrives at a solution of the problem that is simultaneously fully evident and general. (Kant 1797/2006: B743–745)

According to Kant, the only kind of logic available for the philosopher to analyze concepts was traditional syllogistic logic. As Peirce and Russell already noted, syllogistic logic is not very helpful for proving theorems of geometry and other mathematical theories. Thus, Kant was quite right in claiming that the axioms of Euclidean geometry do not logically imply the theorems of Euclidean geometry. If we rely on syllogistic logic, we need help from a nonlogical source to carry out geometrical proofs. For Kant, this source was provided by pure intuition.

The experts on the Kantian philosophy of mathematics have formed no consensus about what exactly "Kantian Pure Intuition" means (cf. Friedman 1992). Here, I am not interested in parsing Kantian philology. Rather, I'd like to take Kant as a starting point.

The important thing about "pure intuition" in a broad Kantian sense is that it casts mathematical proofs as ideal spatio-temporal scenarios, in which certain constructions are carried out according to certain rules constituting the ideal domain in which this mathematical activity takes place. Something like this can already be found in the *Critique of Pure Reason*:

> I cannot represent to myself a line, however small, without drawing it in thought, that is gradually generating all its parts from a point. Only in this way can the intuition be obtained […] Geometry together with its axioms, is based upon this successive synthesis of the productive imagination in the generation of figures. (Kant 1787/2006: B 203–204)

This Kantian drawing of straight lines does not take place in real space-time; rather, it refers to an ideal space-time – more precisely, an idealized Newtonian space-time. The constructions guided by pure intuition take place in this idealized space-time, where *ideal* points, *ideal* trajectories, *ideal* straight lines and so on exist, and where an *ideal* subject is able to draw perfect geometrical figures. This ideal space is defined by Newtonian mechanics and thus, in some sense, geometry presupposes Newtonian mechanics. In other words, a "mixing" of physical and mathematical ideas was essential to the unity of Kant's philosophy of mathematics. As we shall see similar features may be discerned in Cassirer's and Peirce's accounts.

Summarizing, then, I propose to consider "pure intuition" as a faculty involved in checking proofs step by step to see that each rule has been correctly applied – in short, the intuition involved in "operating a calculus" (cf. Hintikka 1980). Kantian pure intuitions should be interpreted as having a strong operational or constructive component. Such a constructive version may help preserve a role for something like intuition even for modern mathematics.

5.3 Russell's Logicist Expulsion of Intuition from Mathematics

For mathematicians, everything changed at the end of the nineteenth century, when modern relational logic arrived on the stage. For Russell, a paragon of an anti-Kantian philosopher of mathematics, the date of this change can be determined quite precisely. In a letter from 1910 to his friend Jourdain he wrote:

> Until I got hold of Peano, it had never struck me that Symbolic Logic would be of any use for the Principles of Mathematics, because I knew the Boolean stuff and found it useless. Peano's EPSILON, together with the discovery that relations could be fitted into this system, led me to adopt symbolic logic. (Cited in Proops 2006: 276)

"The Boolean stuff" Russell mentions was Boole's *An Investigation of the Laws of Thought on Which are Founded the Mathematical Theories of Logic and Probabilities* (1854). We may identify this "stuff" with standard syllogistic logic, which Russell rightly considered as rather useless for mathematics. At least, he was convinced that it would not do the job of deducing mathematical theorems from mathematical axioms. Thus, before he became acquainted with Peano's logic in 1900, Russell agreed with Kant that "logic" is not of much use for mathematical proofs. However, the work of Peano, Cantor, and Frege had made available a much more powerful logic that could do everything that in less fortunate times belonged in the ken of pure intuition. Russell's argument for expelling Kantian intuition from mathematics was simply that pure intuition was no longer needed:

All mathematics, we may say – and in proof of our assertion we have the actual development of the subject – is deducible from the primitive propositions of formal logic: these being admitted, no further assumptions are required.

Russell's *The Principles of Mathematics* (1903) may be considered as the source for a purely logicist conception of mathematical proofs. From Russell onwards, the mainstream philosophy of science conceptualized mathematical proofs as purely logical derivations. Of course, intuition might continue to play a restricted role, insofar as it might be considered as essential in determining which axioms are true. But intuition was expelled even from this last resort, when axioms lost their status of indubitable truths and became mere conventions or implicit definitions. Thereby, the logicist philosophy of mathematics established a neat boundary between the realm of mathematics on the one hand and the realm of empirical science on the other hand – because obviously, deductive logic was not the only method to produce knowledge in the empirical sciences.

Even though we may consider Kant and Russell antagonists with respect to the role of intuition in mathematics, in another sense they belonged to the same ilk. Both argued for a fixed and stable framework for doing mathematics: According to Kant, mathematics was based on some fixed pure intuition; Russell based it on some kind of equally fixed relational logic. Actually, matters never stabilized in the neat way Russell had hoped, since the new relational logic never achieved the fixed and unique character that Russell expected.

5.4 Cassirer's Critical Idealism

In contrast to Kant's stable intuition and Russell's stable logic, Cassirer's philosophy saw all science as an unending conceptual process of which the content and structure were not determined by armchair philosophy once and for all but unfolded in an unending process of scientific conceptualizations. Already in *Kant und die moderne Mathematik* (1907) Cassirer sketched out an attempt to overcome the logicist separation between mathematics and the sciences; he called it "critical idealism." He elaborated this Neo-Kantian approach in *Substance and Function* (1910) and later in the third volume of his opus magnum *The Philosophy of Symbolic Forms* (1929). The fundamental concept of Cassirer's unified philosophy of mathematics and science was the notion of *idealization*, or, more precisely, of *idealizing completion*. According to him, idealization plays a crucial role both in the formation of the concepts of empirical science and in the formation of mathematical concepts; idealizing completion was the common source of both mathematical and scientific concept formation.

Thus, Cassirer occupied a rather peculiar position among the attempts to philosophically understand modern mathematics and its place among the other sciences: On the one hand, he vigorously supported the then-new relational logic inaugurated by Frege, Peano, Russell, and others. In *Kant und die moderne Mathematik* he enthusiastically welcomed Russell's *The Principles of Mathematics* as an important achievement for the philosophical understanding of modern mathematics. On the Other hand, he thought that Russell and others had not fully grasped the philosophical consequences of the new logic and its rejection of intuition. For reasons of space I present only a brief and condensed description of Cassirer's main philosophical theses (for a fuller account see Mormann 2008).

According to Cassirer, the philosophy of science is to be conceived as the theory of the formation of scientific concepts. These concepts do not yield pictures of reality; rather, they provide guidelines for the conceptualization of the world. For example, the fundamental concepts of theoretical physics are blueprints for possible experiences. In the endeavor to conceptualize the world, the factual and theoretical components of scientific knowledge cannot be neatly separated. A scientific theory inextricably interweaves "real" and "nonreal" components. Not a single concept but a whole system of concepts confronts reality. The unity of a concept is not to be found in a fixed group of properties, but in a rule which lawfully represents the diversity as a sequence of elements. The meaning of a concept depends on the system of concepts in which it occurs, which is no a single fixed system but rather a continuous series of systems unfolding in the course of history. Scientific knowledge is a "fact in becoming" ("Werdefaktum"). Our experience is always conceptually structured; there is no nonconceptually structured "given." Rather, the "given" is an artifact of bad metaphysics. Scientific knowledge does not cognize objects as ready-made entities. Rather, it is organized objectually: it objectifies cases of invariant relations in the continuous stream of experience. Thus, the concepts of mathematics and the concepts of the empirical sciences are of the same kind.

I'd like to concentrate on the last claim. As a start, it may be expedient to dwell upon it in more detail, quoting more fully from *Kant und die moderne Mathematik*:

> What "critical idealism" seeks and what it must demand is a *logic of objective knowledge* (gegenständliche Erkenntnis). Only when we have understood that the *same foundational syntheses* (Grundsynthesen) on which logic and mathematics rest also govern the scientific construction of experiential knowledge, that they first make it possible for us to speak of a strict, lawful ordering among appearances and therewith of their objective meaning: only then the true justification of the principles is attained. (Cassirer 1907: 44).

I'll refer to this thesis as the "sameness thesis." It lies at the heart of Cassirer's critical idealist philosophy of science (cf. Mormann 2008). If one subscribes to the sameness thesis, the logicist separation between mathematics and science is not acceptable. According to critical idealism, the philosophy of science should concentrate *neither* on mathematics, as an ideal science, *nor* on empirical science:

> If one is allowed to express the relation between philosophy and science in a blunt and paradoxical way, one may say: The eye of philosophy must be directed neither on mathematics nor on physics; it is to be directed solely on the connection of the two realms. (Cassirer 1907: 48)

More precisely, Cassirer contended that a philosophy of science had to look for the common root from which both physics *and* mathematics sprang: namely, the method of introducing ideal elements – which established the idealizing character of any scientific knowledge. In contrast to Russell, Cassirer did not attempt to neatly separate mathematics and the empirical sciences.

Today, when dealing with idealization in science, one implicitly assumes that idealization only concerns the empirical sciences. For instance, when discussing epistemological and ontological problems of idealization, one deals with ideal gasses, frictionless planes, ideal point masses and so on. One rarely takes into account idealization *within* mathematics, which is thought to be already on the ideal side, so to speak. Thus, we assume that idealization concerns solely the empirical realm. According to Cassirer such a theory of idealization starts too late: Since idealization has a role in both, a comprehensive theory of idealization must take into account both mathematics *and* the empirical sciences.

Moreover, Cassirer insisted that one should not tackle this problem armed with "philosophical" presuppositions of the correct methods of idealization. The methods of idealization should be studied empirically, so to speak; no philosophical intuition will give us the key, which has to be discovered by studying the history of science. Hence, the philosophy of science has to pay attention to the ongoing evolution of science; it has to investigate and explicate the formation of scientific concepts in the real history of science.

In a nutshell, then, the sameness thesis contends that the "common foundational syntheses," on which both mathematical knowledge *and* physical knowledge are based, are idealizing completions carried out by the introduction of "ideal elements." For Cassirer, idealization is a common mark of all sciences *qua* sciences.

The primary role of idealization in mathematics is to underwrite the constructive procedures used in mathematical argumentation, particularly in mathematical proofs. Idealizations aim to single out appropriate domains for doing mathematics, in that they warrant that certain symbolic constructions and procedures can be carried out smoothly. In the elementary case of geometry this means, for instance, that certain points exist – more generally, that certain constructions are feasible. Less elementary, and very generally, the axiom of choice may be interpreted as an often indispensable idealizing assumption that guarantees the construction of choice functions; that is, the possibility of picking out exactly one element of each set in a given set of nonempty sets.

Idealizing completions intend to provide conceptual domains that offer comfortable and promising realms for a variety of symbolic constructions, transactions, and calculations. For instance, in an obvious sense, the domain of natural numbers is less suited to carrying out less than elementary calculations than, say, the domain of real or complex numbers. The ideal character of a domain is to be assessed not by passively staring at its perfect, pure character but rather by exploring the variety of possible symbolic actions for which it offers an expedient frame. Or, to put it the other way round, a domain lacks ideal or conceptual completeness if we meet too many obstacles, exceptions, contradictions and ad hoc assumptions in the course of our conceptual activities within it. The completeness of a conceptual domain is particularly observable in the case of geometry, as manifested in the variety of geometrical constructions we can carry out that ensure us of the existence of certain points, lines, and other geometrical entities. For Kant, the warrant of the ideal completeness of the realm of geometry was pure intuition, which ensured us that the ideal points, lines, and planes of geometry possessed the properties that rendered possible certain constructions. For Cassirer, idealization became a multifaceted, pluralist endeavor that evolved in the ongoing process of science in which the unity of pure thought was constituted. In both cases the ideal character of geometry showed itself in the richness of possible symbolic actions and transactions.

5.5 Idealizations, Constructions, and Corollaries

Cassirer's paradigmatic example of an idealizing completion in mathematics was the construction of Dedekind cuts. To understand its guiding function for the general theory of idealization, I briefly discuss an elementary geometrical problem that shows how useful Dedekind completeness is in geometrical construction. Moreover, this example clearly exhibits the resemblances between Kant's pure intuition, Cassirer's idealization and Peirce's diagrammatic thinking for mathematics and the empirical sciences.

Consider the problem of constructing in the Euclidean plane E an equilateral triangle with a given side AB of length 1. A "naïve" construction proceeds as follows: Consider the circle C_A around A with radius of length 1 and the circle C_B around B with radius 1. Then the intersection of the two circles yields the third vertex X of the equilateral triangle ABX we were looking for. From a logicist point of view, this "intuitive construction" is flawed. Assuming Euclid's original axioms, the logicist will object that we do not know that the two circles C_A and C_B actually intersect. They may somehow avoid having a common point X, since one circle may slip through the other. This is more than a remote possibility. Indeed there are unintended models of Euclidean geometry showing that this indeed might happen. Consider the rational plane $\mathbf{Q}^2$ of ordered pairs of rational numbers $(p, q) \in \mathbf{Q}$. The rational plane satisfies all geometrical axioms Euclid required, but for it the intersection point X does not exist. Assume A to have the coordinates (0, 0) and B the coordinates (0, 1). Then X has the coordinates $(1, \sqrt{3})$. But $\sqrt{3}$ is irrational and therefore $(1, \sqrt{3})$ does not belong to the rational plane $\mathbf{Q}^2$.

In order to ensure the existence of the intersection point X, one has to rely on a new axiom that does not appear in Euclid's *Elements* – namely, Hilbert's axiom of continuity, which is essentially equivalent to Dedekind's axiom ensuring the existence of sufficiently many Dedekind cuts. In sum, the construction of the equilateral triangle can be carried out successfully only if we are operating in a *completed* plane, which ensures that our constructions yield what we expect from them. In other words, the completion of the plane is a necessary presupposition to enable "naïve" constructions such as that of the vertex X above.

Completions of this kind are not restricted to elementary geometry. Cassirer convincingly argued that idealizing completions are typical for all areas of mathematics (for some modern examples, see Mormann 2008). For Kant, some kind of ideal Newtonian space-time determined the variety of these constructions. In contrast, for the Neo-Kantian Cassirer these conceptual frameworks no longer depend on some fixed ahistorical "pure intuitions," but emerge in the evolution of scientific knowledge itself; thus Cassirer's philosophy of science has a sort of Hegelian flavor (cf. Mormann 2008).

Designing conceptual frameworks or settings for doing mathematics is, however, certainly not the entire story of the evolution of mathematics. The important part is putting these frameworks to work by formulating interesting problems and proving important theorems in them. Cassirer did not say much about these more concrete aspects of the idealizational practice of mathematics. Here Peirce's philosophy of mathematics comes to the rescue, in particular the insight that Peirce self-confidently characterized as his "first real discovery":

> My first real discovery about mathematical procedure was that there are two kinds of necessary reasoning, which I call the Corollarial and the Theorematic, because the corollaries affixed to the propositions of Euclid are usually arguments of one kind, which the more important theorems are of the other. The peculiarity of theorematic reasoning is that it considers something not implied at all in the conceptions so far gained, which neither the definition of the object of research nor anything yet known about could of themselves suggest, although they give room for it. Euclid for example, will add lines to his diagram which are not at all required or suggested by any previous proposition, and the conclusion that he reaches by this means says nothing about. I know that no considerable advance can be made in thought of any kind without theorematic reasoning. (Peirce 1976, vol 4: 49)

For reasons of space I can give only some brief hints why Peirce's distinction between theorematic and corollarial reasoning can be used to maintain for diagrammatic or symbolic reasoning an indispensable role in mathematics that can withstand the logicist criticism Russell put forward more than a century ago (for a detailed interpretation of Peirce's distinction see Hintikka 1980). First, according to Peirce, theorematic reasoning, which in geometry may be characterized through the introduction of new points, lines, and other geometrical objects not present in the original formulation of a problem, is not restricted to geometry. Rather, theorematic reasoning pervades all of mathematics. As Hintikka points out, what makes a deduction theorematic is not that it is based on some figures with some more or less well-defined properties but that we must take into account other objects than those needed to state the premise of the argument (cf. Hintikka 1980: 306). The new objects do not have to be visualized, but they do have to be mentioned and used in the argument. In contrast, an argument is corollarial, in Peirce's sense, if it is only necessary to imagine any case in which the premises are true in order to perceive immediately that the conclusion holds in that case (cf. Peirce 1976, vol. 4: 38). It seems appropriate, then, to contend that corollarial reasoning is based on what Russell called "the Boolean stuff"; that is, elementary propositional logic and syllogistic logic. Theorematic deduction, on the other hand, is deduction in which it is necessary to carry out some sort of imaginary experiment in order to bring about some useful effects that may allow drawing further corollarial deductions that finally lead to the desired conclusion (ibid.).

Conceived in this logical way (as Peirce and Hintikka do), the distinction between theorematic and corollarial argumentation does not fall prey to Russell's logicist criticism. Russell argued that there has been no role for intuitions and figures in serious mathematical arguments since the advent of modern relational logic, because valid geometrical reasoning could now be completely formalized. According to him, figures were thought of as indispensable simply because of the incompleteness of earlier axiomatizations. This incompleteness made it necessary for mathematicians to go beyond their own explicit assumptions and to appeal to some sort of Kantian "pure intuition." Peirce, as one of the founding fathers of modern relational logic, would be happy to subscribe to Russell's "complete formalization thesis." Nevertheless, he would insist on the necessity of distinguishing between different logical levels – to wit, corollarial and theorematic arguments. This distinction does not disappear even when geometrical arguments are "formalized." Moreover, as Hintikka has pointed out, if theorematic inference is characterized by the introduction of auxiliary individuals into the argument, one can consider the theorematic character of arguments as a gradual matter (cf. Hintikka 1980: 310).

In other words, one should not consider logic as a monolithic tool but allow for different degrees of complexity, in contrast to Russell's sweeping logicism that lumped all logic together. Following the insights of Peirce and Cassirer, we obtain three different levels of "logical" reasoning in mathematics (and the sciences) ordered by degree of complexity:

(1). Corollarial Reasoning
(2). Theorematic Reasoning
(3). Completional Reasoning

All three levels are involved in mathematical reasoning. The most elementary level is corollarial reasoning, in Peirce's sense, characterized logically by the employment of elementary propositional and syllogistic logics. On the second level, one finds the realm of theorematic reasoning, which has often been characterized as the realm of some kind of "Kantian intuition." It is important, however, to conceive this kind of intuition not as a capacity of perceiving some kind of platonic reality but as the ability to carry out diverse symbolic or ideal constructions. Logically, these constructions can be described as the introduction of new individuals and relations, leading to an increased level of quantificational complexity. Finally, on the highest level, one finds what may be called the completional or idealizing reasoning directed to the design of appropriate "settings" or frameworks in which successful diagrammatic or symbolic constructions, in Peirce's sense, can be carried out. In other words, the axiom systems are proposals or blueprints of how to produce useful constructions.

Idealizing completions offer the framework for theorematic constructions, in Peirce's sense. Frameworks are proposals whose "correctness" has to be assessed pragmatically. Hence, Cassirer may be considered as subscribing to a "theoretical pragmatism" according to which:

> … The truth of concepts rests on the capacity [to lead] to new and fruitful consequences. Its real justification is the effect, which it produces in the tendency toward progressive unification. Each hypothesis of knowledge has its justification merely with reference to this fundamental task (Cassirer 1910: 318ff.)

Cassirer's theoretical pragmatism fits well with the implicit pragmatism upheld by working mathematicians, who prefer settings in which theorems "one likes to be true" are actually true (see Mormann 2008). Similarly, just as it has accused theorematic reasoning of being based on vague intuitions of psychological interest only, a narrow logicist philosophy of mathematics often relegated the choice of "appropriate settings" to the realm of subjective whims and matters of taste. The evolution of twentieth century mathematics has shown that this assessment is hardly tenable. Constructing idealizing completions has become a routine activity, and there is now an explicit theory that deals with these problems: Category theory offers a general framework in which mathematicians can discuss problems of appropriate settings in a manner that goes beyond subjectivist presentations and preferences. In category theory, problems of idealization, completion and the development of mathematical concepts become explicit topics on the agenda of mathematics. These questions are no longer restricted to informal philosophical considerations but have obtained the status of well-defined mathematical problems.

5.6 Concluding Remarks

One of Cassirer's most fruitful philosophical insights in the philosophy of mathematics was that idealizing completions such as Dedekind's were more than just mathematically interesting technical achievements. Rather, these constructions belonged to the conceptual core of modern mathematics, being prototypes for the idealizational constructions essential for twentieth century mathematics *and* for idealizational constructions in the empirical sciences too.

Evidence for this sweeping claim comes not from a priori considerations but from the empirical observation that idealizations and completions have become routine parts of the mathematician's daily work (cf. Mormann 2008). How these completed, idealized frameworks organize the practice of mathematics may be studied by relying on the conceptual apparatus centering around the distinction between theorematic and corollarial reasoning introduced by Peirce, Hintikka, and others.

In sum, the role of idealization may be taken into account as contributing to a more realist philosophy of mathematics. This philosophical approach takes real mathematics seriously, in contrast to the traditional approaches that too closely stick to over-simplified logical models of mathematics. Cassirer took one step on this new road by emphasizing the role of idealizing completions. Peirce took another one by pointing out the importance of diagrammatic constructions. Not that the thoughts of these authors are fully in agreement with Kant's original idealist *Ansatz*. Rather, Kant, Peirce, and Cassirer all still have useful ideas to offer in the philosophical task of explicating the roles of idealization and conceptual constructions in the formation of mathematical concepts. This endeavor falls in line with the general Neo-Kantian attitude that philosophy has the task not of providing secure and unshakable foundations for mathematics, science or any other symbolic endeavor but rather of understanding how they work and elucidating their ongoing evolution.

References

Cassirer, E. (1907). Kant und die moderne Mathematik. *Kant-Studien, 12*: 1–49.

Cassirer, E. (1910/1953). *Substance and function, Einstein's theory of relativity*. New York: Dover.

Cassirer, E. (1923–1929/1955–1957). *The philosophy of symbolic forms*, Vols. I–III. New Haven: Yale University Press.

Ferrari, M., & Stamatescu, I. O. (eds.) (2002). *Symbol and physical knowledge. On the conceptual structure of physics*. Berlin, Heidelberg, New York: Springer.

Friedman, M. (1992). *Kant and the exact sciences*. Cambridge/Massachusetts: Harvard University Press.

Hintikka, J. (1980). C.S. Peirce's "First Real Discovery", and its contemporary relevance. *The Monist, 63*: 304–315.

Ihmig, K.-N. (1996). Cassirers Rezeption des Erlanger Programms von Felix Klein. In M. Plümacher & V. Schürmann (Hrg.): pp. 141–163.

Ihmig, K.-N. (1997). *Cassirers Invariantentheorie der Erfahrung und seine Rezeption des 'Erlanger Programms'*. Cassirer Forschungen Band 2. Hamburg: Meiner Verlag.

Kant, I. (1787/2006). *Critique of pure reason*. New York: Dover.

Lakatos, I. (1978). Mathematics, science and epistemology. In J. Worrall & G. Curry (eds.). *Philosophical papers*, Vol. 2. Cambridge: Cambridge University Press.

Levy, S. (1997). Peirce's theorematic/corollarial distinction and the interconnections between mathematics and logic. In N. Houser, D. D. Roberts & J. Van Evra (eds.). *Studies in the logic of Charles Sanders Peirce*. Indiana University Press: Bloomington and Indianapolis: 85–110.

Mormann, T. (2008). Idealization in Cassirer's philosophy of mathematics. *Philosophia Mathematica (III), 16*: 151–181.

Peirce, C. S. (1976). *The new elements of mathematics*. Edited by C. Eisele. Vols. 1–4. The Hague: Mouton.

Proops, I. (2006). Russell's reasons for logicism. *Journal of the History of Philosophy, 44*(2): 267–292.

Rudolph, E. & Stamatescu, I. O. (1997). *Von der Philosophie zur Wissenschaft, Cassirers Dialog mit der Naturwissenschaft*. Cassirer Forschungen Band 3. Hamburg: Meiner Verlag.

Russell, B. (1903). *The principles of mathematics*. London: Routledge.

Ryckman, T. A. (1991). *Conditio Sine Qua Non*? – *Zuordnung* in the Early Epistemologies of Cassirer and Schlick. *Synthese, 88*: 57–95.

Chapter 6
Authoritarian Versus Authoritative Teaching: Polya and Lakatos

Brendan Larvor

6.1 Criticism and the Autonomy of Science

How can a teacher be authoritative without being authoritarian? Throughout his adult life, Lakatos campaigned against authoritarian teaching on both scientific and political grounds, without always disentangling the two. In 1947, while he was active in the Communist Party's effort to bring Hungary's elite colleges of higher education under state control, he wrote "Eötvös Kollegium–Gyorffy Kollegium" in the journal *Valóság*. Eötvös College was an elite institution, both intellectually and socially. Gyorffy College was a leader in the people's college movement, and was thus more ideologically sound, but lacked the intellectual and material resources of Eötvös College. In *Valóság*, Lakatos argued that Eötvös College should emulate Gyorffy College and open its doors to working-class students, in order to produce the proletarian intelligentsia that the new Hungary required. In turn, the college would achieve greater intellectual heights, since its new working-class students would not suffer from bourgeois misconceptions.[1] Over the next 9 years, Lakatos came to understand that bourgeois elitism is not the only sort, nor perhaps even the most dangerous. At the Petőfi Circle pedagogy meeting in 1956, he delivered a speech, later published as "On Rearing Scholars".[2] In this speech, he denounced political interference in science, "…*the Party cannot guide science*. On the contrary: *it is science that must guide the Party*".[3] He insisted that education should foster

B. Larvor
School of Humanities, University of Hertfordshire, Hatfield, UK

[1] Long (1998) p. 269

[2] Motterlini (1999) pp. 375-382; translated by Ninon Leader. While he was preparing this speech, Lakatos organised a protest against a party official's doctoral thesis that was critical of the late professor of pedagogy, Sándor Karácsony (1891–1952). After midnight and much acrimonious discussion, the panel of examiners rejected the thesis. We should not suppose that this was a simple defence of scholarly independence from politics. As a piece of *Stalinist* party work, this thesis was, by September 1956, behind the political times. The occasion of its formal public defence presented an opportunity for Lakatos and others to rehearse the political revolution to come later that autumn.

[3] Motterlini (1999) p. 380. Bandy and Long (2000) report that Árpád Szabó joined Lakatos in opposing the doctoral thesis against Karácsony, and ended his speech with the words, "It is scholarship that should be guiding the Party, not the Party the scholars."

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_6,

originality and rigour. In the same breath, he called for logic to become compulsory in Hungarian schools, and for a restoration of the right to dissent.[4] These two demands come together because the right to dissent makes space for rational, logical criticism, but to use this space the critic must be logically skilful.

Lakatos' position depends on a distinction between authoritarian teaching and its alternative, which we might equally well call "scientific", "democratic" or "critical" teaching. Authoritarian teaching presents its doctrines as indisputable truths and resents criticism as an affront to its virtue. The alternative style of teaching presents its doctrines as conjectures supported by fallible arguments and open to criticism. Though Lakatos does not say so in this speech, this approach requires teachers to present theories, doctrines and orthodoxies with their supporting arguments.[5] Otherwise, the critical approach has nothing to work on and either atrophies or degenerates into a blanket scepticism.[6] Underlying Lakatos' argument is the thought (familiar to us from Mill and Popper) that every doctrine should be open to criticism because no doctrine can be proved beyond all doubt.[7] This was not so great an intellectual shift as one might suppose, because Hegelian Marxists hold that every theory may be dialectically overcome.[8] Since all theories are fallible, Lakatos claimed, teachers should encourage students to doubt the current orthodoxies. In his 1956 Petőfi Circle speech, Lakatos demanded that this approach to teaching should be taught:

---

[4] Motterlini (1999) p. 380.

[5] This was already part of his thought in 1947, when he published a review of Karoly Jeges' *I Learn Physics*. While he approved of Jeges' aim of introducing physics to non-specialists, Lakatos complained that Jeges introduced concepts "in a scholastic manner, without making them real in terms of experiments" or giving "the historical dialectics of theories" (quoted from Long p. 266). In a short piece published in 1963, he complains that, "…science and mathematics teaching is disfigured by the customary authoritarian presentation. Thus presented, knowledge appears in the form of infallible systems hinging on conceptual frameworks not subject to discussion. The problem-situational background is never stated…" (1978b p. 254).

[6] "Without refutations one cannot sustain suspicion" (Lakatos 1976 p. 49).

[7] "…no scientific theory, no theorem can conclude anything finally…" (Motterlini (1999) p. 379). Lakatos ended this speech thus, "At the last Party Congress in China, Teng Xiao Ping talked about guaranteeing the right to dissent and remarked that if, perchance, truth happened to be on the side of a minority, this right would facilitate the recognition of truth." (*Op. Cit.* p. 382).

[8] Except Marxism, of course. In his review of Jeges, Lakatos remarked, "It is incorrect to give the impression that physics is an eternal science" (quoted from Long p. 266). Marxist dialectical progressivism is distinct from liberal empirical fallibilism, but they both insist that today's orthodoxies may be rationally superseded tomorrow. Hence, opposition to Stalinism's fixed official truths provided a context in which Lakatos could move between them and eventually combine them.

Dialectics in Hegel and Marx is about progress through conflict, which in science means criticism. However, true communism is supposed to mark the resolution of all dialectical oppositions, therefore communist science has no need of criticism; nevertheless, it advances. This explains Lakatos' otherwise perplexing remark that "dialectic tries to account for change without using criticism: truths are 'in continual development' but always 'completely incontestable'" (1976 p. 55n). He has in mind here Soviet or Stalinist 'dialectic', rather than the Hegelian-Marxist dialectics that he elsewhere mentions with approval. György Litván called Lakatos a "natural-born Trotskyite," better fitted to Trotsky's 'permanent revolution' than to Stalinist stasis (Long p. 275). Long elaborates on the tension between Lakatos' Trotskyite tendency and his need for order and clarity.

> New, hitherto unfamiliar chapters ought to be included in pedagogical textbooks, such as "Methods for stimulating curiosity and developing it into interest," "How to teach people to think scientifically," "How to teach people the respect for facts" and – God forbid! – "How to teach people to doubt."[9]

Twelve years later, Lakatos found himself making a similar argument from a rather different perspective. As a professor at the LSE, faced with the demands of student radicals, he argued that students should have the right to criticise, and that "They should be encouraged – and even helped – to make the best of it".[10] However, the university should resist student demands for power over the curriculum, for the same reason that party or government interference in science should be resisted, namely that such interference is driven by extra-scientific, political commitments rather than by scientific or scholarly criticism. Lakatos offered no reason to suppose that curricular struggles between professors are any less party-political than the demands of student Maoists. Here as elsewhere, Lakatos' anti-elitism was less firm than his insistence on it.[11] He consistently argued that every body of scientific knowledge must be open to criticism – but only to *scientific* criticism. However, he never addressed the point that effective scientific criticism requires a level of expertise that is, in almost all cases, the preserve of an elite.[12]

6.2 Criticism in Mathematics Research and Mathematics Teaching

In the case of mathematics, Lakatos' insistence on the importance of criticism found its most developed expression in *Proofs and Refutations.* In the dialogue, criticism turns the naïve Descartes-Euler formula about polyhedral solids (Vertices – Edges + Faces = 2) into Poincaré's algebraic version of the theorem. Or at least, criticism motivates the progression. Criticism exposes limitations and inadequacies in each stage of the development. However, the counterexamples to successive versions of the theorem and most of the materials to repair these faults seem to come from nowhere.[13] The pupils arrive in the classroom already equipped with articulated philosophical and methodological opinions, some of which change under pressure.[14] They also bring a rich stock of heuristics and in one case (Epsilon) a proof due to Poincaré.

---

[9] Motterlini (1999) pp. 379-380.

[10] Lakatos (1978b) p. 249.

[11] See Lakatos (1978b) pp. 111-120, 226-227; (1976) p. 98n2; Larvor (1998) pp. 81-82.

[12] Though not always. See the case of organic farmer Mark Purdey's work on organo-phosphate insecticides and bovine spongiform encephalopathy.

[13] For the spontaneous appearance of counterexamples, see (1976) pp. 10, 11, 13, 15, 16, 19, 21 & 22.

[14] "What a dramatic series of volte-faces! Critical Alpha has turned into a dogmatist, dogmatist Delta into a refutationist, and now inductivist Beta into a deductivist!" (1976) p. 75.

Now, it is central to Lakatos' view that a counterexample does not merely show that a conjecture is false. A counterexample finds fault with some specific aspect of the conjecture. It may suggest a specific improvement to the conjecture, especially if it stimulates a fresh analysis of the conjecture's proof. If the counterexample to the conjecture is also a counterexample to a lemma of the proof (in Lakatos' jargon, a *global* and *local* counterexample[15]), then we might include the false lemma as a condition of the proof (lemma-incorporation[16]) or we might replace it with an (as yet) unfalsified lemma.[17] If the counterexample is *global* but *not local*, it demands a more searching proof-analysis to track down the false premise or the error of reasoning. Repeating these procedures may induce so many refinements to the terms of the conjecture that we can speak of "proof-generated concepts" and eventually of a "proof-generated theorem".[18] Thus, criticism need not merely show that a conjecture is false; it can play a role in shaping its replacement. Nevertheless, the counterexamples and much of the other mathematical material in *Proofs and Refutations* do not grow out of criticism, but rather seems to appear by magic – the participants just happen to have precisely what they need. Lakatos might reasonably reply that the resources that the participants in his dialogue bring with them are dialectical products of critical discussions beyond the scope of his story. He is not obliged to give a philosophical account of the entire content of modern mathematics. It is enough to give some examples of the way criticism advances mathematics. Nevertheless, the patterns of criticism in the *Proofs and Refutations* dialogue can do this only because the critics are well-informed mathematical and philosophical sophisticates.[19] Criticism may be the baking-powder that causes this cake to rise, but the pupils have to bring all the other ingredients with them without the teacher telling them to do so, and mix them skilfully with very little guidance.

This point does not matter so long as we treat the dialogue as a philosophical commentary on mathematical research. After all, research mathematicians *are* well-informed sophisticates. The point becomes pertinent if we try to draw pedagogical lessons from the dialogue. Real students (except perhaps postgraduates) do not arrive in class knowing "trivially true theorem[s] of vector algebra"[20] or able to use mature heuristics such as deductive guessing.[21] Even with their knowledge and skill, it took Europe's leading mathematicians the best part of 300 years to get from the simple Descartes-Euler formula to Poincaré's algebraic version of the theorem. It is unlikely that a class of real students could cover the same distance in the time typically available for mathematics teaching. Certainly, they would need help.

[15] Lakatos (1976) pp. 10-11.

[16] *Op. cit.* pp. 33ff.

[17] *Op. cit.* pp. 57ff.

[18] *Op. cit.* pp. 88ff.

[19] "The class is a rather advanced one. To Cauchy, Poinsot, and to many other excellent mathematicians of the nineteenth century these questions did not occur." (*Op. Cit.* p. 8n3). See also p. 52n3.

[20] *Op. cit.* p. 117.

[21] *Op. cit.* pp. 70ff.

Proofs and Refutations offers no model here. The teacher intervenes frequently, but mostly with general methodological precepts. Aside from the initial formula and proof, the teacher presents very little mathematics. Moreover, the teacher's methodological interventions are sometimes rather trenchant (for example, "I abhor your pretentious 'insight'." p. 30). This would be inappropriate in a school (most pupils would react poorly to having their contribution abhorred by the authority figure). However, in this classroom, the teacher is as much a participant as a guide. The teacher in *Proofs and Refutations* avoids authoritarianism by refusing to claim any special intellectual authority. This option is closed to teachers at all but the highest levels.

In addition to licencing a robust critical vocabulary, the teacher's egalitarian approach serves as an example of intellectual honesty in the face of criticism.[22] Lakatos may have experienced teaching of this sort as a graduate student, in the classes of Professor Sándor Karácsony (1891–1952). Karácsony chaired Lakatos' Ph.D. dissertation committee and was his godfather at his mysterious conversion to Calvinism. According to Bandy and Long, "[Karácsony] was meticulous in critical dialogue with his students. … [one student] was amazed to find open class discussion, no one point of view being demanded, and Karácsony entering class one day with an acknowledgement of a mistake he had made in the previous session."[23]

In short, if we were to take the *Proofs and Refutations* dialogue seriously as a pedagogical model, it would suggest a crude, "hands-off" approach in which students have to discover the mathematics for themselves, without the advantages of knowledge, skill and time enjoyed by the first-rate mathematicians who discovered it first. The teacher's approach in the *Proofs and Refutations* dialogue might work in an advanced post-graduate seminar; at any lower level, it would deprive students of the help they need and intimidate them into silence. The model of teaching on display in *Proofs and Refutations* neglects the fact that students and teachers are not equal partners. This should come as no surprise. We should not allow the classroom setting to mislead us into expecting a pedagogical model. No-one would look to Plato's *Symposium* for advice on preparing a drinks party, still less for a theory of drinking.

Lakatos came closer to a direct discussion of mathematics teaching in the second appendix to the 1976 edition of *Proofs and Refutations*, called "The deductivist versus the heuristic approach".[24] Here, he complains about the "deductivist" manner of presenting mathematics, both in research papers and in textbooks:

> This style starts with a painstakingly stated list of *axioms, lemmas* and/or *definitions*. The axioms and definitions frequently look artificial and mystifyingly complicated. One is never told how these complications arose. The list of axioms and definitions is followed by the carefully worded *theorems*. These are loaded with heavy-going conditions; it seems impossible that anyone should ever have guessed them. The theorem is followed by the *proof*.[25]

[22] *Op. cit*. p. 11.

[23] Bandy and Long 2000 p. 89.

[24] Lakatos (1976) pp. 142-154.

[25] *Op cit*. p. 142.

In this "deductivist" style, the student cannot see the "heuristic background" to the theorem and proof, and is discouraged from asking about it. As in his review of *I Learn Physics* and his Petőfi Circle speech, Lakatos objects to deductivist presentation out of a political distaste for authoritarianism and a concern for the health of mathematics:

> …Showing the proof, the counterexamples, and following the heuristic order up to the theorem and the proof-generated definition would dispel the authoritarian mysticism of abstract mathematics, and would act as a brake on degeneration.[26]

The point, as before, is that when scientific or mathematical results appear without their heuristic backgrounds, it is difficult to criticise them, and when this happens in education, the students do not learn how to criticise objectively or even that such criticism is possible. Lakatos does not quite say that mathematicians and textbook-writers adopt the deductivist style *in order* to prevent criticism. Nevertheless, these pages bristle with the cavalier style and sweeping condemnations of the young Marxist Lakatos:

> It has not yet been sufficiently realised that present mathematical and scientific education is a hotbed of authoritarianism and is the worst enemy of independent critical thought.[27]

At the time when he wrote this, Lakatos' experience of mathematical and scientific education was largely confined to Hungary and interrupted by the war, so one may wonder what ground he had for making such an unqualified generalisation. Moreover, Lakatos' mathematical education was heavily influenced by the "Hungarian" tradition in mathematics, founded by Lipot Fejer (1880–1959). This "school" saw mathematics as inseparable from philosophy and heuristics. Its characteristic teaching style was a kind of Socratic dialogue that allowed advanced students to watch and participate in the birth and development of ideas. It included crucial agents in Lakatos' development, such as Alfred Renyi (who sponsored Lakatos at the Mathematics Research Institute at the Hungarian Academy of Sciences) and George Polya, along with Gabor Szego, John von Neumann and Paul Erdos. Far from being a lone voice, Lakatos was the inheritor of a mature and influential tradition of teaching in something like the style displayed in *Proofs and Refutations*.[28]

6.3 Polya on Teaching Mathematics and Mathematical Heuristics

Lakatos gave three historical examples of proof-generated concepts and theorems (uniform convergence, bounded variation, and the Carathéodory definition of measurable set), to demonstrate the importance of the heuristic background in

[26] *Op cit*. p. 154.

[27] *Op cit*. p. 142n2.

[28] See Jha (2006) pp. 258-260.

understanding a theorem and proof. However, the thought that mathematical proofs are mysterious without their heuristic backgrounds did not originate with Lakatos. Like much else in *Proofs and Refutations*, he owed it to Polya.

Polya illustrated[29] the point with a theorem of his own:

If the terms of the sequence $a_1, a_2, a_3 \ldots$ are non-negative real numbers, not all equal to 0, then $= \sum_{k=1}^{\infty} a_k \frac{(k+1)^k}{k^{k-1}} \frac{1}{k}$

How did anyone arrive at this theorem? It looks like it might have been inspired by thinking about arithmetic and geometric means, but what is *e* (the base of natural logarithms) doing there? The proof establishes that this theorem is true without explaining why. Here is the proof:

Define the numbers $c_1, c_2, c_3, \ldots c_n \ldots$ by: $c_1 c_2 c_3 \ldots c_n = (n+1)^n$

Then trivially:

$$\sum_1^{\infty} (a_1 a_2 \ldots a_n)^{1/n} = \sum_1^{\infty} \frac{(a_1 c_1 a_2 c_2 a_3 c_3 \ldots a_n c_n)^{1/n}}{n+1}$$

Using the inequality between geometric and arithmetic means:

$$\leq \sum_1^{\infty} \frac{a_1 c_1 + a_2 c_2 + \ldots + a_n c_n}{n(n+1)}$$

$$= \sum_{k=1}^{\infty} a_k c_k \sum_{n=k}^{\infty} \frac{1}{n(n+1)}$$

$$= \sum_{k=1}^{\infty} a_k c_k \sum_{n=k}^{\infty} \left(\frac{1}{n} - \frac{1}{n+1} \right)$$

$$= \sum_{k=1}^{\infty} a_k \frac{(k+1)^k}{k^{k-1}} \frac{1}{k}$$

$$< e \sum_1^{\infty} a_n$$

(The last step holds because $(k+1/k)^k$ approaches *e* monotonically from below.)

If anything, the proof deepens the mystery. The definition of the c_i turns out to be just the thing we need – but where did it come from? It is what Polya calls a "*deus ex machina*". "It is not enough" he says, "that a step *is* appropriate: it should *appear* so to the reader".[30] The reader should not have to take it on trust that a step is appropriate, and students should see the process of discovery laid out so that they can draw a lesson in heuristics. Polya gives a potted history of this theorem and

[29] Polya 1954 volume II (*Patterns of Plausible Inference*) p. 147.

[30] *Op. cit.* p. 148.

proof over four pages; I will not reproduce it here.[31] The theorem he eventually proved is not what he set out to prove. He began trying to prove something more straightforward, but his proof-idea did not work at his first attempt. After several trials and modifications, his proof-idea turned out to be apt to prove his eventual result. Thus, this is a proof-generated theorem in Lakatos' sense.

In short, the central claim that heuristic creates proof-generated theorems and thus shapes the content of mathematics is already there in Polya, as is the distinction between deductive and heuristic presentation styles. Moreover, Polya understood that there is more to teaching than presenting the material, however lucidly. He gives careful advice to teachers about the sort of questions they should ask students: questions should be as general as possible, drawn from a short list of heuristically useful questions that will thus take root by repetition. They should be questions that the student might have thought of spontaneously, such as "what are the data?" or "what is the condition?" Thus, "do you know a related problem?" is a better question than "can you apply the theorem of Pythagoras?"[32] The aim is to reproduce in the student the problem-solving skills and mental habits of the teacher.

This consideration of the practicalities of teaching is wholly absent from Lakatos, as is Polya's concern with the affective aspect of problem-solving. The first move in problem-solving, Polya explains, is to set the problem to oneself:

> A problem is not yet your problem just because you are supposed to solve it in an examination. If you wish that somebody would come and tell you the answer, I suspect that you did not yet set that problem to yourself… You need not tell me that you have set that problem to yourself, you need not tell it to yourself; your whole behaviour will show that you did. Your mind becomes selective; it becomes more accessible to anything that appears to be connected with the problem, and less accessible to anything that seems unconnected… You keenly feel the pace of your progress; you are elated when it is rapid, you are depressed when it is slow.[33]

Thus, problem-solving requires a felt commitment with its attendant emotional risks (if you never succeed, you may stay depressed). The teacher has to induce this commitment in students, though Polya says little about how, except that the problem set should be at the right level and arise naturally.[34] The experience of problem-solving, with its selective attention and emotional charge, is part of the educational purpose:

> Your problem may be modest; but if it challenges your curiosity and brings into play your inventive faculties, and if you solve it by your own means, you may experience the tension and enjoy the triumph of discovery. Such experiences at a susceptible age may create a taste for mental work and leave their imprint on mind and character for a lifetime.[35]

---

[31] *Op. cit.* p. 149-152. Of rational reconstructions, Polya says, "…the best stories are not true. They must contain, however, some essential elements of the truth… The following is a somewhat 'rationalised' presentation of the steps that led me to the proof…" p. 148.

[32] Polya (2004) pp. 20-22.

[33] Polya (1954) volume II (*Patterns of Plausible Inference*) pp. 144-145.

[34] Polya (2004) p. 6.

[35] *Op. Cit.* p. v (from the preface to the first printing).

Thus, Polya's motive is quite different from that of Lakatos. Instead of a young political activist railing against authoritarianism, we have an experienced teacher who wishes to share with students the joy of discovery. Polya wanted his students to see what he saw, feel what he felt, care about the things for which he cared. He takes it as given that education includes shaping pupils' minds and characters to emulate existing models.[36] Lakatos had nothing to say about students' experiences, sensibility, cognitive values, quality of mind or character, beyond the exhortations already expressed in his Petőfi Circle speech.

It would be too much to describe Polya as an authoritarian. However, Polya was not a fallibilist.[37] He held the orthodox view that a complete deductive proof establishes a theorem for all time. There would be no point in students attempting to challenge such knowledge, and therefore no point in teachers encouraging such challenges. Polya wanted students to think for themselves, but only so that they could solve problems, not so that they might question the wisdom of their elders. Mathematical education should encourage independence and self-possession,[38] but teachers are models to imitate[39] rather than masters to challenge. The authority of Polya's ideal teacher is mild, benign, but not subject to serious criticism.[40] No doubt, Polya hoped that his students would surpass him and perhaps explore in directions that he would not have considered. But, unlike Lakatos, Polya did not discuss the possibility that students might raise doubts about the value of the curriculum content or the truth of its theorems.

6.4 Does Fallibilism Bring Anything Useful to the Mathematics Classroom?

Polya's attention to the affective, motivational and (so to speak) existential aspects of the student experience is a clear advantage over Lakatos, who, as we have seen, does not have a pedagogy worthy of the name, partly because he pays no attention to the student whatsoever. On the other hand, Lakatos is a fallibilist. In Polya's

---

[36] "Teaching to solve problems is education of the will" *Op. Cit.* p. 94.

[37] "We shall attain complete certainty when we shall have obtained the complete solution, but before obtaining certainty we must often be satisfied with a more or less plausible guess." *Op. Cit.* p. 113.

[38] "The mathematical experience of the student is incomplete if he never had an opportunity to solve a *problem invented by himself*." *Op. Cit.* p. 68. "…he should endeavour to make his first important discovery: he should discover his likes and his dislikes, his taste, his own line." *Op. Cit.* p. 206.

[39] "[The future mathematician] should look out for the right model to imitate. He should observe a stimulating teacher." *Op. Cit.* p. 206.

[40] Perhaps Lakatos had a similar figure in mind when he insisted that control of the curriculum at the LSE should remain exclusively with the professors. See also 'The Traditional Mathematics Professor' (*Op. Cit.* p. 208)

heuristic reconstructions, unsuccessful proof-ideas and solution-attempts fail straight away; there are no *undetected* false lemmas or logical errors waiting for criticism to expose them. Whereas for Lakatos, a mistake may (in principle) go undetected and a flawed proof of a false theorem can enter the canon of mathematical knowledge.[41] Does this confer any pedagogical advantages?

We must first distinguish between two sorts of fallibilism. In the early part of *Proofs and Refutations*, the fallibilism is roughly-speaking Popperian. The mathematical community accepts a false conjecture, perhaps with an invalid or unsound proof in attendance, until someone exposes the error, perhaps with a counterexample. The pupils argue about how to handle polyhedral oddities, but they do so on the assumption that "polyhedron" has a stable and precise meaning. However, it quickly becomes apparent (to the reader if not to the class) that the central terms are in flux, being stretched and deformed. Eventually, the class notices and discusses these semantic shifts, led by Pi, who explains that heuristically interesting refutations always involve changes in language: "Heuristic is concerned with language-dynamics, while logic is concerned with language-statics".[42] The fallibilism in the later pages of *Proofs and Refutations* is at once more subtle and more plausible than the Popperian variety in play at the outset. It is unlikely that a long-established theorem will succumb to an unproblematic counterexample or logical error as in a Popperian refutation. Such cases seem to be extremely rare in the history of mathematics. It is, however, plausible that semantic shifts and changes of theoretical interest might cause a body of mathematical work to fall into disuse, and its theorems lose their canonical status.

This latter fallibilism, which depends on language-dynamics for its heuristic refutations, may be historically and philosophically more plausible than the Popperian variety in which language is clear and stable, and theories are decisively refuted by facts. But is it pedagogically useful? The example of literary study is not encouraging. There, teachers' attempts to teach the insight that even apparently stable meanings of words are never quite still (the "floating signifier") have mostly backfired. Deconstruction, whatever its philosophical merits, has largely frustrated the pedagogical efforts of literature teachers, including those who hoped to use deconstruction to teach an anti-authoritarian moral. The reason is clear: if all signifiers float, and all readings are therefore to some degree conventional, then criticism loses its bite. Critics can dethrone authorities by pointing out that their terms are semantically unstable, but this criticism is itself partly conventional and thus vulnerable to a well-aimed *tu quoque*. Moreover, the "floating signifier" point applies to *all* texts and therefore says nothing distinctive about any. To escape this sceptical bind without hiding or denying the fact that meanings do stretch, shift and break,

---

[41] Aside from early attempts to prove the Descartes-Euler formula, Lakatos gives the example of Cauchy's 1821 proof that the limit of any convergent series of continuous functions is continuous (Lakatos 1976 appendix 1). However, even in this case, Lakatos indicates that Cauchy and others knew straight away that something was not right (*Op. Cit.*p. 131). Thus, even in his best example, Lakatos could not show us the dramatic refutation of an apparently secure theorem.

[42] *Op. Cit.* p. 93.

we need a robust account of why some texts are better than others. To return from literature to mathematics, we need an explanation of how heuristic "refutations" expose real faults in their targets. We need an account of heuristic progress in mathematics. We do not have this explicitly in Lakatos.[43] Given this lack, it would be rash to unleash language-dynamics on any but the most advanced classrooms.

6.5 Some Tentative Suggestions for Classroom Practise

In spite of this negative conclusion, there are other ways of combining the merits of Lakatos and Polya. One is obvious: they agree that heuristic presentations are preferable to proofs that require students to trust in a *deus ex machina*. This need not entail any serious engagement with the history of mathematics; all it requires is that every step in a proof should be motivated at the point where it is made. For this, an unhistorical heuristic presentation will suffice. The next step is also easy to see: there is no reason why students at secondary school should not learn proof-analysis. Teachers would have to supply students with carefully constructed faulty proofs, at least at first.[44] Part of the point of teaching proofs is that students should learn basic logical notions (premise, inference, conclusion, etc.) in addition to learning the mathematical material. Fixing faulty proofs suggests itself as a promising activity for learning these notions, including some concepts (counterexample, *modus tollens*) which do not arise naturally in the contemplation of sound proofs.

Less obvious benefits flow from combining Lakatos' interest in the history of mathematics with Polya's concern for developing student morale, motivation and character. The first is to encourage students by showing them that famous great dead mathematicians struggled with ideas that we now consider elementary. Students should know that it took the whole community of mathematicians an extended effort to understand complex numbers, or convergent series, or even negative numbers and decimal notation. This is not history deployed to make mathematics "relevant". Rather, the point is to encourage perseverance. It is discouraging to struggle with something apparently basic, if you do not know that it took many skilled hands to establish it in the first place.

Learning that famous brains also struggle is encouraging, but not so valuable as the experience of being convinced of some proposition, seeing that you were mistaken,

---

[43] Though the heuristic patterns in *Proofs and Refutations* are instructive. Lakatos addressed the corresponding problem in the philosophy of science with his Methodology of Scientific Research Programmes (1978a). Some authors have tried to carry this model (or parts of it, with modifications) from natural science into mathematics (see Hallett 1979, Koetsier 1991, Corfield 2003); for criticism of MSRP see Larvor (1998) esp. chapters four and six; for criticism of Methodologies of Mathematical Research Programmes, see *Op. Cit.* and Larvor (1997). For a consideration of Kuhnian approaches to the question of progress in mathematics, see Gillies (1992). For semantic shifts in mathematics, see Derrida (1978) or Grosholz (2007).

[44] Polya gives some examples, including a proof that all girls have the same colour eyes.

and then achieving cautious confidence in an improved version of the original thought. Such an experience obviously presents an opportunity to learn heuristic lessons, but it is also character-forming. Students should learn to suspect inarticulate or untested convictions, and they should form the habit of running conjectures through plausibility tests. Indeed, part of the point of this exercise is to come to regard established orthodoxies and personal convictions alike as conjectures. Polya emphasised the importance of feelings of relevance and confidence, but such feelings do not occur naturally; they have to be developed and refined. Discovering that one sometimes misplaces one's confidence is part of that process. This experience is particularly important for those students who will go on to become authorities (on any subject, not only mathematics).

Finally, there is a case for "live" problem-solving, that is, where the teacher does not know the answer and may make mistakes. Teachers could take problems from a central source that does not publish the answers straight away, so the students know that the teacher is solving the problem for real. Aside from practising heuristics, students can then learn how to challenge each other's claims (and those of the teacher) politely and respectfully. They can learn the art of preventing a rational discussion from degenerating into a blazing row. It has to be live, so that the teacher shares the students' vulnerability. Teachers expect students to leave their comfort zones and run the risk of making errors in public. The teacher can gain moral authority by running the same risk. The teacher must not bluff or bluster, and must take seriously the thought that a student may have an insight first. If the teacher makes an error, he or she should acknowledge it, as Karácsony apparently did. Such intellectual honesty is the difference between "authoritative" and "authoritarian".

References

Bandy, A., & Long, J. (2000). Dress rehearsal for a revolution? *The Hungarian Quarterly, XLI*, 157.

Corfield, D. (2003). Towards a philosophy of real mathematics. Cambridge/New York: Cambridge University Press.

Derrida, J. (1978). *Edmund Husserl's origin of geometry: an introduction*. New York: Nicolas Hays (J. P. Leavey, Trans.).

Feyerabend, P., & Lakatos, I. (1999). *For and against method*. Chicago: University of Chicago Press. Motterlini (ed).

Gillies, D. (1992). *Revolutions in mathematics*. Oxford/New York: Clarendon Press/Oxford University Press.

Grosholz, E., & Breger, H. (2000). *The growth of mathematical knowledge*. Boston: Kluwer.

Grosholz, E. R. (2007). *Representation and productive ambiguity in mathematics and the sciences*. New York: Oxford University Press.

Hallett, M. (1979). Towards a theory of mathematical research programmes (in two parts). *British Journal for the Philosophy of Science*, 30, 1–25, 135–159.

Jha, S. R. (2006). Hungarian studies in Lakatos' philosophies of mathematics and science – Editor's Introduction. *Perspectives on Science, 14*(3), 257–262.

Koetsier, T. (1991). *Lakatos' philosophy of mathematics: a historical approach* (Studies in the History and Philosophy of Mathematics Vol. 3). Amsterdam: North Holland.

Lakatos, I. (1947). Eötvös Kollegium – Gyorffy Kollegium. *Valóság*, 3.

Lakatos, I. (1976). *Proofs and refutations*. In J. Worrall, & E. Zahar (Eds.) Cambridge: Cambridge University Press.

Lakatos, I. (1978a). *The methodology of scientific research programmes (Philosophical papers, Vol. 1)*. In J. Worrall, & E. Curry (Eds.) Cambridge: Cambridge University Press.

Lakatos, I. (1978b). *Mathematics, science and epistemology (Philosophical papers, Vol. 2)*. In J. Worrall, & E. Curry (Eds.) Cambridge: Cambridge University Press.

Larvor, B. (1997). Lakatos as historian of mathematics. *Philosophia Mathematica, 5*(1), 42–64.

Larvor, B. (1998). *Lakatos, an introduction*. London: Routledge.

Long, J. (1998). Lakatos in Hungary. *Philosophy of the Social Sciences, 28*, 244–311.

Polya, G. (1954). *Mathematics and plausible reasoning*. Princeton, NJ: Princeton University Press.

Polya, G. (2004). *How to solve it: a new aspect of mathematical method*. Princeton Science Library Edition with a new foreword by John Conway. (Original publication 1945).

Chapter 7
Proofs as Bearers of Mathematical Knowledge

Gila Hanna and Ed Barbeau

This paper aims to explore, from the point of view of mathematics education, Yehuda Rav's inspiring paper "Why do we prove theorems?" (Rav 1999). His central thesis is that the "essence of mathematics resides in inventing methods, tools, strategies and concepts for *solving problems*" (p. 6). From this thesis Rav draws the conclusion that proofs should be the primary focus of mathematical interest, because it is proofs that embody these very methods, tools, strategies and concepts, and thus are the bearers of mathematical knowledge.

While Rav's focus is on the practice of mathematics, ours is on its teaching. Educators have long recognized the explanatory value of many proofs, but they have had in mind primarily the light such explanatory proofs can shed on the mathematical subject matter with which they deal. This paper aims to show that proofs can also be bearers of mathematical knowledge in the classroom in another sense, the sense proposed by Rav: that proofs have the potential to convey to students "methods, tools, strategies and concepts for *solving problems*." (p. 6)

The paper is divided into two parts: the first part elaborates upon Rav's thesis, and the second part presents examples of proofs from the mathematics curriculum and discusses their role in conveying mathematical knowledge.

7.1 Exposition of Rav's Thesis

The consensus among mathematicians, philosophers and mathematics educators is that proofs are central to mathematics, primarily because it is a proof that establishes the truth of a mathematical claim. Rav (1999) does not dispute this, but he asserts that there is an aspect of proof that has been overlooked, and that the importance of proof goes well beyond the establishment of mathematical truth. In his

G. Hanna (✉)
Department of Curriculum, Teaching and Learning, Ontario Institute for Studies in Education of the University of Toronto, Toronto, Ontario, Canada, M5S 1V6
e-mail: ghanna@oise.utoronto.ca

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives,* DOI 10.1007/978-1-4419-0576-5_7,

view, a proof is valuable not only because it demonstrates a result, but also because it may display fresh methods, tools, strategies and concepts that are of wider applicability in mathematics and open up new mathematical directions.

Indeed, Rav believes that if the only role of a proof were to compel acceptance of a mathematical theorem, then mathematicians would be content to have a machine that answered "true" or "false" to any imaginable proposition (a machine to which he gives the name "Pythiagora"). This is only a thought experiment, of course, and Rav does not claim that such a machine could exist even in principle. His point is that mathematicians would not be satisfied even if such a machine did exist.

Mathematicians would not be satisfied because reliance on a machine, by making proofs unnecessary, would stunt the growth of mathematics. In Rav's view, proofs are indispensable to the broadening of mathematical knowledge and are in fact "the heart of mathematics, the royal road to creating *analytic tools* and catalyzing growth" (p. 6). The very act of devising a proof contributes to the development of mathematics. Proofs yield new mathematical insights, new contextual links and new methods for solving problems, giving them a value far beyond establishing the truth of propositions. As Rav states his thesis, "*proofs rather than the statement-form of theorems are the bearers of mathematical knowledge*" (italics in source, p. 20).

In his paper he supports this view through a series of examples. But he first makes a distinction between two kinds of proof. The first kind he calls a "derivation," which is a formal proof, that is, a "syntactic object of some formal system" (p. 11). Such a proof is the syntactical application of rules of logical inference. It consists of a finite string of formulae, to which no meaning need be assigned; the formulae are either axioms or derived from axioms. A machine could verify such derivations without having to appeal to the meaning of the constituent formulae.

The second kind of proof is a "conceptual proof," by which Rav means an informal proof "of customary mathematical discourse, having an irreducible semantic content" (p. 11). Such a proof consists of a rigorous argument acceptable to mathematicians, but it does make appeal to the meaning of the concepts and formulae used. Though a conceptual proof does not have a precise mathematical definition, mathematicians would readily understand its overall structure and could verify the correctness of the each of its steps. Most proofs submitted to scholarly journals of mathematics are conceptual proofs.

While acknowledging the importance of derivations (i.e., formal proofs) as a branch of mathematical logic, and conceding that "current mathematical theories can be expressed in first-order set-theoretical language," Rav excludes formal proofs from further discussion in his paper. He makes it clear that when he uses the term "ordinary mathematical proofs" he means "conceptual proofs." It is worth stressing here that when Rav says, in the central thesis of his paper, that proofs are the "bearers of mathematical knowledge," it is conceptual proofs that he has in mind.

Since it is primarily these ordinary mathematical proofs, rather than formal derivations, that students of mathematics encounter at all levels, Rav's paper is of particular interest to the teaching of mathematics.

The kind of new knowledge that a proof may bring to mathematics can be shown by the following two examples. In the first, Rav demonstrates that attempts at proving the Goldbach Conjecture yielded several new methods as well as many new results in number theory and related fields (pp. 7–8). Rav mentions in particular the sieve method (Brun's sieve), which enjoyed wide application and was used, for example, in obtaining the following results in number theory:

(a) There exist infinitely many integers n such that both n and $n+2$ have at most nine prime factors;
(b) Every sufficiently large even integer is the sum of two numbers each having at most nine prime factors. (pp. 7–8)

In his second example, Rav shows that attempts at proving the Continuum Hypothesis also had many remarkable consequences. Techniques developed in the course of attempting a proof of this hypothesis led to the formulation and proof of the "two-class theorem" by Cantor (p. 9), to developments in topology, and to Hilbert's concept of "the definability of objects by recursive schemes" (p. 11); they also provided important tools in the seminal work of Gödel.

Rav is not the only one who assigns to proofs a role that goes well beyond demonstrating *that* a theorem is true and *why* a theorem is true. Avigad (2006) lends support to Rav's central thesis when he says:

> We do have some fairly good intuitions as to some of the reasons that one may appreciate a particular proof. For example, we often value a proof when it exhibits methods that are powerful and informative; that is, we value methods that are generally and uniformly applicable, make it easy to follow a complex chain of inference, or provide useful information beyond the truth of the theorem that is being proved. (p. 2)

Avigad adds that in describing mathematical practice, which he sees as consisting largely of proving, a philosopher is compelled to examine the association of proof and method. For Avigad, this association could take two distinct forms. On the one hand, a proof could be associated with the creation of a novel method used in proving a particular result (for example, the method of Gauss sums that is used to prove the law of quadratic reciprocity), and on the other hand a proof could make use of an existing method in order to demonstrate its worth in a different proving context. According to him, "In both situations, praise for the proofs can be read, at least in part, as praise for the associated methods." (p. 107).

Rav's thesis also finds support from Bressoud. In his book *Proofs and Confirmations: The Story of the Alternating Sign Matrix*, Bressoud (1999) recounts the 20-year history of the proof of the conjecture about the number of Alternating Sign Matrices, culminating in its completion by Doron Zilberger, announced in 1992 and finally accepted in 1995 (Bressoud and Propp 1999, p. 643). Bressoud's intention is to tell a story of the "discovery of new mathematics" and "to guide you (the reader) into this land and lead you up some of the recently scaled peaks" (1999, p. xiii).

He shows how various attempts at reaching a proof relied on techniques from the theories of partitions, functions, polynomials and determinants, as well as from statistical mechanics, to name only some of the areas. More importantly for the topic of this paper, Bressoud shows how the attempts at proving the Alternating Sign Matrix conjecture, along with the finished proof, not only introduced new methods but also suggested new avenues for research.

It is also enlightening to note the following comment by Zeilberger:

> "The fact that a conjecture resists vigorous attacks by skilled practitioners is an impetus for us either to sharpen our existing tools, or else to create new ones. The value of a proof of an outstanding conjecture should be judged, not by its cleverness and elegance, and not even by its 'explanatory power,' but by the extent in which it enlarges our toolbox." (as cited in Bressoud 1999, p. 190)

Dawson (2006), having analyzed the reasons why mathematicians reprove theorems, lends additional support to Rav's claim that the often innovative strategies and methods embodied in proofs, rather than the theorems proved, are the primary value that proofs bring to mathematics. Dawson presents a persuasive analysis showing that there are eight reasons that propel mathematicians to seek new proofs to theorems that have already been accepted, and that most of these reasons have to do with methods: (1) "To remedy perceived gaps or deficiencies in earlier arguments"; (2) "To employ reasoning that is simpler, or more perspicuous, than earlier proofs"; (3) "To demonstrate the power of different methodologies"; (4) "To provide a rational reconstruction (or justification) of historical practices"; (5) "To extend a result, or to generalize it to other contexts"; (6) "To discover a new route"; (7) "Concern for methodological purity"; and (8) "The existence of multiple proofs of theorems serves an overarching purpose that is often overlooked, one that is analogous to the role of *confirmation* in the natural sciences" (Dawson 2006, pp. 275–281).

Corfield (2003) would also appear to reiterate the thoughts elaborated above when he states: "What mathematicians are largely looking for from each other's proofs are new concepts, techniques, and interpretations" (p. 56). He clearly shares with Rav the view that there is more to proof than establishing the truth or falsity of a proposition.

7.2 Proof in Mathematics Education: Beyond Justification and Explanation

Proof and its teaching have been extensively discussed for the last two decades in the literature on mathematics education and in particular in the proceedings of the International Group for the Psychology of Mathematics (Mariotti 2006). But Rav's specific idea, that proof is a bearer of mathematical knowledge, has not been explicitly discussed. The research on proof in mathematics education seems to have dealt primarily with the logical aspects of proof and with the problems encountered in having students follow deductive arguments.

These areas of emphasis are apparent from the specific issues addressed in much of this recent research. The following is by no means an exhaustive list of issues, but is fairly representative: The epistemological aspects of proof (Balacheff 2004; Hanna 1997); the cognitive aspects of proof (Tall 1998); the role of intuition and schemata in proving (Fischbein 1982, 1999); the relationship between proving and reasoning (Yackel and Hanna 2003; Maher and Martino 1996); the usefulness of heuristics for the teaching of proof (Reiss and Renkl 2002); the emphasis on the logical structures of proofs in teaching at the tertiary level (Selden and Selden 1995); proof as explanation and justification (Hanna 1990, 2000; Sowder and Harel 2003); proof and hypotheses (Jahnke 2007); curricular issues (Hoyles 1997); proof in the context of dynamic software (Jones et al. 2000; Moreno and Sriraman 2005); the analysis of mathematical arguments produced by students (Inglis et al. 2007); the relationship between argumentation and proof (Pedemonte 2007). Understandably, the empirical classroom research on the teaching of proof has focused upon students' difficulties with learning proof and on the design of effective teachers' interventions (see the survey of research in the last 30 years in Mariotti 2006).

There are some exceptions to the emphases mentioned above. Lucast (2003) presents a case for "Proof as method: a new case for proof in mathematics curricula," in which it is argued that "proof is valuable in the school curriculum because it is instrumental in the cognitive processes required for successful problem solving" (p. 1). Lucast maintains that proof and problem solving are largely the same process and that both lead to "understanding," and her emphasis is on models of problem solving and their bearing on justification. The present paper, on the other hand, aims to show that in mathematics education a proof can be used to teach mathematical methods and strategies.

Bell (1976) and de Villiers (1990) discussed various meanings and functions of proof. De Villiers (1990, p. 18) listed five functions that he described as "… a slight expansion of Bell's (1976) original distinction between the functions of verification, illumination and systematization." These functions are (bold and italics in the source): "(1) *verification* (concerned with the **truth** of a statement), (2) *explanation* (providing insight into **why** it is true), (3) *systematization*, (the **organization** of various results into a deductive system of axioms, major concepts and theorems), (4) *discovery* (the discovery or invention of **new** results) and (5) *communication* (the **transmission** of mathematical knowledge)." This list stopped short of stating that proof contains techniques and strategies useful for problem solving, as Rav claims.

It is true that there are some twenty mathematics education research papers on proof that refer to Rav's paper (Google scholar, May 2007). These papers did not focus, however, on Rav's view that proofs "are the bearers of mathematical knowledge." Their references to Rav's paper were made in order to call attention to two other well-argued points: Rav's objection to a view of mathematics too tightly bound up with formalism, and his emphasis on the social dynamics for attaining reliability in mathematics.

In assessing whether Rav's central idea can be applied profitably in the classroom, there are several considerations. The first and most important is whether there are indeed mathematical examples of proof at the secondary-school level that lend themselves to the introduction of mathematical methods, tools, strategies and concepts which Rav so values. More precisely, are there such examples that are not disruptive to the curriculum? It is the main contention of this paper that such examples of proof can indeed be found in the regular syllabus. We will consider two cases in the next section.

The following two examples deal with proofs that are common to most secondary-school mathematics curricula around the world. They are presented as case studies, in which the proofs have been annotated extensively to demonstrate that they do have the capacity to expand the students' toolbox of techniques and strategies for problem solving.

The demonstration of this capacity is central to the thesis of this paper that Rav's insights have applicability in the classroom. Accordingly, the following case studies concentrate on properties intrinsic to the proofs and not on the ways in which they might be taught or understood by the students. While the proofs do in fact justify the correctness of their conclusions, we do not deal with the logical features of the proofs, or with the degree to which they might be convincing.

7.2.1 Case I: The Quadratic Formula

While students at school get exposed to very few "theorems," particularly in areas other than geometry, they nevertheless have to learn a few formulae, which are essentially statements of results. An example of this is the formula for the solution of a quadratic equation.

The solutions of the quadratic equation

$$ax^2 + bx + c = 0 \qquad \text{where } a \neq 0, \text{are given by } x = \frac{-b \pm \sqrt{b^2 - 4ac}}{2a}$$

At the most basic level, the students may simply use this formula to solve particular quadratic equations. It is even possible for them to apply it blindly, not realizing that they can check their solutions by substituting back into the equation. However, if they do make such substitutions, then, on empirical grounds, they will undoubtedly come to trust it and apply it mechanically.

At this point, students may perceive that there are two independent methods of solving quadratic equations, one, factoring, that is not guaranteed of success, and the other, use of the formula, which will work all of the time.

One way to establish the formula is to substitute the values of x given by the formula and verify that they do indeed satisfy the quadratic equation. This is a legitimate proof, but does it leave anything to be desired? On the plus side of the ledger, it emphasizes what the formula actually delivers: values of the variable that satisfy the equation. On the minus side, apart from the messiness of the substitution, how likely is it that students will be able to apply it flexibly and reliably? There is no indication of the significance of the formula, how such a complicated expression might arise, and how it might fit in with other properties and applications of the quadratic and related functions. The formula is a black box.

Simply verifying that the formula works has another defect: We do not know that it yields the only solutions of the quadratic equation. There may be other numbers that satisfy it, and perhaps we may come across a situation in which these alternatives are what we want.

An actual discussion of how the formula is obtained leads us to questions of strategy. In the present case, we might frame the question differently. Instead of asking, "What is a formula for the solutions of a quadratic equation?" we ask, "How can we solve a quadratic equation?" The second question induces us to think about process rather than product, and to consider how we might start.

For example, we might ask whether there are quadratic equations that are easy to solve. There are two possible answers that we might give. First, we can solve equations when the quadratic is factorable into linear polynomials. Secondly, we can solve quadratic equations of the form $x^2 = k$, when k is positive; indeed, in this case the answer is: $x = \pm\sqrt{k}$. Is there any way we can reduce the problem of solving a general quadratic to either of these cases? We note that in fact these are related; the equation $x^2 = k$ can be converted to $0 = x^2 - k = (x - \sqrt{k})(x + \sqrt{k})$. (Note: It may be necessary in some circumstances to satisfy students that $x^2 = k$ has only these two solutions. This might be done by considering the monotonicity of the function x^2 or by appealing to the fact that the product of two nonzero quantities cannot vanish. Either way inducts students into the underlying structure.)

Most students will probably not know how to proceed from here on their own, and will have to be taught the technique of completing the square. But such considerations will inform the technique when it is presented. What makes it easy to solve $x^2 - k = 0$ is the absence of the linear term, and so we need to perform a gambit in effect to absorb the linear term in the general equation. The key recognition is that $ax^2 + bx$ can be rewritten as $a(x^2 + \frac{b}{a}x)$, and that the quantity in parenthesis comprises the first two terms in the expansion of $(x + \frac{b}{2a})^2$ and differs from this expansion by a constant, namely, $\frac{b^2}{4a^2}$. Thus we "complete the square"; add a term $\frac{b^2}{4a^2}$ on the left side to give us the square of a linear polynomial, and then subtract it again, in effect adding 0. When $a \neq 0$, we transform $ax^2 + bx + c = 0$ to:

$$x^2 + \frac{b}{a}x + \frac{b^2}{4a^2} = -\frac{c}{a} + \frac{b^2}{4a^2} \Leftrightarrow x^2 + \frac{b}{a}x + \frac{b^2}{4a^2} = -\frac{4ac}{4a^2} + \frac{b^2}{4a^2} \Leftrightarrow (x + \frac{b}{2a})^2 = \frac{b^2 - 4ac}{4a^2}$$

and finally arrive at the formula: $x = \frac{-b \pm \sqrt{b^2 - 4ac}}{2a}$ (where $a \neq 0$).

This may be the first time that secondary school students see this general technique of adding and then subtracting a term in an expression, a useful technique that they will see frequently as they advance their study of mathematics. We note here that completing the square does not stem logically from a previous statement or axiom. Rather it is a topic specific move and an additional mathematical tool for the students to use in other similar situations.

By adding this technique to their toolkit, students may be able to take advantage of situations where it is more efficient to use this technique rather than to simply apply the formula. For example, given the task of solving $x^2 - 8x - 48 = 0$, and not recognizing a factorization, the student could just as easily render the equation as $(x-4)^2 - 64 = 0$ as apply the formula.

Having explicitly identified the ingredients of the situation, we can play around with them. Both factoring quadratics and using the formula lead to solutions of the equation. But we can use the formula also to *obtain* a factorization for any quadratic, even if the coefficients have to be non-integers. Since students going on in mathematics will inevitably meet situations, other than solving equations, in which factoring a polynomial is desirable, we have to be sensitive to possible procedures for this. Even more useful than the formula itself is the strategy – completing the square. The following example will illustrate.

Consider the quartic polynomial: $x^4 + 4$

Is this factorable over the integers? It is not obvious that it is. However, if students have been able to absorb the essence of the square-completion technique, then some might be able to complete the square in a different way to get

$$(x^4+4x^2+4)-4x^2=(x^2+2)^2-(2x)^2=(x^2-2x+2)(x^2+2x+2)$$

There is the possibility of students being able to leap ahead in the curriculum. The equation $x^4 + 4 = 0$ would normally require some knowledge of complex numbers and roots of unity to solve; however, from the above factorization as a product of quadratics, even a student in the lower secondary grades would be able to generate a solution.

We might also ask, if we can complete the square, why not complete the cube, and apply an analogous technique to solving

$$ax^3 + bx^2 + cx + d = 0.$$

The left side can be written as

$$a(x+\frac{b}{3a})^3 + (c-\frac{b^2}{3a})x + (d-\frac{b^3}{27a^2}) = 0$$

In this way, we can reduce the problem of solving any cubic to solving cubics of the form $x^3 - px + q = 0$, which is the usual starting point for general methods of solving the cubic. In a similar way, we can arrive at $x^4 + ax^2 + bx + c = 0$ as a "canonical form" for equations of the fourth degree.

If we follow the invitation of the proof to consider equations of the third and fourth degrees, we realize that we have developed means of expressing the roots in terms of the coefficients, using the four arithmetic operations along with the extraction of square and cube roots. It is a natural question to ask whether the solutions of higher degree equations are attainable from the arithmetic operations and extraction of roots of any order applied to the coefficients.

Delving into the proof reinforces an important perception that students should have about algebra. In any algebraic quest, we are in the business of reading off information from an expression. Sometimes the information can be easily read off, and sometimes it is buried and needs to be brought to light. The purpose of algebraic manipulation is to cast an expression into a form in which the desired information can be drawn. In the case of a quadratic, we have the standard form in descending powers of the variable, the factored form as a product of linear factors and the completion of the square. The factored form allows us to immediately read off its roots. When we use the completion of the square form, as shown above, while we need an additional step to solve the equation, we can see right away where the quadratic polynomial assumes its maximum or minimum value and exactly what that value is. In fact, we do also get some information about the roots as well. If both a and $4ac-b^2$ are positive, for example, then we can see that the quadratic is positive for all real values of x and so has no real roots.

Thus we see that consideration of the proof has benefits that go far beyond the mere validation of a formula. In the present case, we gain the perception of reducing the general situation to a canonical type, the understanding of how the character of the roots depends on the coefficients, the certainty that the quadratic equation can have no more that two roots. More importantly from the point of view of this paper, we gain the knowledge of a technique whose range of applicability is wider than the situation at hand, and a broader knowledge of quadratics that can be knitted into a more comprehensive whole.

7.2.2 *Case II: An Angle Inscribed in a Semi-circle Is a Right Angle*

The various proofs of this theorem will highlight the mathematical knowledge they contain. In addition, they show mathematical results as markers on a path, ways of giving form to a mathematical journey. A proof tells us where a mathematical result lives, about its neighborhood and associates; it highlights the significant ideas that underlie it.

Proposition. Let *A* and *B* be opposite ends of the diameter of a circle and let *C* be a point on its circumference. Then angle *ACB* is right.

This geometric result is familiar to many high school students. Although it is simply stated, there are many dimensions to it and the mere statement of the result will inevitably fail to convey its richness. As with any geometric result, certain properties are highlighted for consideration and related; the posited relationship might seem quite mysterious and incomprehensible. In order to feel more at home and perceive that the result is somehow natural, it is desirable to probe deeply and sense how the mathematical structure is woven together. This particular result can be approached from many directions (Barbeau 1988), and the purpose of what follows is to comment on the mathematical content of some of these.

The standard argument makes the observation that with *O* the centre of the circle, *OA*, *OB* and *OC* are all equal and so we have some equal angles in isosceles triangles and draw the conclusion that the angle at *C* is the sum of the angles at *A* and *B*, and so is equal to 90°. This argument highlights the significance of the circle hypothesis – the centre bisects the diameter and is equidistant from *A*, *B* and *C* (see Fig. 7.1).

What are the other ingredients? We need a theorem about isosceles triangles and about the sum of the angles in a triangle. The last raises the question of the sort of geometry in which the result holds. This is a Euclidean result.

The standard argument also raises the question of the truth of the converse. Suppose that we have a triangle *ABC* whose right angle is at *C*. Then the angle at *C* is the sum of the angles at *A* and *B*; so we can construct a cevian *CO* which splits the angle at *C* so that angle ACO = angle CAO and angle BCO = angle CBO. This gives us a couple of isosceles triangles and so $AO = BO = CO$. Thus, *C* lies on a circle with centre *O* and diameter *AB*. This proof gives us a diagram that can be interpreted in two ways – one that gives us the result itself and the second that gives us its converse.

Suppose we tweak the diagram of this argument in another way. Produce *CO* to some point *X*, and note that the exterior angle *XOB* is twice angle *OCB* and exterior angle *XOA* is equal to twice angle *ACO* (see Fig. 7.2). Then the straight angle *AOB* is twice angle *ACB*, making the latter angle right. Looking at the matter in this way reveals that it is part of a larger picture.

By bending *AB* at *O*, we can now deduce, with the same argument, the result that angle *ACB* is half the angle subtended at the centre by a chord *AB*, so that the angle subtended by a chord at the major arc of a circle is constant (see Fig. 7.3). In a similar

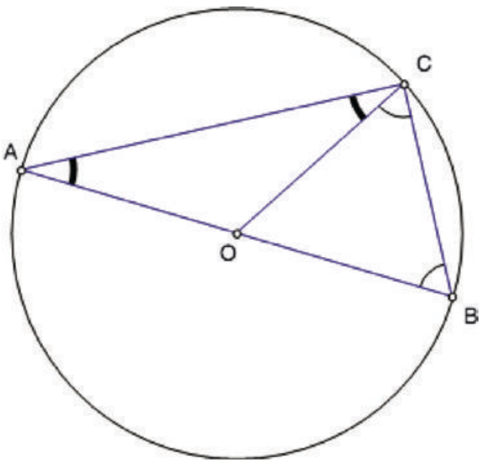


Fig. 7.1 Angle inscribed in a semi-circle

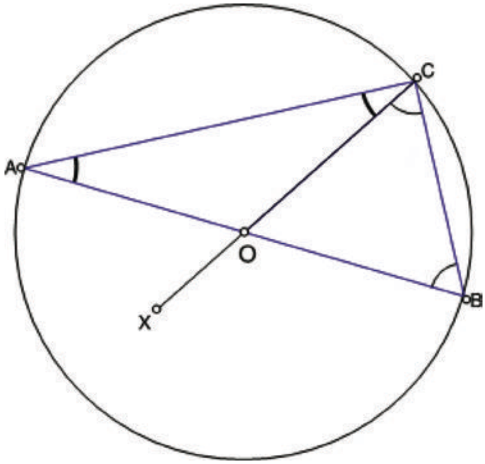


Fig. 7.2 Extending CO

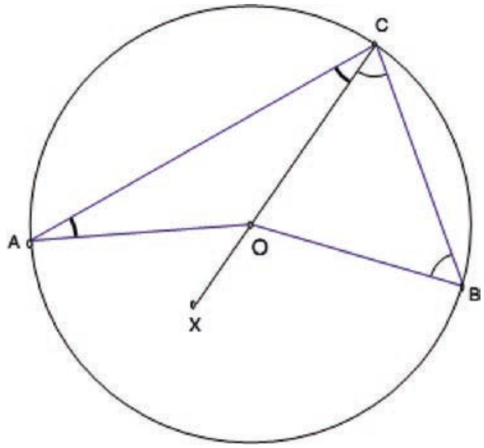


Fig. 7.3 Bending AB at O

way, it can be shown that the angle subtended at the minor chord is constant (and supplemental to the other angle). From here, it is a natural step to obtain some properties of concyclic quadrilaterals.

This more general result is not contained in the statement of the theorem, but by looking at the elements of the proof, we can arrive at it.

The next proof is the second transformation argument that involves a dilatation with factor 1/2 and centre B. This dilatation takes $A \to O$ and $C \to E$, the midpoint of chord CB. Now, E being the midpoint of chord CB means that OE right bisects it (this is basically a consequence of triangle COB being isosceles). Thus OE is perpendicular to *CB*. Now reverse the dilatation; since angles are preserved *AC* is perpendicular to *CB*, and we are done. This argument has quite a different flavor than the first one and introduces a symmetry element into the situation that is not apparent from the bald statement of the theorem. Thus the proof contains mathematical knowledge beyond mere deductive reasoning.

There are some areas of mathematics, such as algebra, calculus and trigonometry that provide a general framework for proving results of a particular type. In using general techniques, we are situating the result among a category of those that can be handled in a specific way. This focuses attention on the particular characteristics that make the techniques applicable. For example, we can conceive of the situation of the proposition in the cartesian plane, the complex plane or two-dimensional vector space (see Fig. 7.4). The proposition contains elements that are capable of straightforward formulation in each of these areas.

In the cartesian plane, the circle can be described by a simple quadratic equation and the condition for perpendicularity of two lines involves their slopes. If we coordinatize A, B and C as (−1, 0), (1, 0) and (x, y) where $x^2+y^2=1$, then we can check that 1 plus the product of the slopes of AC and BC is 0. In the complex plane, where multiplication by i corresponds to the geometric rotation through 90° about the origin, the proof becomes a matter of verifying that if A is taken to be −1, B as +1 and C as z where $z\overline{z}=1$ then (z−1)/(z+1) is a real multiple of $z-\overline{z}$ and so pure imaginary. Finally, the vector proof can be carried out with or without coordinates. In the latter case, the proof is particularly slick. Taking the centre of the circle as the origin of vectors, then $(C-B)\cdot(C-A)=C^2-C\cdot(A+B)+A\cdot B=0$ since $A=-B$ and $C^2=B^2=A^2$ is the square of the radius of the circle.

Some proofs reveal more than others; from some of the arguments, it can be quickly inferred that angle *ACB* is right if and only if *AB* is the diameter of a circle that contains *C*, so that the converse really is also built into the proof.

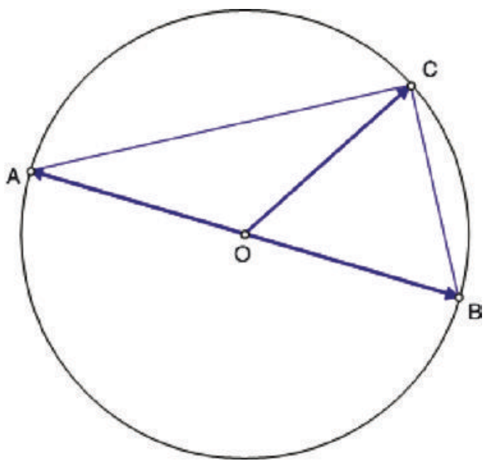


Fig. 7.4 Vector argument

In summing up the lesson of these case studies, one might consider that those students whose learning is most robust are likely to be those who have developed a multifaceted way of looking at mathematical facts. Their mathematical knowledge is rich with many connections and corroborations. One way of presenting our point in this paper is to say that the bald statement of results and practice of techniques in the classroom has little chance to foster this multifaceted view, while having to construct or follow well-chosen proofs, with the concomitant exposure to unfamiliar methods, tools, strategies and concepts that Rav has shown, can convey to the student a much richer understanding of mathematics.

Several additional examples could have been presented, such as the many different proofs of the infinitude of primes, each resting on a particular technique; the hundreds of proofs of the Pythagorean theorem, each using a different method or technique; the many proofs of numerical results that may be proved by mathematical induction or by an algebraic technique such as the telescoping method.

An example of the last is the finite sum of the series $\sum_{n=1}^{N} \frac{1}{n(n+1)}$ which can be treated as a telescoping sum, as follows:

$$\sum_{n=1}^{N} \frac{1}{n(n+1)} = \sum_{n=1}^{N} \left(\frac{1}{n} - \frac{1}{n+1}\right)$$

$$= \left(1 - \frac{1}{2}\right) + \left(\frac{1}{2} - \frac{1}{3}\right) + \cdots + \left(\frac{1}{N} - \frac{1}{N+1}\right)$$

$$= 1 + \left(-\frac{1}{2} + \frac{1}{2}\right) + \left(-\frac{1}{3} + \frac{1}{3}\right) + \cdots + \left(-\frac{1}{N} + \frac{1}{N}\right) - \frac{1}{N+1} = 1 - \frac{1}{N+1}$$

7.2.3 *Other Considerations*

It is clear that there do exist proofs that could be used productively, in the sense of Rav's thesis, in the secondary-school classroom. There are a number of questions that need to be answered, however. As we have not performed a classroom experiment, and as there do not seem to be initiatives based on this very specific approach to mathematical proofs, we will outline some issues that will need to be dealt with in a program of research.

What would be the effect on the current curriculum? Would drastic changes be necessary, or would only minor adjustments be required to infuse into the current syllabus this way of apprehending mathematics? If topics already in the syllabus are suitable for this new emphasis on method, then the main issues become those of expenditure of time and how crowded the syllabus becomes. One has to weigh whether this initial expenditure of time is going to pay off in the long term when students become more engaged, their learning is more robust, their understanding is deeper and they can progress more rapidly in later parts of the curriculum because they have assimilated valuable mathematical instincts.

Approaching proof as more than a formal way of certifying a result is bound to make increased demands on the teacher and involve more engagement by the students. The long-term value would seem to be clear, though not quantified, but can the increased demands be managed?

Finally, we must not forget the teachers themselves. What new material and orientation must they take on board for any changes in the classroom to be carried out effectively? What does this mean for professional development?

7.2.4 Conclusion

As discussed in the first part of this paper, Rav (1999) and others have shown that proofs can extend mathematical knowledge by bringing to the fore new techniques and methods, and Rav (1999) has maintained in fact that for this reason proofs should be a primary focus of interest in mathematics. We argue that what is true of mathematics itself may well be true of mathematics education: In other words, that proofs could be accorded a major role in the secondary-school classroom precisely because of their potential to convey to students important elements of mathematical elements such as strategies and methods.

It is important to call attention to the potential for exploiting this aspect of proof in the classroom. Mathematics educators have always made use of the fact that there are many different styles of proving, showing students how one can arrive at valid conclusions in different ways, using topic-specific moves, algebraic manipulations, geometric concepts, dynamic geometry, arithmetical computations, computing and more. Nevertheless, educators have overlooked to a large extent the role of proof as a bearer of mathematical knowledge in the form of methods, tools, strategies and concepts that are new to the student and add to the approaches the student can bring to bear in other mathematical contexts.

The adoption of the approach to proof which we have presented would require that proofs suitable for this teaching approach and for the secondary-school curriculum be assembled and polished and then be made available to teachers and curriculum planners. It would also necessitate research into the most effective ways to teach proofs with this new approach in mind.

The approach to using proof which we have discussed here does not challenge in any way the accepted "Euclidean" definition of a mathematical proof (as a finite sequence of formulae in a given system, where each formula of the sequence is either an axiom of the system or is derived from preceding formulae by rules of inference of the system), nor does it challenge the teaching of Euclidean derivation itself. It points out, however, that the teaching of proof also has the potential to convey to students other important pieces of mathematical knowledge and to give them a broader picture of the nature of mathematics. In highlighting a sometimes unappreciated value of proof, it also gives educators an additional reason for keeping proof in the mathematics curriculum.

Acknowledgements Preparation of this paper was supported in part by the Social Sciences and Humanities Research Council of Canada. We are grateful to Ella Kaye and Ysbrand DeBruyn for their assistance. We wish to thank the anonymous reviewers for their helpful comments.

A previous version appeared in *ZDM, The International Journal on Mathematics Education* 2008;40(3):345–353. It is reproduced by permission from Springer.

References

Avigad, J. (2006). Mathematical method and proof. *Synthese, 153*(1), 105–159.

Balacheff, N. (2004). The researcher epistemology: a deadlock from educational research on proof. Retrieved April 2007 from http://www-leibniz.imag.fr/NEWLEIBNIZ/LesCahiers/Cahiers2004/Cahier2004.xhtml.

Barbeau, E. (1988). Which method is best? *Mathematics Teacher, 81*, 87–90.

Bell, A. (1976). A study of pupils' proof-explanations in mathematical situations. *Educational Studies in Mathematics, 7*, 23–40.

Bressoud, D. M. (1999). *Proofs and confirmations: The story of the alternating sign matrix conjecture*. Cambridge: Cambridge University Press.

Bressoud, D., & Propp, J. (1999). How the alternating sign matrix conjecture was solved. *Notices of the AMS, 46*(6), 637–646.

Corfield, D. (2003). *Towards a Philosophy of Real Mathematics*. Cambridge: Cambridge University Press.

Dawson, J. W. (2006). Why do mathematicians re-prove theorems? *Philosophia Mathematica, 14*, 269–286.

DeVilliers, M. (1990). The role and function of proof in mathematics. *Pythagoras, 24*, 7–24.

Fischbein, E. (1982). Intuition and proof. *For the Learning of Mathematics, 3*(2), 9–18. 24.

Fischbein, E. (1999). Intuition and schemata in mathematical reasoning. *Educational Studies in Mathematics, 38*(1–3), 11–50.

Hanna, G. (1990). Some pedagogical aspects of proof. *Interchange, 21*(1), 6–13.

Hanna, G. (1997). The ongoing value of proof in mathematics education. *Journal für Mathematik Didaktik, 97*(2/3), 171–185.

Hanna, G. (2000). Proof, explanation and exploration: An overview. *Educational Studies in Mathematics, 44*(1–2), 5–23. Special issue on "Proof in Dynamic Geometry Environments".

Hoyles, C. (1997). The curricular shaping of students' approaches to proof. *For the Learning of Mathematics, 17*(1), 7–16.

Inglis, M., Mejia-Ramos, J. P., & Simpson, A. (2007). Modelling mathematical argumentation: The importance of qualification. *Educational Studies in Mathematics, 66*(1), 3–21.

Jahnke, H. N. (2007). Proofs and hypotheses. *ZDM. The International Journal on Mathematics Education, 39*(1–2), 79–86.

Jones, K., Gutiérrez, A., & Mariotti, M. A. (Eds.) (2000). Proof in dynamic geometry environments. *Educational Studies in Mathematics*, *44*(1–2); Special issue.

Lucast, E. (2003). *Proof as method: A new case for proof in mathematics curricula*. Unpublished masters thesis, Carnegie Mellon University, Pittsburgh, PA.

Maher, C. A., & Martino, A. M. (1996). The development of the idea of mathematical proof: A 5-year case study. *Journal for Research in Mathematics Education, 27*(2), 194–214.

Mariotti, A. (2006). Proof and proving in mathematics education. In A. Gutiérrez & P. Boero (Eds.), *Handbook of research on the psychology of mathematics education: Past, present and future* (pp. 173–204). Rotterdam/Taipei: Sense Publishers.

Moreno-Armella, L., & Sriraman, B. (2005). Structural stability and dynamic geometry: some ideas on situated proof. *International Reviews on Mathematical Education., 37*(3), 130–139.

Pedemonte, B. (2007). How can the relationship between argumentation and proof be analysed? *Educational Studies in Mathematics, 66*(1), 23–41.

Rav, Y. (1999). Why do we prove theorems? *Philosophia Mathematica, 7*(1), 5–41.
Reiss, K., & Renkl, A. (2002). Learning to prove: The idea of heuristic examples. *Zentralblatt für Didaktikt der Mathematik, 34*, 29–35.
Selden, J., & Selden, A. (1995). Unpacking the logic of mathematical statements. *Educational Studies in Mathematics, 29*(2), 123–151.
Sowder, L., & Harel, G. (2003). Case studies of mathematics majors' proof understanding, production, and appreciation. *Canadian Journal of Science, Mathematics and Technology Education, 3*(2), 251–267.
Tall, D. (1998). *The cognitive development of proof: Is mathematical proof for all or for some*. Paper presented at the UCSMP Conference, Chicago.
Yackel, E., & Hanna, G. (2003). Reasoning and proof. In J. Kilpatrick, W. G. Martin & D. Schifter (Eds.), *A research companion to principles and standards for school mathematics* (pp. 227–236). Reston, VA: National Council of Teachers of Mathematics.

Chapter 8
Mathematicians' Individual Criteria for Accepting Theorems and Proofs: An Empirical Approach

Aiso Heinze

8.1 Introduction and Theoretical Framework

Accounting for the acceptance of new mathematical results as part of the "official" body of mathematics is a complex and difficult field of research. Former ideas that there are objective criteria, for example, logical rules, which suffice to decide whether results are correct or incorrect, turned out to be inadequate. In particular, Lakatos (1976) emphasized the importance for acceptance decisions of social processes within the mathematical community. At present, there is a consensus that social processes particularly play an important role in the acceptance of new scientific results, theorems, and proofs. Thirty years ago, Manin already wrote that "a proof becomes a proof after the social act of accepting it as a proof" (Manin 1977, p. 48). If we consider mathematical proofs as thought-experiments, then the question arises whether there are some general objective criteria framing the social process of accepting these experiments (in the sense of demarcationism, Lakatos 1978). Even if such criteria do exist, we may still ask how researchers agree on whether a new result satisfies these objective criteria or not. It is an open question how these social processes work and how they can be described.

The scientific communities in empirical sciences like natural sciences and social science, have developed and accepted some criteria. For example, in natural sciences new results based on physical or chemical experiments have to be replicated independently under the same conditions by another research team. In social sciences, the situation is different; there may be contradictory results from different empirical studies. Usually, after some years a meta-analysis is conducted to identify a tendency in these research studies. A comparatively new test is the so-called mega-analysis; that is a meta-analysis of different meta-analyses (e.g., the mega-analysis on gender effects from Hyde 2005).

A. Heinze
Department of Mathematics Education, Leibniz Institute for Science Education, Kiel, Germany

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives,* DOI 10.1007/978-1-4419-0576-5_8,

But what about the situation in mathematics? Do we have something like replication, meta- or mega-analysis? In the case of computer-assisted mathematical proofs, researchers claim that a proof has to be replicated independently (e.g., Lam 1991). Here, "independently" means that the same result has to be obtained by a different computer program based on a different algorithm. However, what about ordinary mathematical proofs? If proofs are thought-experiments, then we can consider the reading, understanding and accepting of a given proof as a replication (the same experiment under the same conditions by a different researcher). But how many replications do we need for the acceptance of a new result as new mathematical knowledge? Do we have something like a meta-analysis, in the sense that enough mathematicians must have accepted a result?

To approach this problem from another side, we can ask what a mathematician has to do to get a theorem and proof accepted by the mathematical community as new mathematical knowledge. First of all, the result must be published and must be reviewed by other mathematicians. There are several possibilities for publication, such as journals, conference presentations, preprints and so on. Here, the acceptance of a new result rests mainly on activities of the mathematician's peer group – colleagues who are experts in the same research area. Mathematics is divided into a large number of highly specialized research areas, so many theorems and proofs are only interesting (or even understandable) for "some" mathematicians. Thus, acceptance of theorems and proofs mainly takes place in a relatively small peer group. Consequently, results accepted by experts in one specific area are likely to be accepted by the whole mathematics community because the nonexpert mathematicians have to trust their expert colleagues. In some sense, this process is an elitist one (Lakatos 1978). Nevertheless the process of acceptance within a peer group certainly rests on some objective criteria.

8.2 Mathematicians' Criteria for Acceptance of Mathematical Results

Hanna (1983) gave a number of criteria important for assessing results. She states that most mathematicians accept a new theorem when some combination of the following factors is present:

1. They understand the theorem, the concepts embodied in it, its logical antecedents, and its implications. There is nothing to suggest it is not true;
2. The theorem is significant enough to have implications in one or more branches of mathematics (and is thus important and useful enough to warrant detailed study and analysis);
3. The theorem is consistent with the body of accepted mathematical results;
4. The author has an unimpeachable reputation as an expert in the subject matter of the theorem;
5. There is a convincing mathematical argument for it (rigorous or otherwise), of a type they have encountered before. (Hanna 1983, p. 70)

Analyzing the 1980s discussions in the mathematical community about tackling or proving well-known theorems, Neubrand (1989) reorganized Hanna's factors and added a language-factor, which encompasses the communication between the author of a proof and the mathematical community as well as the communication within the mathematical community. Therefore, a proof has to be formulated and presented in appropriate language. Neubrand summarized:

> The process of acceptance of a proof by the community of mathematicians is initiated by the proposal of a convincing argument by an accepted member of the mathematical community, and by a careful check of the argumentation by the experts in that field. But then the existence of some combination of the understanding-, significance-, compatibility-, reputation-, and language-factors is necessary to ensure the final acceptance of the proof (Neubrand 1989, p. 6).

Though these considerations give some ideas about the social processes within the mathematical community's accepting a proof, many questions remain. Can we really compare the cases of major theorems and proofs like the Four-Colour-Theorem, Fermat's Last Theorem and the Poincaré Conjecture with the cases of hundreds of new (minor but important) theorems and proofs every day? Can we speak of "the mathematical community" and consensus within it, or are mathematicians individualists who only believe what they have checked on their own?

The phenomenological approach to mathematical practice seems an appropriate methodology for a careful investigation of these questions. Leng (2002) describes this approach as a study of mathematical practice to ground philosophical claims:

> The phenomenological approach is motivated by the simple claim that any philosopher of mathematics worth her salt should have a clue as to what actually goes on in real mathematical research. (Leng 2002, pp. 5–6).

Leng's (2002) qualitative empirical study is a nice example of using this approach to examine mathematicians' daily practice and behavior. Leng observed mathematical practice in two research seminars led by a famous mathematician at the Fields Institute in Toronto. Her motivation was a deeper investigation of Lakatos' (1976) claim that mathematical development is counterexample-driven. Leng concludes that her study confirmed that dissatisfaction with mathematical results leads to mathematical progress. However, this dissatisfaction is generally not due to counterexamples for given theorems but to the feeling that the existing theorems can be improved.

The observations of the sociologist Bettina Heintz (2000) during her visit at the Max Planck Institute for Mathematics (Bonn, Germany) can also be subsumed under the phenomenological approach. Heintz combined the investigation of mathematical practice in this international research institute with interviews. She explained the strong coherence and consensus in the mathematical discipline as based on an existing consensus to which mathematicians are acculturated by their education. This consensus of action (as a unique mathematical "form of life," in Wittgenstein's sense[1]) historically developed through the strong formalization of mathematical communication by written texts (Heintz 2000).

[1]Heintz refers to §241 in Wittgenstein (1953).

The phenomenological approach seems a promising method for examining the processes of acceptance of theorems (and proofs) by research mathematicians, about which I know of no quantitative empirical data. There are some empirical results concerning undergraduate students' proof schemes (Harel and Sowder 1998). Harel and Sowder pointed out an authoritarian proof scheme; that is, students accept proofs because they originate from an authoritative person or source like a mathematics teacher or a textbook. In an empirical study, Inglis and Mejia-Ramos (2006) investigated whether research mathematicians ($N=74$) and undergraduate students ($N=302$) are influenced by the reputation of the author of a mathematical argument. They chose a heuristic argumentation from a talk by the Fields Medalist Timothy Gowers and asked their participants how much they were persuaded by this argumentation; half of the sample got the information that the author was Gowers, the other half got the argumentation without information about the author. The findings gave clear evidence that both mathematicians and undergraduate students were influenced by an authoritarian proof scheme. There was no significant difference between undergraduate students and mathematicians in their judging of the argumentation, but a clear significant difference between the judgments on an anonymous argumentation versus an argumentation by a Fields Medalist. However, as Inglis and Mejia-Ramos (2006) mention, their participants did not judge a proof but only a heuristic argumentation.

8.3 Research Questions, Sample and Design

Because we lack empirical data concerning mathematicians' behavior and activities in judging and accepting theorems, I conducted an exploratory empirical study. In the study, I distinguished three different situations for accepting new results. I asked a sample of mathematicians to what extent they agree to given criteria when accepting a theorem

(1) in their own research area,
(2) in other research areas in which they are not expert,
(3) when reviewing a research article.

Overall the study addressed the following research questions:

- Which conditions are sufficient for mathematicians to accept a theorem as correct?
- Are there differences between theorems in a mathematician's specific research area and theorems in other mathematical areas?
- Which conditions are sufficient for mathematicians to accept a theorem and proof when reviewing a research paper for a journal?

This study had an explorative character in part because it was not clear at all whether mathematicians would be willing to participate in it. Because I decided this first survey should not be too time-consuming for the participants, the quality of the

1. Wann akzeptieren Sie einen mathematischen Satz, den Sie nicht selber bewiesen haben, als gültig? Bitte unterscheiden Sie hier Sätze aus Ihrem eigenen Spezialgebiet und Sätze aus anderen Gebieten der Mathematik, die Sie für Ihre Arbeit benötigen.

Hinreichendes Kriterium in der täglichen mathematischen Arbeit	eigenes Spezialgebiet				anderes Fachgebiet			
In meiner mathematischen Arbeit nehme ich an, dass ein mathematischer Satz gültig ist, wenn …	(fast) immer	häufig	manchmal	(fast) nie	(fast) immer	häufig	manchmal	(fast) nie
… der Satz in einer referierten Zeitschrift zitiert wird.	○	○	○	○	○	○	○	○
… ich die Schlüsselstellen eines zugehörigen Beweises geprüft habe.	○	○	○	○	○	○	○	○
… andere Mathematiker den Satz verwenden (z.B. Vorträge, Preprints etc.).	○	○	○	○	○	○	○	○
… ich weiß, dass schon lange ein publizierter Beweis vorliegt und es bisher keinen Widerspruch gab.	○	○	○	○	○	○	○	○
… der Satz von Kollegen verwendet wird, die hohe Standards haben.	○	○	○	○	○	○	○	○
… mir die Beweisidee eines zugehörigen Beweises plausibel erscheint.	○	○	○	○	○	○	○	○
… der Satz mit Beweis in einer referierten Zeitschrift erschienen ist (ohne dass ich den Beweis geprüft habe).	○	○	○	○	○	○	○	○
… der Satz mit der Theorie konsistent ist.	○	○	○	○	○	○	○	○
… der Satz von einem berühmten Kollegen stammt.	○	○	○	○	○	○	○	○
… ich einen Beweis im Detail geprüft habe.	○	○	○	○	○	○	○	○

Fig. 8.1 Screenshot with one part of the online-questionnaire

questionnaire (in a test-theoretical sense) was comparatively low. The study could be considered as a first attempt, which could lead to research hypotheses for further research studies.

The sample consists of $N=40$ mathematicians from the Mathematics Departments at the Universities of Augsburg and München in Germany: 15 experienced senior mathematicians (full professors or Privatdozenten – comparable with associate professors) and 25 junior mathematicians (PhD students and postdoctoral fellows).

I collected the data via a short, online questionnaire presented on a webpage on the internet (see Fig. 8.1). In principle, the questionnaire was open for public access for 2 weeks; however, there was no public hyperlink to it. The mathematicians in the sample got the hyperlink by email. The online questionnaire asked them to rate given statements on a classical four-point Likert scale with the stages "(almost) always – frequently – sometimes – (almost) never" (A translation of the questionnaire items is presented in the Appendix). For this explorative study, I did not plan to group items to scales.

8.4 Results

The following presents descriptive results from the survey. Regarding the acceptance of mathematical theorems in their own research area the answers of the senior and junior mathematicians are depicted as mean values in a bar chart in Fig. 8.2.

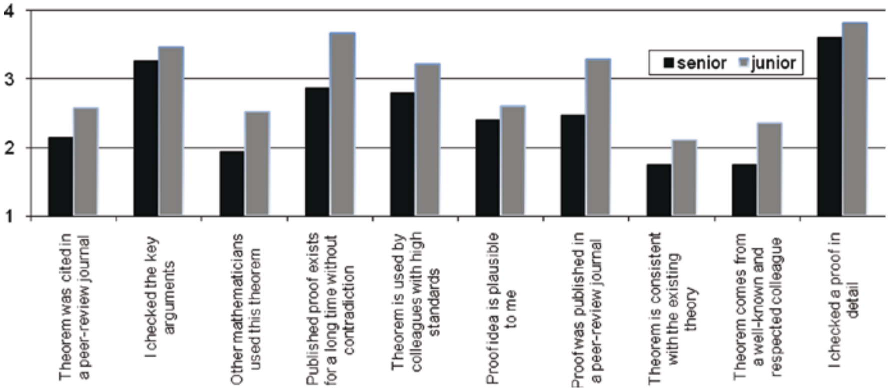


Fig. 8.2 Criteria of junior and senior mathematicians for the acceptance of new theorems in their own research area (mean values of the Likert scale)

The Likert scale is represented by 4 = (almost) always, 3 = frequently, 2 = sometimes, 1 = (almost) never.

Analyzing the data showed that the main points for the mathematicians in the sample were that

- they checked a proof (in detail or key arguments) by themselves; or
- they are sure that other mathematicians with high standards checked the result; or
- they can assume that many other mathematicians checked the result, because it has existed as a proof for a long time and no contradiction has been found.

All these criteria got a mean value greater than 3, which means at least "frequently" on the Likert scale. The peer-review processes of journals also have a certain reputation; there was obviously some trust in this kind of self-monitoring of the mathematical community. However, it is interesting that senior mathematicians in particular frequently did not automatically accept reviewed proofs as correct. In general, the junior mathematicians were much more liberal in the process of acceptance, though (or because) they are less experienced. Even for this small sample, there are significant differences ($p<0.05$) for three items ("published for a long time," "peer-review journal," "from a well-known and respected colleague"). Maybe sheer experience makes senior mathematicians more careful when studying new theorems and proofs. The other factors, like consistency within the theory, proofs from well-known and respected colleagues, plausible proof ideas, and applications of the theorem by other mathematicians, played a minor role for both senior and junior mathematicians as criteria for the acceptance of results.

In the situation of mathematical theorems and proofs that do not belong to a mathematician's specific research area, I obtained similar results (see Fig. 8.3; again the Likert scale is represented by 4 = (almost) always, 3 = frequently, 2 = sometimes, 1 = (almost) never).

Overall, in only one case did a significant difference ($p<0.05$) occur: in the role of key arguments when checking a new result. Apparently, just checking key arguments is more acceptable in one's own research area than in unfamiliar research areas.

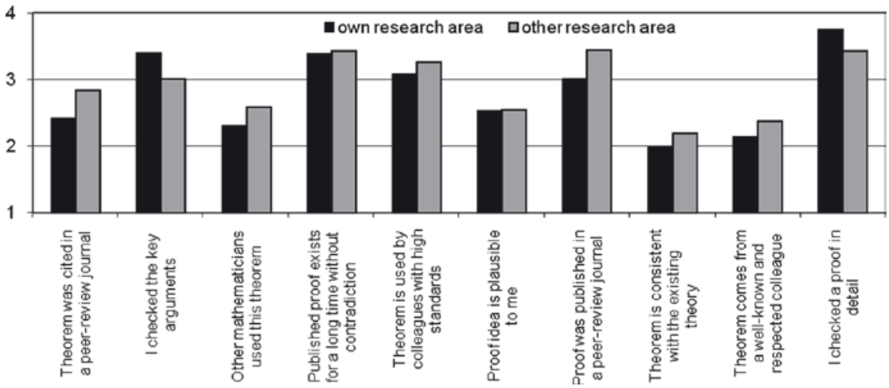


Fig. 8.3 Criteria of mathematicians for the acceptance of new theorems in their own and in other research areas (mean values of the Likert scale)

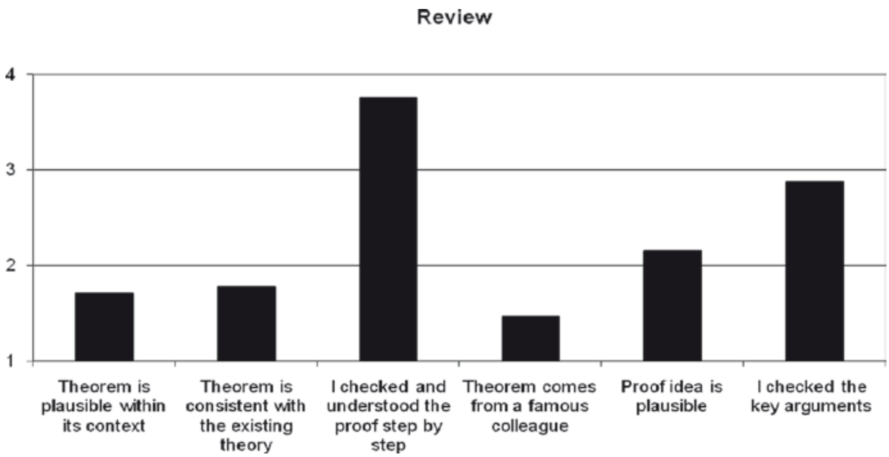


Fig. 8.4 Criteria of mathematicians for the acceptance of new theorems when reviewing a research paper (mean values of the Likert scale)

A tendency towards a difference could also be observed for the role of peer review journals. It is more likely that mathematicians accept reviewed proofs in unfamiliar research areas than in their own research area; however, due to the small sample the difference was not significant.

Results about the criteria for accepting theorem and proof in a review process for a peer-review journal are given in Fig. 8.4. (Again, the Likert scale is represented by 4 = (almost) always, 3 = frequently, 2 = sometimes, 1 = (almost) never).

Here again, an important point for the mathematicians in this sample was that they checked a proof themselves (either in detail or the key arguments). Other factors, like consistency within the theory, proofs from well-known and respected colleagues, plausible proof ideas and applications of the theorem by other mathematicians, played only a minor role. Overall, there were only small, insignificant differences between junior and senior mathematicians.

8.5 Discussion

In a letter to the editor of Scientific American, William Thurston wrote in 1994: “Mathematical truth and reliability come about through the very human process of people thinking clearly and sharing ideas, criticizing one another and independently checking things out” (Thurston 1994). Observing the empirical data presented above leads me to stress particularly the last words “independently checking things out.” My empirical findings indicate a tendency for mathematicians to accept a proof mainly because it was checked by themselves, produced by colleagues with high standards or published a long time ago and had not since been contradicted (i.e., one can assume that it was checked several times). Peer-review journals played a certain role in acceptance, but senior mathematicians in particular seemed to be skeptical about them. Generally, each mathematician apparently has to check proofs individually in order to accept the results. This fits with the contents of a long email one senior mathematician sent me. In particular, he wrote:

> In principle, I must be able to prove each theorem I use. That’s what I tell my students: an authoritarian proof is valid in theology but not in mathematics. They are responsible for everything they write (even if they quote something from a book or a well-known mathematician). Each mathematician must rebuild the mathematics he uses for himself.

Remarkably, checking new results individually also plays an important role for results that do not belong to the mathematician’s specific research area. One can extrapolate that all mathematicians are to a certain extent individualists who construct their own body of individually-accepted mathematics and only trust their colleagues in exceptional cases: not surprising, in view of the different functions of a proof. One of the main aims in reading a proof is to understand why the proof is correct and the theorem is true (e.g., de Villiers 1990; Hanna and Jahnke 1996). However, this apparent individualistic character of mathematicians raises questions about whether a social process of accepting new theorems and proofs really takes place or can be described.

The empirical findings presented here are preliminary and explorative; further research with better-developed instruments and better-quality data is necessary to explore how mathematicians accept new results. Empirical data collected anonymously by questionnaires may not suffice, since the problem of social desirability cannot be controlled effectively. Particularly for mathematics, this might be an issue; because there are strong norms about mathematical practice (cf. Heintz 2000, as outlined in Sect. 2). In other words, in self-reported descriptions of their everyday practice mathematicians may present an image of themselves as scientists who are very critical and strict when checking proofs, but in their real daily practice they may behave differently.

Presently, we lack empirical data concerning mathematicians’ views and social processes in the mathematical community. The present results indicate that surprises may be in store, as future empirical studies lead to more insight into the question of when a proof really becomes a proof.

Appendix: Questionnaire (Translation)

On the Acceptance of Mathematical Theorems and Proofs

The public image of mathematics includes the belief that mathematics is a thoroughly exact and formalistic science. Mathematicians seem to be people who do everything quite formally. In reality, however, this perception is only partially true. With this questionnaire, I would like to ask you how you – as a mathematician – really work in your everyday mathematical research.

Completing the following questionnaire will take you only a few minutes. Anonymity is assured.

1. When do you accept a mathematical theorem of which you did not find proof by yourself to be true? Please distinguish between theorems from your own research area and theorems from other research areas that you use during your everyday mathematical work**.**

	Own research area				Other research areas			
Sufficient condition for accepting a theorem. *During my everyday mathematical work I accept a theorem to be true, if…*	frequently ☐	sometimes ☐	(almost) never ☐	(almost) always ☐	frequently ☐	sometimes ☐	(almost) never ☐	(almost) always ☐
… the theorem was cited in a peer review journal.	☐	☐	☐	☐	☐	☐	☐	☐
… I checked the key arguments of the proof.	☐	☐	☐	☐	☐	☐	☐	☐
… other mathematicians used this theorem (talks, preprints etc.)	☐	☐	☐	☐	☐	☐	☐	☐
… I know that there exists a published proof for a long time and there has no contradiction been found yet.	☐	☐	☐	☐	☐	☐	☐	☐
… the theorem is used by colleagues with high standards.	☐	☐	☐	☐	☐	☐	☐	☐
… the proof idea of a proof is plausible to me.	☐	☐	☐	☐	☐	☐	☐	☐
… the theorem including the proof was published in a peer-review journal (but I did not check the proof).	☐	☐	☐	☐	☐	☐	☐	☐
… the theorem is consistent with the existing theory.	☐	☐	☐	☐	☐	☐	☐	☐
… the theorem comes from a well-known and respected colleague.	☐	☐	☐	☐	☐	☐	☐	☐
… I checked a proof in detail.	☐	☐	☐	☐	☐	☐	☐	☐

2. Assume that you are asked to review a paper for a professional journal. Clearly, not only the relevance of the given results for the particular area of research is of interest, but also the correctness of these results. However, a detailed analysis of the proofs is time-consuming in general.

   When do you accept a theorem to be true in a reviewing process?

Sufficient condition for accepting a theorem in a reviewing process.	(almost) always	frequently	sometimes	(almost) never
Reviewing an article I accept a theorem to be true, if…	☐	☐	☐	☐
… the statement of the theorem is plausible in the context of the article.	☐	☐	☐	☐
… the theorem is consistent with the existing theory.	☐	☐	☐	☐
… I checked the proof step by step and understood it.	☐	☐	☐	☐
… the theorem comes from a well-known and respected colleague.	☐	☐	☐	☐
… the proof idea of the proof is plausible to me.	☐	☐	☐	☐
… I checked the key arguments of the proof.	☐	☐	☐	☐

3. Please give us some data about you and your research interest:

3.1 I am…

☐ Professor
☐ Priv.-Doz. / Dr. habil. (*comparable to associate professor*)
☐ Doktor (*PhD*)
☐ Doktorand (*PhD-Student*)

3.2 To which branch of mathematics (such as calculus, algebra, geometry etc.) would you assign your research area?

Do you have remarks or comments in this context?

Thank you for your participation!

References

De Villiers, M. D. (1990). The role and function of proof in mathematics. *Pythagoras, 24*, S17–24.

Hanna, G. (1983). *Rigorous proof in mathematics education*. Toronto: OISE Press.

Hanna, G., & Jahnke, H. N. (1996). Proof and proving. In A. Bishop, K. Clements, C. Keitel, J. Kilpatrick & C. Laborde (Eds.), *International Handbook of Mathematics Education* (pp. 877–908). Dordrecht: S. Kluwer.

Harel, G., & Sowder, L. (1998). Students' proof schemes: results from exploratory studies. In A. H. Schoenfeld, J. Kaput & E. Dubinsky (Eds.), *Research in Collegiate Mathematics Education,* (pp. 234–283). Providence, RI: S. American Mathematical Society.

Heintz, B. (2000). *Die Innenwelt der Mathematik. Zur Kultur und Praxis einer beweisenden Disziplin [The inner world of mathematics. Culture and practice of a proving discipline].* Wien, New York: Springer.

Hyde, J. S. (2005). The gender similarities hypothesis. *American Psychologist, 60*(6), 581–592.

Inglis, M., & Mejia-Ramos, J. P. (2006). Is it ever appropriate to judge an argument by its author? In D. Hewitt (Ed.), *Proceedings of the British Society for Research into Learning Mathematics 26(2).* UK: University of Bristol.

Lakatos, I. (1976). *Proofs and Refutations*. Cambridge: Cambridge University Press.

Lakatos, I. (1978). *Mathematics, Science and Epistomology*. In J. Worrall, & G. Currie (Eds.) Philosophical Papers, Vol. 2. Cambridge: Cambridge University Press.

Lam, C. W. H. (1991). The search for a finite projective plane of order 10. *American Mathematical Monthly, 98*, S305–318.

Leng, M. (2002). Phenomenology and mathematical practice. *Philosophia Mathematica, 10*(3), 3–25.

Manin, Y. I. (1977). *A course in Mathematical Logic*. New York, Heiderlberg, Berlin: Springer.

Neubrand, M. (1989). Remarks on the acceptance of proofs: the case of some recently tackled major theorems. *For the Learning of Mathematics, 9*, 2–6.

Thurston, W. P. (1994). Letter to the Editors. *Scientific American, 270*(1), 5.

Wittgenstein, L. (1953). *Philosophical investigations*. In G. E. M. Anscombe, & R. Rhees (Eds.) Oxford: Blackwell.

Part II
Proof and Cognitive Development

Chapter 9
Bridging Knowing and Proving in Mathematics: A Didactical Perspective

Nicolas Balacheff

To Andrien Douady

9.1 An Ad Hoc Epistemology for a Didactical Gap

9.1.1 The Didactical Gap

More often than not, the problem of teaching mathematical proof has been addressed almost independently from the teaching of mathematical "content" itself. Some curricula have exposed learners to a significant amount of mathematics without learning about mathematical proof as such (Herbst 2002, p. 288); others teaching mathematical proof as a subject in itself without significantly relating it to concrete practical examples (cf. Usiskin 2007, p. 75). The most common didactical tradition chooses to introduce proof in the context of geometry – usually at the turn of the eighth grade – while completely ignoring it in algebra or arithmetic, where things seem to be reduced to "mere" computations. This orientation has changed slightly in the past decade with an increasing emphasis on the teaching of proof. However, an implicit distinction between form and content has lead to references to teaching "mathematical reasoning" (e.g., NCTM standards) or "deductive reasoning" (e.g., French national programs) instead of mathematical proof as such which would have moved "form" much more to the forefront of the didactical scene.

Nevertheless, it is generally acknowledged that mathematical proof has specific characteristics, among them a formal type of text (the US vocabulary often refers to "formal proof"), a specific organization and an undisputable robustness once syntactically correct. These characteristics have given mathematics the reputation of having exceptionally stringent practices as compared to other disciplines, practices that are not socially determined but inherent to the nature of mathematics itself.

Hence, the answer to the question: "*Can one learn mathematics without learning what a mathematical proof is and how to build one?*" is "*No*." But now one can observe a *double* didactical gap: (i) mathematical proof creates a rupture between

N. Balacheff (✉)
CNRS, Laboratoire d'informatique de Grenoble, CNRS/UJF/Grenoble-INP, France
e-mail: Nicolas.BaLacheff@imag.fr

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives,* DOI 10.1007/978-1-4419-0576-5_9,

mathematics and other disciplines (even the "exact sciences") and (ii) a divide in the course of mathematical teaching during the (almost) standard first 12 years of education (into an era before the teaching of proof and one after).

The origin of these gaps lies at the crosspoint of several lines of tension: rigor versus meaning, internal development versus application-oriented development of mathematics, ideal objects defined and manipulated by symbolic representations versus experience-based empirical evidence. I do not analyse these tensions here; I mention them to evoke the complexity of the epistemological and didactical problems which confront us.

One source of the didactical problems is that teaching must take into account the learners' initial understanding and competence: *We can teach only to ones who know…* The learners' existing knowledge often proves resistant, especially because the learners may have proven its efficiency, as in the case of their argumentative skills. In order to overcome this difficulty, teachers organize situations, *mises en scène* and discourses in order to "convince" or "persuade" learners (in the vocabulary of Harel and Sowder 1998). Argumentation seems the best means to this end. It works both as a tool for teaching and as a tool for doing mathematics for a long while. But then learners suddenly face an unexpected revelation[1]: *In mathematics you don't argue, you prove…*

Looking to bridge this transition, mathematics educators have searched for ideas in psychology. In the middle of the twentieth century, the success of Piaget's "stage theory" of development suggested that proof could be taught only after the required level of development had been reached[2]. As a result, mathematical proof was introduced suddenly in curricula (if at all) in the ninth grade – generally, the year that students have their 13th birthday. However, this strategy has not worked so well, suggesting to some that Piaget may have been wrong.

Some mathematics educators then turned to psychologies of discourse and learning, feeling that the followers of Piaget had not paid enough attention to language and social interaction. Some suggested the ideas of Vygotsky and the socio-constructivists could have provided a solution (e.g. Forman et al. 1996). However, this line of thought did not appear to be the panacea either. Then Lakatos' work seemed to suggest that a solution might be found in the epistemology of mathematics itself

[1] Argumentation means here "verbal, social and rational activity aimed at convincing a reasonable critic of the acceptability of a standpoint by putting forward a constellation of one or more propositions to justify this standpoint" (van Eemeren *et al.*, 2002, p.xii). "In argumentative discussion there is, by definition, an explicit or implicit appeal to reasonableness, but in practice the argumentation can, in all kinds of respects, be lacking of reasonableness. Certain moves can be made in the discussion that are not really helpful to resolving the difference of opinion concerned. Before a well-considered judgment can be given as to the quality of an argumentative discussion, a careful analysis as to be carried out that reveals those aspects of the discourse that are pertinent to making such a judgment concerning it reasonableness." (ibid., p.4)

[2] See e.g. Piaget J. (1969) p. 239: "L'enfant n'est guère capable, avant 10-11 ans, de raisonnement formel, c'est-à-dire de déduction portant sur des données simplement assumées, et non pas sur de vérités observées." More precisely, For more, c.f. Piaget J. (1967) Le jugement et le raisonnement chez l'enfant. Delachaux et Niestlé.

(e.g. Reichel 2002); however, such attempts also failed amid skepticism from mathematicians and researchers.

The responsibility for all these failures does not belong to the theories which supposedly underlie the educational designs, but to naive or simplifying readers who have assumed that concepts and models from psychology can be freely transferred to education. In particular, they rarely take into account the nature of mathematics as content (while often emphasizing the nature of the perceived practice of mathematicians).

My objective here is then to question the constraints mathematics imposes on teaching and learning, postulating that, as for any other domain, learning and understanding mathematics cannot be separated from understanding its intrinsic means for validation: *mathematical proof*. First, I address the epistemology of proof, on which we could base our efforts to manage or bridge the didactical gap discussed above.

9.1.2 The Need to Revisit the Epistemology of Proof

Although apparently a bit simplistic, it may be good to start from the recognition that mathematical ideas are not a matter of feeling, opinion or belief. They are of the order of "knowing" in the Popperian sense[3], by virtue of their very specific relation to proof (and proving). They provide tools to address concrete, materialistic or social problems, but they are not about the "real" world. To some extent, mathematical ideas are about mathematical ideas; they exist in a closed "world" difficult to accept but difficult to escape. For this reason, mathematical ideas do not exist as plain facts but as statements which are accepted only once they have been proved explicitly; before that, they cannot be[4] instrumental either within mathematics or for any application.

However, despite this emphasis on the key role of proof in mathematics, it must be remembered that at stake is not *truth* but the *validity of a statement within a well-defined theoretical context* (cf. Habermas 1999). For example, Euclidean geometry is no truer than Riemannian geometry. This shift from the vocabulary of truth to the vocabulary of validity, which suggests a shift from *proof* to *validation,* is more important than we may have realized. Validation refers to constructing reasons to accept a specific statement, within an accepted framework shaped by accepted rules and other previously accepted statements. From this perspective, mathematical validation searches for an *absolute proof in an explicit context*; it can thus claim certainty as a foundational principle.

This view of validity and proof is antiauthoritarian (Hanna and Jahnke 1996, p. 891), insofar as it assumes a common agreement about a collective and well-

---

[3]Popper (1959) proposed falsification as the the empirical criterion of demarcation of knowledge, scientific theories or models.

[4]Or should not be...

understood effort. It thus fits the classical conception of what a scientific proof should be, since such a proof must clearly not depend on specific individual or social interests. Proving is an example of an intellectual enterprise that allows a minority to overcome the opinion of an established majority, according to shared rules. This is related to an ancient meaning of the word "demonstration" in English (e.g., Herbst 2002, p. 287).

So the concept of *proof* is not a stand-alone concept; it goes with the concepts of "validity of a *statement*" and "*theory*." This has been well explained and illustrated by the Italian school, especially Alessandra Mariotti (1997). However, the word "theory" is the most difficult for learners. No such thing is available to learners a priori, and to understand what the word means seems out of reach. Nevertheless, learners have ideas about mathematics and about mathematical facts. They also have experience in arguing about the "truth" of a claim or the "falsity" of a statement they reject; but this is experience in argumentation in contexts that are not framed by a theory in scientific terms. To construct a proof requires an essential shift in the learner's epistemological position: passing from a practical position (ruled by a kind of logic of practice) to a theoretical position (ruled by the intrinsic specificity of a theory).

In addition, we cannot engage in the validation of "anything" that has not been first expressed in a language. This principle applies across disciplines (Habermas 1999), but plays a special role in mathematics, where the access to "mathematical objects" depends in the first place on their semiotic availability (Duval 1995).

In other words, the teaching and learning of mathematical proof requires mastery of the relationships among knowing, representing and proving mathematically.

9.2 A Model to Bridge Knowing and Proving

9.2.1 Short Story 1: Fabien and Isabelle Misunderstandings

Consider the following problem[5]:

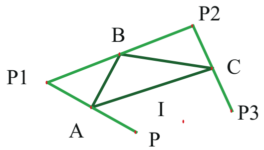
Construct a triangle ABC. Construct a point P and its symmetrical point P1 about A. Construct the symmetrical point P2 of P about B, construct the symmetrical point P3 of P about C. Move P. What can be said about the figure when P3 and P are coincident? Construct the point I, the midpoint of [PP3]. What can be said about the point I when P is moved? Explain.

Constructing the diagram (Fig. 9.1) with dynamic geometry software,[6] one can easily notice that the point I does not move when one manipulates the point P. This *fact* seems surprising; the crux of the situation is to propose an explanation.

[5] From Capponi (1995), *Cabri-classe*, sheet 4–10.

[6] E.g. Cabri-geometry (here used for the drawing), or Geometer Sketchpad; or Geogebra or one of the several others now available sometimes open access.

Consider the following problem[5]:



Construct a triangle ABC. Construct a point P and its symmetrical point P1 about A. Construct the symmetrical point P2 of P about B, construct the symmetrical point P3 of P about C. Move P. What can be said about the figure when P3 and P are coincident? Construct the point I, the midpoint of [PP3]. What can be said about the point I when P is moved? Explain .

Fig. 9.1 Short story 1 problem

Let us examine the interaction between a tutor, Isabelle, and a student, Fabien, about this problem.[7] Fabien has observed the fact but he has no insight about the reason: "*The point I does not move, but so what...*" However, he noticed and proved that ABCI is a parallelogram. At this stage, from the point of view of geometry (and of the tutor), the reason I stands immobile while P moves should be obvious. The tutor then provides Fabien with several hints but with no results. After a while she desperately insists: *"The others, they do not move. You see what I mean? Then how could you define the point I, finally, without using the points P, P1, P2, P3?"* Throughout the interaction, the tutor is moved by one concern which can be summarized by the question: *"Don't you see what I see?"* But Fabien does not see the "obvious"; it is only when she tells him the mathematical reasons for the immobility of I that the tutor provokes a genuine "Aha!" effect...

In order to explain the immobility of I, the teacher had to get the student to construct a link between a *mechanical world* – that of the interface of the software[8] – and a *theoretical world* – the world of geometry. Only this link can turn the observed *fact* (the immobility of I) into a *phenomenon* (the invariance of I). But the construction of this link is not straightforward; it is a *process of modeling.*

Teacher and student did share representations, words, and arguments so that they could communicate and collaborate; however, this did not guarantee that they shared understanding. Educators have made considerable efforts to develop representations that could make the nature and the properties of mathematical concepts more tangible. But these remain just representations with no visible referent; manipulating them and sharing factual experience does not guarantee shared meaning. Nevertheless, they are the only means of communication, since in mathematics the referent, in a semiotic sense, is itself a representation (i.e., a tangible entity produced on purpose).

[7] A more detailed analysis can be found in Balacheff and Soury-Lavergne (1995), Sutherland and Balacheff (1999).

[8] Another student's search for an explanation illustrates well what is meant here by mechanical world: "*So... I have said... But is not very clear... That when, for example, we put P to the left, then P3 compensates to the right. If it goes up, then the other goes down...*" (Sébatien, [prot. 78–84]).

In the next section, I will explore this issue of representation and its relation with the learners' building of meaning, and then take up the challenge of defining "knowing" in a way that may not solve the old epistemological problem but will provide some grounds to build a link between knowing and proving.

9.2.2 Trust, Doubt and Representations

The fascination for proof without words[9], which would give access to the very meaning of the validity of a mathematical statement without the burden of sophisticated and complicated discourses, is a symptom of the expectations mathematics educators have attached to the use of nonverbal representations in mathematics teaching. The development of multimedia software, advanced graphical interfaces and access to "direct manipulation" of the represented "mathematical objects" has even strengthened these expectations. The above story of the Fabien and his tutor's misunderstandings is initial evidence that things might be slightly more difficult. I will explore this difficulty now, starting with an example coming from professional mathematics.

In 1979, Benoit Mandelbrot noticed in a picture produced by a computer and a printer that the Mandelbrot set[10] – as it is now known, following a suggestion of Adrien Douady – was not connected. "A striking fact, which I think is new" Mandelbrot[11] remarked. John Hubbard, a former PhD student of Adrien Douady's who became his well known collaborator, reported that:

> Mandelbrot had sent [them] a copy of his paper, in which he announced the appearance of islands off the mainland of the Mandelbrot set M. Incidentally, these islands were in fact not there in the published paper: apparently the printer had taken them for dirt on the originals and erased them. (At that time, a printer was a human being, not a machine). Mandelbrot had penciled them in, more or less randomly, in the copy [they] had. (Hubbard 2000 pp. 3–4)

This anecdote reflects two things: first, the efficiency and strength of the computer-based picture in supporting a conjecture; second, the fragility of this same picture, which depends on both the algorithmic and technical conditions of its production. Then, Hubbard reported:

> One afternoon, Douady and I had been looking at this picture, and wondering what happened to the image of the critical point by a high iterate of the polynomial $z^0 + c$ as c takes

[9]See Claudi Alsina and Roger B. Nelsen (2006), *Math Made Visual: Creating Images for Understanding Mathematics*, published by MAA, and a good example in Roger B. Nelsen (1993), *Proofs without words: exercises in visual thinking*, published by MAA. See Hanna (2000, esp. pp.15–18) for an analysis.

[10]Considering the sequence of complex numbers $z_{n+1} = z_n^2 + c$, the Mandelbrot set (or set M) is obtained by fixing $z_0=0$ and varying the complex parameter c.

[11]Quotation from p.250 of Mendelbrot (1980) Fractal aspects of the iteration of $z \to \lambda z(1\text{-}z)$ for complex λ and z. Annals of the New York Academy of Sciences. 357 (1) 249 - 259

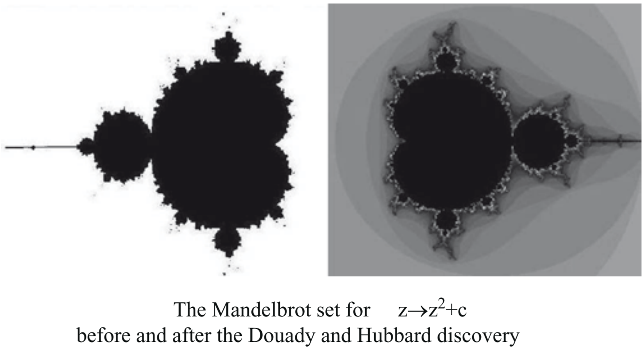


Fig. 9.2 The Mandelbrot set for $z \to z^2+c$ before and after the Douady and Hubbard discovery

> a walk around an island. This was difficult to imagine, and we had started to suspect that there should be filaments of M connecting the islands to the mainland. (ibid.)

Soon, Adrien Douady realized that this meant that the set M is connected[12], but "the proof of this fact is by no means obvious," he remarked (Douady 1986, p. 162). The proof followed after a long process of writing, initiated by a *Note aux Comptes-rendus* in 1982. After the discovery of the connectedness, images of the set M got transformed, offering a more beautiful picture full of colors which, so to speak, "displayed" the connectivity of M (Fig. 9.2).

This case supports the idea of complex relations between representation and mathematical objects – or, more precisely, the role of representations as mediators for the conceptualisation of mathematical objects. It invites more caution in considering evidence in a nonverbal representation. Not to say that nonverbal representations or expressions of an argument are of no value; rather, I emphasize that the frequent claim in education that, *"A picture is worth a thousand words"* has limits and cannot be accepted without further examination.

For example, graphic calculators are widely used by students. They provide students with efficient tools for calculus, blending graphical and symbolic representations. The use of this technology has led to new problem-solving strategies that take advantage of the low cost of exploring of graphical representations. Among them is what Joel Hillel (1993, p. 29) called "window shopping," which consists of playing with the various possibilities offered by the display. The diagrams (Fig. 9.3) reproduce two appearances of the graph of the same function, $f(x)=x^4-5x^2+x+4$. As one can "see," these pictures can induce different conjectures about, for example, the numbers of zeros of the polynomial or its behavior within the interval [−2, +2]

It is now common for teachers to warn students and teach them strategies to ensure reliable, optimal use of their calculators. Still, the problem of knowing how

[12] Régine Douady remembers that Adrien had been quickly convinced of the connectivity of M, thanks to the theoretical argument which convinced him in an astonishingly "simple" way. However, to complete the explicit proof took some time (*2008, personal communication*).

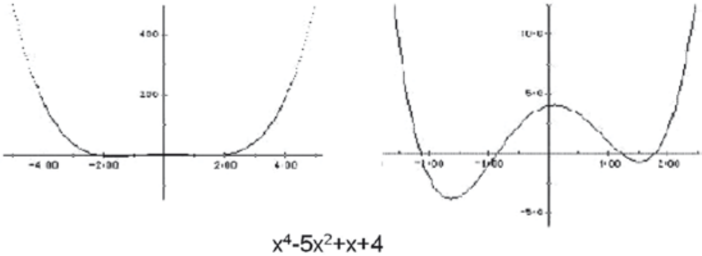


Fig. 9.3 Two representations for one function, an example of window shopping (Hillel 1993, p.29)

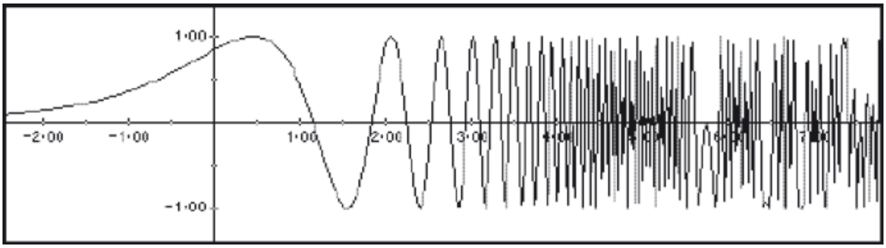


Fig. 9.4 A stroboscopic effect

to balance trust and doubt when using these machines and looking for conjectures has no straightforward answer. Part of achieving this balance depends not only on how learners critically organize their explorations but also on the reliability of the embedded software. Consider the case of the function $g(x) = \sin(e^x)$. Most students are prepared to study this function without a priori foreseeing difficulties; that is, until their machine displays something like Fig. 9.4.

"Window shopping" will not help to answer the questions this display raises. An algebraic study will just leave students with a question they probably cannot solve with their knowledge of mathematics and computer science. This picture results from the interference between the computation of the coordinates of each point to be displayed and the choice of which pixel to turn black on the screen. In the end, it is the product of a kind of stroboscopic effect, as suggested by Adrien Douady[13]. Producing a "correct" figure would be a matter of first mathematically notating both the capabilities and the limitations of the drawing instrument and then using sophisticated computational strategies to decide on the intervals at which to plot an "informative" graph.

The problem of how students can decide to trust or doubt mathematical representations goes beyond graphical representations to include any representa-

[13] Personal communication

tion. A last example, taken from Luc Trouche work (2003) on computer algebra systems demonstrates this. Consider the equation [Ln($e^x - 1$) = x]: One can use a pocket graphical calculator to solve it algebraically or to graph it; the two pictures below (Fig. 9.5) (from Trouche 2003, p. 27) show the respective results.

The results speak for themselves. The optimal treatment leading to a solution – in this case, that this equation has no solution – consists of a formal transformation of the algebraic expression, producing [$e^x - 1 = e^x$].

The difficulty students may have relates not to their lack of mathematical knowledge but to a general human inclination not to question their knowledge and their environment unless there is a tangible contradiction between what is expected after a given action and what is obtained, as my final example will demonstrate.

In this case, upper secondary students were asked to tell what is the limit at + ∞ of the function [f(x)=ln(x)+10sin(x)]. Without a graphic calculator, only five percent of the students answered wrongly; with a graphic calculator, which displayed the window reproduced below (Fig. 9.6), this number grew to 25% (Guin and Trouch 2001, p. 65).

Given such cases of error, teachers and mathematics educators might have to consider whether graphic calculators contribute positively to mathematics learning or whether students have difficulty shifting from one semiotic context to another. (Other examples of common errors include: the value of π is exactly 3.14, or a

Fig. 9.5 A case where a graphic calculator misleads the user (Trouche 2003, p.27)

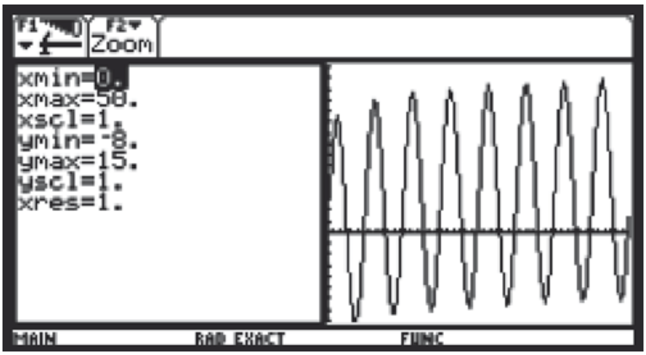


Fig. 9.6 Ploting the function [f(x) = ln(x) + 10 sin (x)]

convergent series reaches its limit, or the Fibonacci series $U_0 = 1$, $U_1 = (1 + \sqrt{5})/2$, $U_n = U_{n-1} + U_{n-2}$ is divergent). Most such errors, or "misconceptions" to use the 1980s term, are probably symptomatic of the students' knowledge, which can be legitimate in certain contexts although possibly wrong mathematically. To analyse this issue further, we must have a conceptualization of the students' knowledge which (i) allows us to make sense of it from a mathematical perspective; (ii) is relevant from a cognitive perspective; and (iii) opens the possibility of didactical solutions.

9.2.3 A Phenomenological Definition of Knowing

Studying students' productions that were mathematically incorrect, the mathematics educators of the 1980s usually chose to use the word "misconception." As noted by Jere Confrey (1990), such student errors should be first considered as indications of what they know. Comfrey used the generic word "conception" to refer to the rationale of students' answers to a given problem or question. I postulate that such conceptions result from the learner's interactions with the environment, and that learning is both a process and an outcome of the learner's adaptation to this environment. By "environment," I refer to a physical setting, a social context or even a symbolic system (especially now that the latter can be depicted by a technology which dynamically materializes it).

However, only some characteristics of the environment are relevant from the point of view of learning. Educators do not deal with the learner in all his or her social, emotional, physiological and psychological complexity, but from a knowledge perspective: as *the epistemic subject*. The same principle applies to the environment, which we restrict to *the milieu* defined as *the subject's antagonist system* in the learning process (Brousseau 1997, p. 57); that is, we only consider those features of the environment that are relevant from the epistemic perspective. This means that our characterizations of the (epistemic) subject and of the milieu are interdependent systemically (and dynamically, since both will evolve during the learning process).

Pragmatically, the only accessible evidences of a conception are behaviors and their outcomes. The educator's problem is to interpret this evidence as an indicator of adaptive strategies, and demonstrate the student's conception in a model (Brousseau 1997, p. 215)[14]. Below, I propose a formalization that will provide such a model. Below, I propose a formalization that will provide such a model. Recognizing this interdependence, expressed by Noss and Hoyles[15] (1996, p. 122) as *situated abstraction*, accepts that people could demonstrate different and possibly contradictory conceptions depending on circumstances, although knowledgeable observers may ascribe them to the same source concept.

[14]For the convenience of the English-speaking reader, I take all the references to Brousseau's contributions to mathematics education from Kluwer, 1997 but Brousseau's work was primarily published between 1970 and 1990.

[15]This proposition should be understood in the light of the development of the "situated learning paradigm" of Jeane Lave and Etienne Wenger, whose work was published in the early 1990s.

Thus, a conception is attached neither to the subject nor to the milieu, but exists as *a property of the interaction between the subject and the milieu* – its antagonist system (Brousseau 1997, p. 57). The objective of this interaction is to maintain the viability of the subject/milieu system (or [S↔M] system) by returning it to a safe equilibrium after some perturbation (i.e., the tangible materialization of a problem). This implies that the subject recognizes the perturbation (e.g., a contradiction or uncertainty) and that the milieu has features which make the perturbation tangible (since otherwise, the milieu may "absorb" or "tolerate" errors or dysfunctions) (Fig. 9.7).

From this definition of *conception*, I can derive a definition of *knowing* as the characterization of a dynamic set of conceptions. This definition has the advantage of being in line with our usual use of the word "knowing" while providing grounds to understand the possible contradictions evidenced by learners' behaviors and their variable mathematical development. A conception is a situated knowing; in other words, it is the instantiation of a knowing in a specific situation detailed by the properties of the milieu and the constraints on the relations (action/feedback) between this milieu and the subject.

This definition of conception provides a starting point but still has to be refined in order to make it relevant to our research. To do so, I will now introduce the model cK¢[16], in order to provide an effective tool to concretely represent and analyze the corpus of data obtained from the observation of students' activities. This model aims to establish a necessary bridge between knowing and proving by providing a

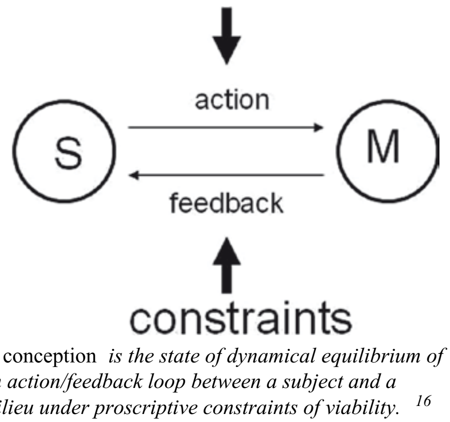


Fig. 9.7 A conception is the state of dynamical equilibrium of an action/feedback loop between a subject and a milieu under proscriptive constraints of viability. These constraints do not address how the equilibrium is recovered but the criteria of this equilibrium. Following Stewart (1994, pp. 25–26), I argue that these constraints are proscriptive – they express necessary conditions to ensure the system's viability – and not prescriptive, since they do not tell in detail how equilibrium must be reconstructed.

[16]The letters cK¢ stand for : "conception," "knowing" and "concept"; more about this model is presented and discussed on [http://ckc.imag.fr]

more balanced role to control structures with respect to the role usually assigned to actions and representations.

9.2.4 A Model to Bridge Knowing and Proving: cK¢

That validation plays a key role in the emergence of "knowing" has been established at least since Popper proposed the criterion of falsification and Piaget introduced the process of cognitive disequilibrium. This principle is also inherent in a "conception" as we define it, adding the explicit condition that a conception is not self-contradictory.

"Proving" is the most visible part of the intellectual activity related to validation. However, as the Italian school has clearly demonstrated (Boero et al. 1996a), proving cannot be separated from the on-going controlling activity involved in solving a problem or achieving a task. To some extent, "proving" can be seen as an ultimate achievement of controlling and validating. No one can claim to know without a commitment to and a responsibility for the validity of the claimed knowledge. In return, this knowledge functions as a means to establish the validity of a decision in the course of performing a task and even in the process of building new knowledge – especially in the learning process. In this sense, knowing and proving are tightly related. Hence, *a conception is validation dependent*: In other words, we can diagnose the existence of a conception because there is an observable domain in which "it works," in which there are means to validate it and to challenge possible falsifications. This is the essence of Vergnaud's (1981, p. 220) statement that problems are the sources and criteria of concepts.

Vergnaud demonstrated that we could characterize students' conceptions with three components: problems, representation systems and invariant operators (1991, p. 145)[17]. I take this model as a starting point, with the addition of the related control structure.

Then, I can characterize a conception by a quadruplet (P, R, L, Σ) in which:

– P is a set of problems,

  This set corresponds to the class of the disequilibria the considered subject/milieu [S↔M] system can recognize; in mathematical terms: P is the set of problems which can be solved – in pragmatic terms, P is the conception's *sphere of practice*.

– R is a set of operators,
– L is a representation system,

  R and L describe the feedback loop relating the subject and the milieu, namely the actions, feedbacks and outcomes.

– Σ is a control structure,

  The control structure describes the components that support the monitoring of the equilibrium of the [S↔M] system. This structure ensures the conception's coherence; it includes

[17]Vergnaud in fact proposed this definition at the beginning of the 1980s.

the tools needed to take decisions, make choices, and express judgement on the use of an operator or on the state of a problem (i.e., solved or not).

This model aims at accounting for the [S↔M] system and is not restricted to one of its components[18]. The representation system allows the formulation and the manipulation of the operators by the active subject as well as by the reactive milieu. The control structure allows expression and discussion of the subject's means for deciding the adequacy and validity of his or her action as well as the milieu's criteria for selecting a feedback. This symmetry allows us both to take the subject's perspective when evaluating his or her knowing and the milieu's perspective when designing the best conditions to stimulate and support learning. Moreover, it gives us a framework in which to describe, analyze and understand the didactical complexity of learning proof by taking into account the interrelated relevant dimensions: the subject, the milieu and the problem.

In the next section I will give an illustration of this distinctive role of the control structure and the light it sheds on the learners' behaviors we observe and aim at understanding. I will then summarize the proposed framework discussing the relations we must establish between action, formulation and validation in order to understand the didactical complexity of learning and teaching mathematical proof. These three dimensions provide the means we need to build a bridge between knowing and proving.

9.3 Proving From a Learning Perspective

9.3.1 Short Story 2: Vincent and Ludovic Mismatch

Vincent and Ludovic are two middle school students who had no specific difficulties with mathematics. They volunteered to participate in an experiment that Bettina Pedemonte (2002) was carrying out to study the cognitive unity between problem solving and proof. The problem was the following:

Construct a circle with AB as a diameter. Split AB in two equal parts, AC and CB. Then construct the two circles of diameter AC and CB… and so on (Fig. 9.8).

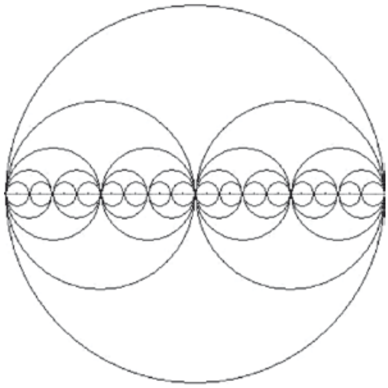
How does the perimeter vary at each stage?

How does the area vary?

With no hesitation, the two students expressed – with the formulas they knew well – the perimeter and the area of the first steps in the series of drawings. Their letters represent quantities and the formulas are another description of the reality the drawing factually displays. The students conjectured that the perimeter will be constant and that the area decreases to zero. But Vincent noticed that "*the area is*

[18]By extension, one can often refer to students' conceptions as acceptable given that one can account precisely for the circumstances, which are the milieu and the constraints within which [S↔M] functioned.

Construct a circle with AB as a diameter. Split AB in two equal parts, AC and CB. Then construct the two circles of diameter AC and CB… and so on.



How does the perimeter vary at each stage?
How does the area vary?

Fig. 9.8 Short story 2 problem

always divided by 2…so, at the limit? The limit is a line, the segment from which we started …." The discussion then continued:

41. *Vincent*: It falls in the segment… the circle are so small.
42. *Ludovic*: Hmm… but it is always 2πr.
43. *Vincent*: Yes, but when the area tends to 0 it will be almost equal…
44. *Ludovic*: No, I don't think so.
45. *Vincent*: If the area tends to 0, then the perimeter also… I don't know…
46. *Ludovic*: I will finish writing the proof.

Although Vincent and Ludovic collaborate well and seem to share the mathematics involved, the types of control they have on their problem-solving activity differ. Ludovic is working in the algebraic setting (c.f., Douady 1985); the control is provided by his ensuring the correctness of the symbolic manipulation and his knowledge of elementary algebra. Vincent is working in a symbolic-arithmetic setting; the control comes from a constant confrontation between what the formula "tells" and what is displayed in the drawings. Both students understood the initial situation in the "same" way, both syntactically manipulated the symbolic representations (i.e., the formulas of the perimeter and of the area), but their controls on what they performed were different, revealing that the conceptions they mobilized were also significantly different. I deduce that the operators they manipulated (algebraic writings, sketching diagrams, etc.), although they coincided from the behavioral perspective, were semantically different. Moreover, from this evidence, an observer could argue that the students were not addressing the same "problem"; Vincent was "baffled" by the gap between what he saw and what he computed, while Ludovic was "blind" to this gap. (Actually, Ludovic's knowledge of calculus would not have been sufficient to provide any relevant explanation).

The symbolic representation plays the role of a semiotic mediator between the two students' different conceptions. It allows communication between the students and is instrumental for each in controlling the problem-solving process and building a proof. We know that two different representations may demonstrate two different understandings; however, here one given representation also supports different understandings and hence different proofs.

9.3.2 The Complex Nature of Proof

Many theorists have attempted to answer the question of what counts as a proof, from either an epistemological or an educational point of view. However, there is no single, final answer. The Vincent and Ludovic discussion above confirms that sheer formal computation is not enough. As in one of the best previous anecdotes in the history of mathematics[19], Vincent could well say to Ludovic: *I see it, but I don't believe it*. As several authors have emphasized, a proof should be able to fulfill the need for an explanation; however the explanatory nature of a proof may become the object of an even more irreconcilable disagreement than was its rigor. Consider the simple mathematical statement: The sum of two even numbers is itself even. Figure 9.9 provide a sample of proofs of this statement. A discussion of these proofs by mathematicians, mathematics teachers and learners provokes very different responses from each.

The arguments in such a discussion involve three types of critical considerations: the search for certainty, the search for understanding and the requirements for a

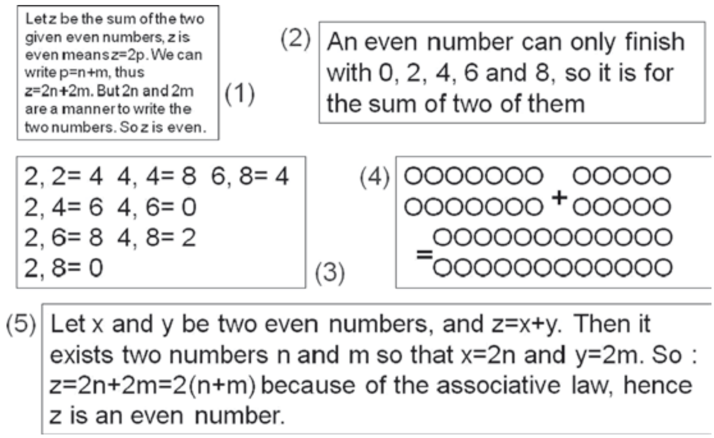


Fig. 9.9 Example adapted from Healy and Hoyles (2000, p. 400)

[19] "*Je le vois, mais je ne le crois pas*," wrote Cantor to Dedekind, in 1877, after having proved that for any integer n, there exists a bijection between the points on the unit line segment and all of the points in an n-dimensional space.

successful communication. The complex nature of proof lies in the fact that any effort to improve a candidate-proof on one of these dimensions may change its value on the other two. There is no clear standard to decide on the correct balance. Restricting the evaluation to the "certainty" side is playing safe, as this side is compulsory for the transformation of mathematical ideas. However, such reductionism is not viable from a learning perspective, especially when students are first introduced to mathematical proof; their control structures are not appropriately evolved. Educators at this point need to give academic status to activities that may not lead to what would be a proof for professional mathematicians but that still make sense as mathematical activities. Hence, my proposal to structure the relations between explanation, proof and mathematical proof as I did to ground my own work (Balacheff 1988). This structure distinguished between pragmatic and intellectual proof, and within both it identified categories related first to the nature of the student's knowing and his or her available means of representation.

The rationale for this organization (sketched below in Fig. 9.10) is the postulate that the explaining power of a text (or nontextual "discourse") is directly related to the quality and density of its roots in the learner's (or even mathematician's) knowing. What is produced first is an *"explanation" of the validity of a statement from the subject's own perspective*. This text can achieve the *status of proof if it gets enough support from a community* that accepts and values it as such. Finally, it can be claimed as *mathematical proof if it meets the current standards of mathematical practice*. So, the keystone of a *problématique* of proof in mathematics (and possibly any field) is the nature of the relation between the subject's knowing and what is involved in the "proof."

This recognition of a proof's roots in knowing may justify a statement as strong as Harel and Sowder's that "one's proof scheme is idiosyncratic and may vary from field to field, and even within mathematics itself," (1998, p. 275). However, this view misses the social dimension of proof, which transcends an entirely subjective feeling of understanding (as well as "ascertaining" or "persuading"; Harel and Sowder, ibid., p. 242). From a didactical perspective, the issue is not psychological but epistemological, being directly related to the role a proof plays in building links between a theory that provides its framework and means and a statement that it aims to validate. The transcendence of a proof, proposed by Habermas (1999) as a

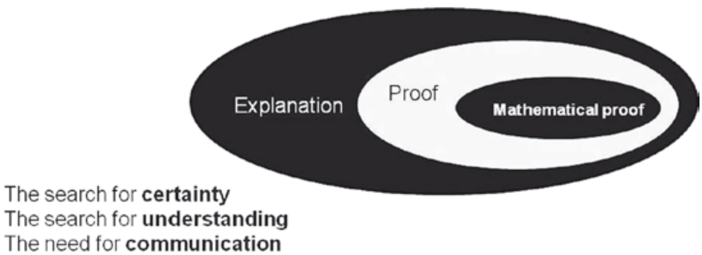


Fig. 9.10 From its producer perspective what comes first is an "explanation" of the validity of a statement, reaching the status of proof and of mathematical proof require specific processes either social or syntactical. The explanatory character of the proof may be lost in this process which balance the constraints of certitude, understanding and communication

requirement for a *problématique* of truth and justification, is a dimension too often forgotten in favor of a psychological or sociological analysis of proving. This transcendence is not a dogmatic but a pragmatic position which allows the construction of knowledge as a collective asset which can be shared and be sustainable without depending on its author(s) and circumstance(s) of birth.

The technicalities of mathematical proof are then essential, and can be accepted as the price for a viable construction of mathematics. In this respect, formal rigor is a weapon against the biases that "idiosyncratic proof schemes" may produce.

9.3.3 Knowing and Proving in the Didactical Genesis of Proof

Learning mathematics starts with the first years of schooling, at least from an institutional point of view. As is well documented, learners at this elementary level depend as much on their experience as on the teacher as a reference to distinguish between their opinions, their beliefs and their actual knowledge. The criterion for assessing this difference rests either in the tangible efficiency of the knowledge at stake or in ad hoc validation by the teacher. But the teacher has to rely on knowledge, demonstrating that authority is not the ultimate reference. Hence, efficiency and tangible evidence are the supports for the validity of a statement: It's true because we verify that it works. Mathematical learners are first of all practical persons; to enter mathematics they have to change their intellectual posture and become a theoretician. This shift can easily be seen in the passage from practical geometry (the geometry of drawings and shapes) to theoretical geometry (the deductive or axiomatic geometry), or from symbolic arithmetic (computation of quantities using letters) to algebra. A learner making the transition from the practical to the theoretical has to face the epistemological difficulty of a transition from knowing in action to knowing in discourse: The origin of knowing is in action but the achievement of mathematical proof is in language (see Fig. 9.12).

Again, the tight relationship among action, formulation (semiotic system) and validation (control structure) imposes itself (Brousseau 1997). This trilogy which defines a conception (Fig. 9.11), also shapes didactical situations[20]; there is no validation possible if a claim has not been explicitly expressed and shared; and there is no representation without a semantic which emerges from the activity (i.e., from the interaction of the learner with the mathematical milieu).

Indeed, this passage from mathematics as a tool whose rationale is "transparent," to mathematics as a theoretically-grounded means for the production and evaluation of explicit validation has a key stepping stone: language; as a *symbolic technology* (Bishop 1991, p. 82), not just a means for social interaction and communication. Language allows learners to understand and appropriate the value of mathematical proof compared with the pragmatic proof they were used to. Now, this language

[20] figure 9.11 sketches the interactions between these three poles

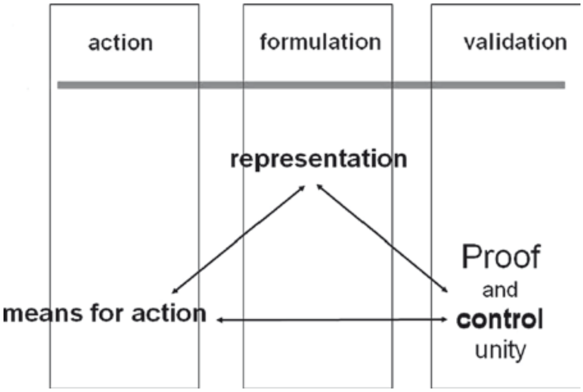


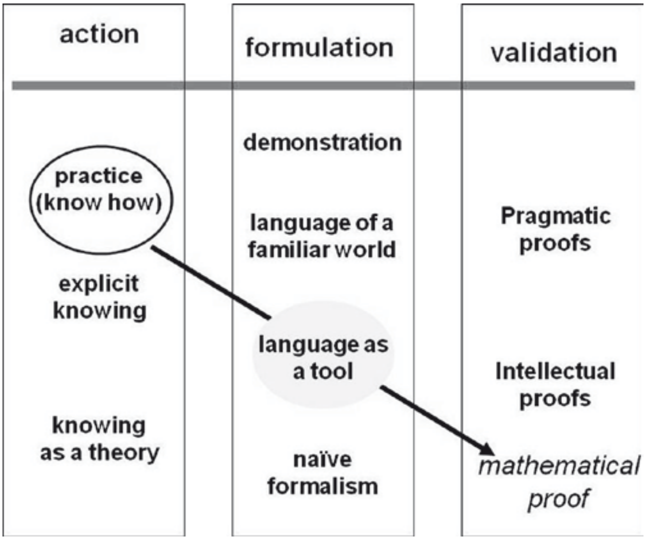
Fig. 9.11 The three interelated and interacting dimensions of mathematical knowledge

could be of lower levels than the naïve formalism mathematicians use; the level of language will bind the level of the proof learners can produce and/or understand. However, there is room for genuine mathematical activity at all these levels, provided that the learners have moved beyond empiricism and have seen the added value of the theoretical posture (see Fig. 9.12).

9.4 Still an Open Problem: The Situations…

After a few decades, researchers have now reached a consensus on the variety of meanings that proof may have for learners (if not for teachers). Several classifications and analyses of the complexity of the different aspects of mathematical proof have been extensively reported. Although they still express significant differences (Balacheff 2008), researchers have converged on considering mathematical proof as a core issue in the challenge of learning and teaching mathematics; mathematical knowing and proving cannot be separated. In other words an educational *problématique* of proof cannot be separated from that of constructing mathematical knowledge.

This challenge is well understood from an epistemological perspective. However, it is far from clear from a didactical perspective. A lot of effort has gone into proposing problems and mathematical activities which could facilitate the learning of mathematical proof. At the turn of the twentieth century, computer science and human–computer interaction research have made so much progress that it is possible to provide learners and teachers with environments able to provide much more mathematically relevant feedback on users' activities. Especially, dynamic geometry environments and computer algebra systems allow learners to experience conjecturing and refuting in a manner never available before, hence giving them access to a dialectic necessary to ground the learning of mathematical proof.



This figure illustrates the approximate mapping between the critical categories in each of the three dimensions (action, formulation and validation). It requires teachers to provide students with the means to switch from a pragmatic approach of truth to a theoretical approach of validity based on mathematical proof. Realising that language as a tool is a critical milestone on this move.

Fig. 9.12 This figure illustrates the approximate mapping between the critical categories in each of the three dimensions (action, formulation and validation). It requires teachers to provide students with the means to switch from a pragmatic approach of truth to a theoretical approach of validity based on mathematical proof. Realising that language as a tool is a critical milestone on this move

However, there is some evidence that learners can remain in a pragmatic intellectual posture, not catching the value of mathematical proof.

Prompting the ultimate move from pragmatic to theoretic knowing requires designing situations so that the pragmatic posture is no longer safe or economical for the learners, while the theoretical posture demonstrates all its advantages. The resultant social and situational challenges are levers which one can use to modify the nature of the learners' commitment to proving. Such design is possible if solving a problem is no longer the main issue and fades away behind the issue of being "sure" of the validity of the solution. We already have some examples which witness the possibility of designing such situations (e.g., Bartolini-Bussi 1996, Boero et al.1996b, Arsac and Mantes 1997, etc.). The scientific challenge is now to better understand the didactical characteristics of these situations and to propose a reliable model for their design, for the sake of both researchers and teachers.

References

Arsac, G., & Mantes, M. (1997). Situations d'initiation au raisonnement déductif. *Educational Studies in Mathematics, 33*, 21–43.

Balacheff, N. (1988). Une étude des processus de preuve en mathématique chez des éléves de Collège (Vols. 1 & 2). Thèse d'état. Grenoble: Université Joseph Fourier.

Balacheff, N. (2008). The role of the researcher's epistemology in mathematics education: an essay on the case of proof. *ZDM Mathematics Education, 40*, 501–512.

Balacheff, N., & Soury-Lavergne, S. (1995). Analyse du rôle de l'enseignant dans une situation de préceptorat à distance: TéléCabri. In R. Noirfalise, M.-J. Perrin-Glorian (Eds.) Actes de la VII Ecole d'été de didactique des mathématiques (pp. 47–56). Clermont-Ferrand: IREM de Clermont-Ferrand.

Bartolini Bussi, M. G. (1996). Mathematical discussion and perspective drawing in primary school. *Educational Studies in Mathematics, 31*(1/2), 11–41.

Bishop, A. (1991). Mathematical culture and the child. In A. Bishop (Ed.) *Mathematical enculturation: a cultural perspective on mathematics education* (pp. 82–91). Berlin: Springer.

Boero, P., Garuti, R., Lemut, E., & Mariotti, M. A. (1996a). *Challenging the traditional school approach to theorems: a hypothesis about the cognitive unity of theorems*. Valencia, Spain: PME XX.

Boero, P., Garuti, R., & Mariotti, M. A. (1996b). *Some dynamic mental processes underlying producing and proving conjectures*. Valencia, Spain: PME XX.

Brousseau, G. (1997). *Theory of didactical situations in mathematics*. Dordrecht: Kluwer.

Confrey, J. (1990). A review of the research on students conceptions in mathematics, science, and programming. In C. Courtney (Ed.) Review of research in education. *American Educational Research Association 16*, 3–56.

Douady, A. (1986). Julia sets and the Mandelbrot set. In H.-O. Peitgen & P. H. Richter (Eds.), *The beauty of fractals: images of complex dynamical systems* (pp. 161–173). Berlin: Springer.

Douady, R. (1985). The interplay between different settings: Tool object dialectic in the extension of mathematical ability. In L. Streefland (Ed.), *Proceedings of the IX International Conference for the Psychology of Mathematics Education* (pp. 33–52). Holland: Noodwijkerhout.

Duval, R. (1995). *Sémiosis et pensée humaine*. Berne: Peter Lang.

van Eemeren, F. H., Grootendorst, R., & Snoeck Henkemans, F. (2002). *Argumentation: analysis, evaluation, presentation*. Mahwah, NJ: Lawrence Erlbaum Associates.

Guin, D., & Trouche, L. (2001). Analyser l'usage didactique d'un EIAH en mathématiques: une tâche nécessairement complexe. *Sciences et Techniques Educatives, 8*(1/2), 61–74.

Habermas, J. (1999). Wahrheit und rechtfertigung. Frankfurt: Suhrkamp (French translation: Vérité et justification. Gallimard, Paris, 2001).

Hanna, G., & Jahnke, N. (1996). Proof and proving. In A. Bishop, et al. (Eds.), *International handbook of mathematics education* (pp. 877–908). Dordrecht: Kluwer.

Hanna, G. (2000). Proof, explanation and exploration: an overview. *Educational Studies in Mathematics, 44*, 5–23.

Harel, G., & Sowder, L. (1998). Students' proof schemes: results from exploratory studies. In A. Schonfeld, J. Kaput, & E. Dubinsky (Eds.) *Research in collegiate mathematics education III*. (Issues in Mathematics Education, Vol. 7, pp. 234–282). Providence, RI: American Mathematical Society.

Healy, L., & Hoyles, C. (2000). A study of proof conceptions in algebra. *Journal for Research in Mathematics Education., 31*(4), 396–428.

Herbst, P. (2002). Establishing a custom of proving in american school geometry: evolution of the two-column proof in the early twentieth century. *Educational Studies in Mathematics, 49*(3), 283–312.

Hillel, J. (1993). Computer algebra systems as cognitive technologies: implication for the practice of mathematics education. In C. Keitel & K. Ruthven (Eds.), *Learning through computers: mathematics and educational technology* (pp. 18–47). Berlin: Springer.

Hubbard, J. (2000). Preface to Tan Lei (Ed.) The Mandelbrot set, theme and variations (pp 1–8). London Mathematical Society Lecture Note Series, 274. Cambridge, MA: Cambridge University Press.

Mariotti, M. A. (1997). Justifying and proving in geometry: the mediation of a microworld. Revised and extended version of the version published in M. Hejny, & J. Novotna (Eds.) *Proceedings of the European Conference on Mathematical Education* (pp. 21–26). Prague: Prometheus Publishing House.

Forman, E. A., Mimick, N., & Stone, A. (1996). *Contexts for learning: sociocultural dynamics in children's development*. Oxford: Oxford University Press.

Noss, R., & Celia Hoyles, C. (1996). *Windows on mathematical meanings: learning cultures and computers*. Berlin: Springer.

Pedemonte, B. (2002). Etude didactique et cognitive des rapports de l'argumentation et de la démonstration dans l'apprentissage des mathématiques. PhD Thesis, Grenoble: Université Joseph Fourier.

Popper, K. (1959). *The logic of scientific discovery*. London: Routledge.

Reichel, H.-C. (2002). Lakatos and aspects of mathematical education. In G. Kampis, L. Kvasz & M. Stöltzner (Eds.), *Appraising Lakatos: mathematics, methodology, and the man* (pp. 255–260). Berlin: Springer.

Stewart, J. (1994). Un système cognitif sans neurones: les capacités d'adaptation, d'apprentissage et de mémoire du système immunitaire. *Intellectika, 18*, 15–43.

Sutherland, R., & Balacheff, N. (1999). Didactical complexity of computational environments for the learning of mathematics. *International Journal of Computers for Mathematical Learning, 4*, 1–26.

Trouche, L. (2003). *Construction et conduite des instruments dans les apprentissages mathématiques: nécessité des orchestrations. Mémoire d'habilitation à diriger des recherches*. Paris: Université de Paris VII.

Usiskin, Z. (2007). What should Not Be in the algebra and geometry curricula of average college-bound students? *Mathematics Teacher, 100*, 68–77.

Vergnaud, G. (1981). Quelques orientations théoriques et méthodologiques des recherches françaises en didactique des mathématiques. *Recherches en didactique des mathématiques, 2*(2), 215–231.

Vergnaud, G. (1991). La théorie des champs conceptuels. *Recherches en didactique des mathématiques, 10*(2/3), 133–169.

Chapter 10
The Long-Term Cognitive Development of Reasoning and Proof

David Tall and Juan Pablo Mejia-Ramos

10.1 Introduction

In recent years, a framework of cognitive development from child to mathematician has been developed in the Mathematics Education Research Centre at the University of Warwick, based on the work of Eddie Gray, David Tall, and their research students (Tall 2006). A paper indicative of the collaborative nature of this effort is presented by Tall et al. (2001) under the title *Symbols and the bifurcation between procedural and conceptual thinking*; the authors address the broader question of why some students succeed in mathematics, yet others fail, based on research studies carried out for doctoral dissertations in mathematics education at the University of Warwick. These papers may be found via the website davidtall.com.

In this presentation we focus specifically on the transition from school mathematics to the formal theory of mathematics as published in journals, crucially taking into account the concepts that undergraduate students have met before their introduction to the mathematics as it is practiced by mathematicians. Technically, a *met-before* is part of the individual's concept image in the form of a mental construct that an individual uses at a given time based on experiences they have met before. Human beings bring their previous experiences to bear on new situations that they meet. As they grow more sophisticated, this prior knowledge is compressed into *thinkable concepts* that, connected together in *knowledge schemas,* frame the way in which individuals think. In particular, proof develops initially through practical experiment and then through thought experiment drawing implications from given starting points, through symbolic manipulation of arithmetic and algebraic formulae, and only at a later stage through set-theoretic definition and formal proof.

D. Tall (✉)
Institute of Education, University of Warwick, Coventry, CV4 7AL, UK
e-mail: davidtall@mac.com

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives,* DOI 10.1007/978-1-4419-0576-5_10,

10.2 Theoretical Framework

The child is born with a genetic structure set-before birth in the genes, but the generic facilities of perception and action need to be coordinated and refined into coherent perceptions of the world and integrated action schemas such as see-grasp-suck. Mathematical procedures are extensions of these natural propensities that may be learnt in a basic procedural sense but are usually better appreciated within a more coherent meaningful framework of connections.

In the final chapter of *Advanced Mathematical Thinking*, Tall (1991) reflected on the nature of mathematical proof and theorized that there were two different sources of meaning prior to the introduction of formal definition and proof. One focused on objects and their properties, classified into categories and leading to a van Hiele type development of increasing sophistication, building from primitive perception, to more refined conceptions, descriptions, then definitions used for making inferences, building a coherent deductive framework characteristic of Euclidean Geometry. In the initial stages of perception and description, properties occur at the same time, a triangle with three equal sides also has three equal angles. Proof begins to arise in this development at the level of definition and deduction where an equilateral triangle defined as having three equal sides, as a consequence, also has three equal angles.

The other source of meaning builds through the compression of a repeatable action as an overall process that can be performed without effort, which enables students to learn procedures to perform routine mathematical algorithms. Some students develop a flexible use of symbolism that can operate both as processes to *do* mathematics and concepts to *think about* it. Gray and Tall (1994) introduced the term *procept* to refer to the dual use of symbolism as process and concept in which a process (such as counting) is compressed into a concept (such as number), and symbols such as $3+2$, ¾, $3a+2b$, $f(x)$, dy/dx operate dually as computable processes and thinkable concept. Here proof develops through generalized arithmetic and algebraic manipulation.

In subsequent years, this framework has been developed into what Tall (2006) described as three mental worlds of mathematics:

- the *conceptual-embodied* (based on perception of and reflection on properties of objects);
- the *proceptual-symbolic* that grows out of the embodied world through actions (such as counting) and symbolization into thinkable concepts such as number, developing symbols that function both as processes to *do* and concepts to *think about* (called procepts);
- the *axiomatic-formal* (based on formal definitions and proof) which reverses the sequence of construction of meaning from definitions based on known concepts to formal concepts based on set-theoretic definitions.

The term "world of mathematics" is used here with special meaning. It has often been suggested to us that these should be simply considered as different "modes of thinking," in particular these ideas may easily be reformulated in what the French school refer to as different "registers," such as verbal, spoken, written, graphic, symbolic, formal, etc. (Duval 2006), or as different representations in American College Calculus such as verbal, numeric, algebraic, graphic, analytic.

The choice of the word "world" is used here deliberately to represent not a single register or group of registers, but the *development* of distinct ways of thinking that grow more sophisticated as individuals develop new conceptions and compress them into more subtle thinkable concepts. The focus on long-term development involves making new links and suppressing earlier aspects which are no longer relevant to develop an increasingly sophisticated world of mental thought, rather than a cross-sectional study of the use of different registers or representations to focus on different aspects of a particular problem situation.

The conceptual embodied world includes not only perceptions of physical objects, but also (later on) visuo-spatial reasoning using internal conceptions built from external perceptions. It grows from the immediate perception and action of the young child to the focus of attention on aspects such as the idea that a point has location but not size, that a line has no thickness and can be extended as far as desired. In this way the focus of attention moves from the specifics of human perception to the subtle essence of underlying regularities that grow into Platonic conceptions that some experts may see as a separate and ideal world. Others, however, see this greater level of sophistication as a natural product of human mental construction focusing on essentials and suppressing detail that is no longer central to the growing sophisticated thought processes.

The symbolic world grows in quite a different way, encapsulating counting as number, addition as sum, repeated addition as product, sharing as fractions, generalized arithmetic processes as algebraic expressions, infinite approximating sequences as limit. This development is described with its growth and discontinuities in Tall et al. (2001). It relates to the process-object compression that Dubinsky (1991) calls "encapsulation" following Piaget and Sfard (1991) terms "reification" within her framework in which operational mathematics is recast in a structural form.

The axiomatic formal world develops from the properties arising in embodiment and symbolism, now formulated in terms of set theoretic definitions of mathematical structures with all other properties derived using mathematical proof (Tall 1991, 2002).

There is a concern that each of the terms used here is employed with different meanings in the literature. For instance, Lakoff (1987) says that *all* thought is "embodied," Peirce (1932) and Saussure (1916) use the term "symbolic" in a wider sense than this, Hilbert (1900/2000) and Piaget (Piaget and Inhelder 1958) use the term "formal" in different ways – Hilbert in terms of formal mathematical theory, Piaget in terms of the "formal" operational stage when teenagers begin to think in logical ways about situations that are not physically present.

It is for this reason that the two-word names are introduced as "conceptual-embodied" referring to the embodiment of abstract concepts as familiar images (as in "Mother Theresa is the embodiment of Christian charity"), "proceptual-symbolic" referring to the particular symbols that are dually processes (such as counting, or evaluation) and concepts (such as number and algebraic expression), "axiomatic-formal" to refer to Hilbert's notion of formal axiomatic systems.

However (and this is a simple but important compression of knowledge), when these terms are used in a context where their meaning is clear, they will be shortened to *embodied*, *symbolic* and *formal*. This will allow the worlds to operate in tandem, such as the embodied-symbolic combination which can operate in both directions, for instance, representing algebraic equations as graphs or projective geometry as homogeneous coordinates. Later the embodied and symbolic worlds may underpin formal thinking as embodied formalism or symbolic formalism or even an integrated combination of all three.

Although there is a hierarchy in the order in which these worlds begin to develop, each new world develops concurrently with older worlds (see Fig. 10.1). As school students enter the symbolic world, their ways of thinking in the embodied world continue to develop, just as mathematics university students continue to operate in the embodied and the symbolic worlds as they begin to develop more formal ways of thinking. Similarly, professional mathematicians have a variety of working methods; some performing embodied thought experiments to suggest theorems which may then be published in purely formal terms, others basing their mathematical proofs explicitly on powerful computations and symbol manipulations.

10.3 Different Types of Reasoning and Proof

Each world carries with it aspects that are more than simply ways of thinking, they also involve ways of perception, action and reflection and the emotions and meanings that accompany that thinking. Tall (2004a, b) suggests that each world of mathematics carries with it different kinds of warrants for truth that grow in sophistication as the individual matures.

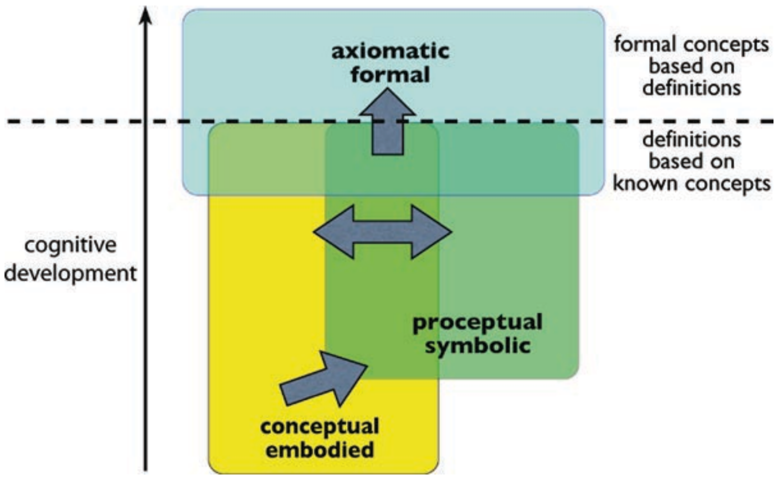


Fig. 10.1 The cognitive growth of three mental worlds of mathematics

For instance:

- In the embodied world, the individual begins with physical experiments to find how things fit together, for example, squares fit together to form a pattern that covers a flat table, so that four corners make a complete turn, and two corners make a straight line. Later verbal descriptions become definitions and are used in Euclidean geometry both to support the visual constructions with verbal proofs and to build a global theory from definitions and proof.
- In the (proceptual) symbolic world, arguments begin with specific numerical calculations and develop into the proof of algebraic identities such as $(a - b)(a + b) = a^2 - b^2$ by symbolic manipulation.
- In the formal world, the desired form of proof is by formal deduction, such as the intermediate value theorem proved by using the completeness axiom.

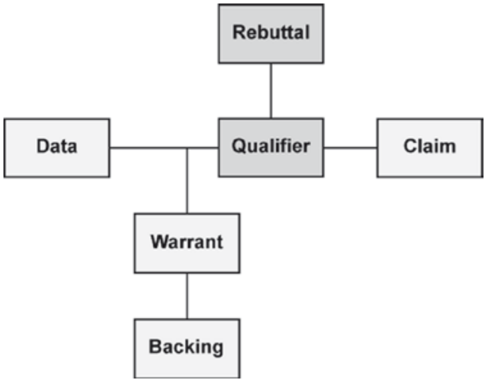
In this way we see that the categorization into three worlds *each of which develops in sophistication* is not simply a question of three different modes of thinking, but of different strands of long-term development that complement and extend each other.

10.4 Degrees of Confidence in Proof

In addition to these different kinds of justification, there is also considerable variation in the level of confidence that students and mathematicians have in the conclusion of a given mathematical argument. Proof in mathematics requires that each statement must be true or false with no middle ground. But this is only the tip of the iceberg: as a proof is constructed, arguments may be used at various times with varying levels of confidence. Toulmin (1958) put forward a perspective of argumentation that takes into account the kind of arguments that may be used in proof building, and he introduced a layout for modeling a general argument that differentiates six main types of statements. Starting from a *claim* that one wishes to support with given *data*, some kind of reason is produced to link the data and the claim. This linking statement is called the *warrant* of the argument, which may be supported by some kind of *backing*. Most importantly, a *qualifier* may be used to express the strength with which the claim may be taken, and a *rebuttal* may be used to state the possible limitations in the scope of the argument (Fig. 10.2).

Although Toulmin (1958) did not address the modeling of mathematical argumentation and proof, in a later work Toulmin et al. (1979) suggested that this layout could indeed be useful to model the procedure of proving in mathematics, and illustrated this in the context of Euclidean geometry (p. 89). Furthermore, Toulmin's layout has been used in mathematics education to analyse the collective argumentation of students and teachers in the mathematics classroom (Krummheuer 1995; Forman et al. 1998; Yackel 2001; Stephan and Rasmussen 2002; Rasmussen et al. 2004; Knipping 2003), students' written and verbalized arguments in task-based interviews (Hoyles and Küchemann 2002; Evens and Houssart 2004; Weber and Alcock 2005; Alcock and Weber 2005; Pedemonte 2007; Inglis et al. 2007), and by

Fig. 10.2 Toulmin's layout



philosophers of mathematics to analyse mathematical proofs (Alcolea Banegas 1998; Aberdein 2005, 2006a, b).

The following example from Inglis and Mejia-Ramos (2008), illustrates how a student uses a non-absolutely-qualified embodied argument to gain insight into a possible proof. Linvoy is a second year maths undergraduate in a top ranked U.K. university. In an interview, he was asked to work on the following task (based on a problem by Raman 2002):

> Determine whether the next statement is true or false (explain your answer by proving or disproving the statement): *The derivative of a differentiable even function is odd.*

After working unsuccessfully for a couple of minutes with the definitions of even/odd function and that of the derivative of a function, Linvoy said:

> "Perhaps if I think of it in a bit of a less formal way, if I just think of it as the derivative of a function being the gradient at a particular point… and… um… [draws the graph of an even sinusoidal function] I think of some graph like this which happens to be [inaudible] because it's an even function, and then… yes, I suppose one way of looking at this is that at any point here, like say you take this point [picks a point of the function in the first quadrant], you've got this gradient going like that, if you compare the exact other part, you've got the gradient going in the opposite direction because it's exactly, ummm, it's like a mirror image, so... and that is, that is odd, because that gradient would be exactly the negative of that gradient.
>
> So, yeah, I suppose, just from that basic example I suppose that intuitively does, does seem like it would make sense, but what about… maybe it's just the example of the function I've chosen, but that can't be right, because, what I'm thinking is… if you take, I mean, any [draws another set of axis]… this can do whatever it likes, but say we're interested at some point where it's doing that [draws a small portion of the graph of a generic function in the first quadrant], then it's going to have that gradient and then if we transfer it it's going to be like that, so it's going to have that gradient, which would be the exact opposite of that… yeah, thinking of it like this, it does seem true, just thinking of it in those terms, ummm… like before I'd be happier if I could think of some way to prove it…"

Linvoy uses particular and generic examples as warrants to reach conclusions paired with non-absolute qualifiers such as "[it] does seem like it would make sense," and "it does seem true" (Fig. 10.3). This kind of argumentation proves to be common

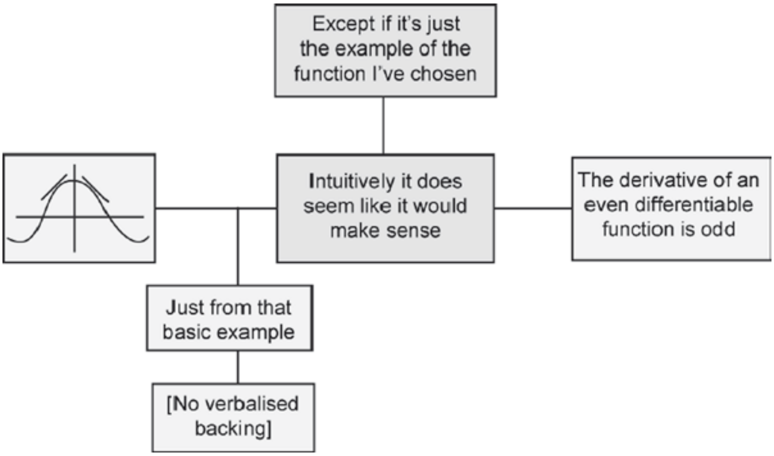


Fig. 10.3 Linvoy's response

not only in the work of undergraduate students, but also successful mathematicians (Inglis et al. 2007). This suggests that in considering proof in mathematics we need to take into account not only the final form of proof, but the nature of the argumentation that leads to the proof which may carry with it different types of warrant and degrees of confidence. During the *building* of a proof, and even at the stage of presenting a proof, warrants need not be absolute, but may be accompanied by qualifiers which may be different for different individuals depending on their experience.

10.5 Natural and Formal Thinking

All individuals build on their met-befores. Pinto and Tall (1999, 2002) expressed this succinctly by distinguishing between *formal thinking* that builds on set-theoretic definitions to construct formal proofs and *natural thinking* that uses thought experiments based on embodiment and symbolism to give meaning to the definition and suggests possible theorems to translate into formal proof.

The met-befores evoked in the building of proof include not only conceptual embodiments, as in Linvoy's case, but also proceptual symbolic calculations, for instance, in group theory developing from permutations, in vector space theory handling matrices, or in analysis performing calculations in specific cases to provide a warrant for the truth of a possibly more general statement.

A *natural* approach can be based on embodiment, symbolism or a combination of both and may continue to link to embodied mental imagery while translating the imagery into a written proof. A *formal* approach, on the other hand focuses on the statement of the theorem and the necessary logical steps to reach the desired conclusion. These distinctions may be seen in the work of famous mathematicians, with Polya, Poincaré, Einstein and Atiyah talking about natural thinking in terms of examples and visualizations while Weierstrass, Dieudonné and MacLane speak of formal thinking based explicitly on well-formulated definitions.

In the undergraduate classroom, Weber (2004) added to this framework a *procedural* approach that simply involves learning the proof by rote. This fits into our framework with a procedural approach corresponding to a more primitive action-schema form of learning while natural and formal thinkers attempting to build up knowledge schemas based on concept image and/or concept definition.

10.6 From Formal Proof Back to Embodiment and Symbolism

A major goal in building axiomatic theories is to build a *structure theorem*, which essentially reveals aspects of the mathematical structure in embodied and symbolic ways. Typical examples of such structure theorems are:

- An equivalence relation on a set A corresponds to a partition of A;
- A finite dimensional vector space over a field F is isomorphic to F^n;
- Every finite group is isomorphic to a group of permutations;
- Any complete ordered field is isomorphic to the real numbers.

In every case, the structure theorem tells us that the formally defined axiomatic structure can be conceived an embodied way and in many cases there is a corresponding manipulable symbolism. For instance, an equivalence relation on a set A – axiomatized as reflexive, symmetric and transitive – corresponds to an embodiment that partitions the set. Any (finite dimensional) vector space is essentially a space of n-tuples that can (in dimensions 2 and 3) be given an embodiment and (in all dimensions) can be handled using manipulable symbolism. Any group can be manipulated symbolically as permutations and embodied as a group of permutations on a set. A complete ordered field specified as a formal axiomatic system corresponds precisely to the symbolic system of infinite decimals and to the embodied visualization of the number line.

Thus, not only do embodiment and symbolism act as a foundation for ideas that are formalized in the formal-axiomatic world, structure theorems can also lead back from the formal world to the worlds of embodiment and symbolism (see Fig. 10.4). These new embodiments are fundamentally different with their structure built using concept definitions and formal deduction. Furthermore, the structure theorems have a life of their own which may go beyond and extend human imagination, as for instance with vector space theory where two dimensional space can be embodied in a plane and three-dimensional space in the human world we live in, yet higher dimensions require conceptual embodiments that are only obtained by deep introspection, as in the case of Zeeman (1960) visualizing how to unknot spheres in five dimensions.

New embodiment and symbolism may be a springboard for imagining new developments and new theorems; it may not. For instance, the embodied interpretation that a complete ordered field is the real line gave generations of mathematicians the belief

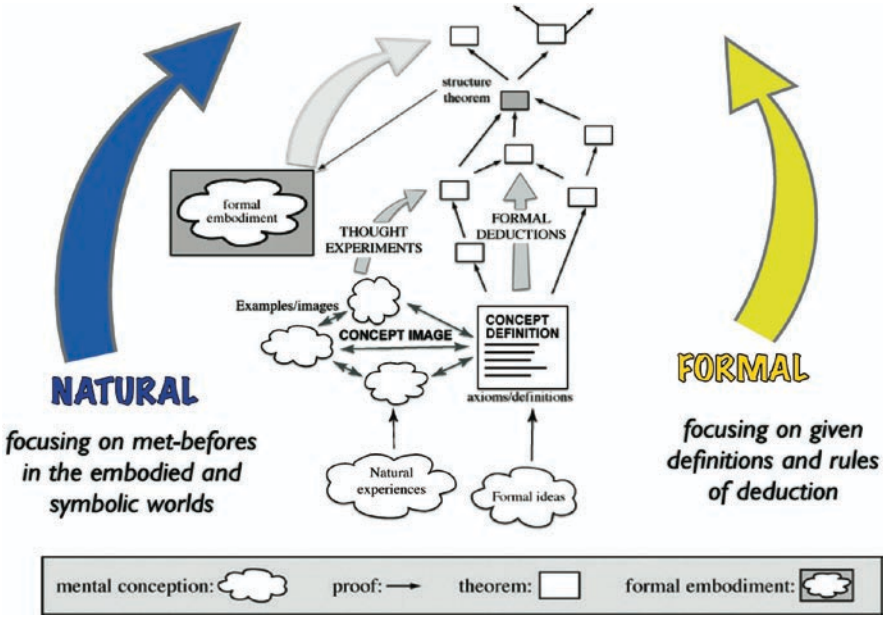


Fig. 10.4 From embodiment and symbolism to formalism and back again (Tall 2002)

that including the irrationals completed the real line geometrically by "filling in all the gaps between rationals." This is not true, for it is possible to imagine (as did earlier generations) that the embodied number line has yet more elements that are infinitesimally close, but not equal, to real numbers (Tall 2005). Thus the embodiment of structure theorems proved formally still need to be considered as warrants for truth that may *suggest* possible new theorems that may in fact be flawed.

Even well-accepted theorems may later prove to have "gaps" in their proof that are not justified by their assumptions that may be based not on logic, but on embodied conceptions of the mathematics. For instance, after 2000 years of belief in the logic of Euclidean proof, Hilbert found a subtle flaw in the proof that the diagonals of a rhombus meet inside the figure at right angles. The Euclidean theory had not defined the notion of "inside" and so new axioms were added to specify when a point C on a line AB was "between" A and B.

10.7 Students and Embodiment in Proof

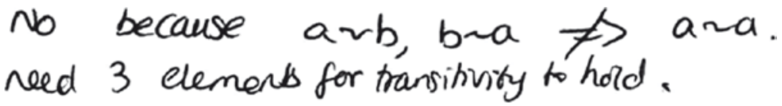
The role of embodiment proves (☺) to be a two-edged sword in the learning of students for it can mislead as well as inspire. For instance, Chin (2002) found that students learning about equivalence relations may embody not the whole definition, but subtly embody *individual axioms*. Thus the transitive axiom if $a \sim b$ and $b \sim c$ then $a \sim c$ for all *a, b, c*

may be interpreted like the transitive law in a strong order relation, so that *a*, *b*, *c* are seen to be *different*.
In his famous lecture given at the turn of the twentieth century, Hilbert (1900/2000) referred to embodiment of the transitive law in the following terms:

> To new concepts correspond, necessarily, new signs. These we choose in such a way that they remind us of the phenomena which were the occasion for the formation of the new concepts. So the geometrical figures are signs or mnemonic symbols of space intuition and are used as such by all mathematicians. Who does not always use along with the double inequality $a > b > c$ the picture of three points following one another on a straight line as the geometrical picture of the idea "between"? (p. 410)

Even Hilbert, the architect of the formalist viewpoint, took inspiration from embodiment.

This may be one explanation of the following statement where a student was unable to deduce that if $a \sim b$ and $b \sim a$ then $a \sim a$:



An alternative explanation put forward by Asghari (2005) noted that the Greek notion of equivalence (in terms of lines being parallel or triangles being congruent) was always conceived in terms of a relation between two *different* things. According to this explanation, an element cannot be equivalent to itself, just as a line fails to be parallel to itself, for it meets itself and two parallel lines do not meet.

Thus it is always necessary to look at the interpretations that individuals place on concepts to find the more subtle sources of their beliefs. As their cognitive structure is built genetically on structures set-before birth and experiences met-before throughout their lives, previous conceptual embodiment and proceptual symbolism will color their thinking in subtle ways.

10.8 Conclusion

Our analysis of how the mathematical thinking is built up by individuals over their life-time from child to mathematician reveals a combination of various kinds of conceptual embodiment and proceptual symbolism leading on to axiomatic formal proof and how concepts that have been met-before affect new thinking. Proof as practiced by mathematicians builds on the experiences that they have integrated into their thinking. Even though proof as an ideal may be considered to be absolute, proof as practiced by human beings, even mathematicians, is a human construct with human strengths of insight and human weaknesses of construction.

In practice, it is not "all or nothing," but is based on implicit or explicit "warrants for truth" that carry with them a measure of uncertainty that varies between individuals and between the ways in which their proofs are framed.

In this paper we have put forward a framework based on conceptual embodiment leading to proceptual symbolism, combining to underpin the axiomatic-formal world of mathematical proof. We have given examples of how mathematicians and students think about proof and how not only does embodiment and symbolism lead into formal proof, but how structure theorems return us to more powerful forms of embodiment and symbolism that can support the quest for further development of ideas. We have also cautioned how proofs presented by students (and also mathematicians) can contain subtle meanings that are at variance with the formalism. Mathematical proof may indeed be the summit of mathematical thinking but it is just the top of one mountain and requires human ingenuity, with all its strengths and flaws, to attempt to reach for the peak of ultimate perfection.

References

Aberdein, A. (2005). The uses of argument in mathematics. *Argumentation, 19*, 287–301.

Aberdein, A. (2006a). The informal logic of mathematical proof. In R. Hersh (Ed.), *Eighteen unconventional essays on the nature of mathematics* (pp. 56–70). New York: Springer.

Aberdein, A. (2006b). Managing informal mathematical knowledge: Techniques from informal logic. In J. M. Borwein, W. M. Farmer (Eds.) *Lecture notes in computer science: vol 4108, Mathematical knowledge management* (pp. 208–221). New York: Springer.

Alcock, L., & Weber, K. (2005). Proof validation in real analysis: Inferring and checking warrants. *Journal of Mathematical Behavior, 24*, 125–134.

Alcolea Banegas, J. (1998). L'Argumentació en matemàtiques. In E. C. Moya (Ed.), *XII Congrés Valencià de Filosofia* (pp. 135–147). Valencia: Diputació de Valencià.

Asghari, A. (2005). *Equivalence*. PhD thesis, University of Warwick.

Chin, E.-T. (2002). *Building and using concepts of equivalence class and partition*. Ph D, University of Warwick.

Dubinsky, E. (1991). Reflective abstraction in advanced mathematical thinking. In D. O. Tall (Ed.), *Advanced mathematical thinking* (pp. 95–123). Dordrecht: Kluwer.

Duval, R. (2006). A cognitive analysis of problems of comprehension in a learning of mathematics. *Educational Studies in Mathematics, 61*, 103–131.

Evens, H., & Houssart, J. (2004). Categorizing pupils' written answers to a mathematics test question: 'I know but I can't explain'. *Educational Research, 46*, 269–282.

Forman, E., Larreameny-Joerns, J., Stein, M. K., & Brown, C. A. (1998). "You're going to want to find out which and prove it": Collective argumentation in a mathematics classroom. *Learning and Instruction, 8*(6), 527–548.

Gray, E., & Tall, D. O. (1994). Duality, ambiguity and flexibility: a proceptual view of simple arithmetic. *Journal for Research in Mathematics Education, 26*(2), 115–141.

Hilbert, D. (2000). Mathematical problems. *Bulletin of the American Mathematical Society*, 37(4), 407–436 (Original work published 1900).

Hoyles, C., & Küchemann, D. (2002). Students' understandings of logical implication. *Educational Studies in Mathematics, 51*(3), 193–223.

Inglis, M., & Mejia-Ramos, J. P. (2008). Theoretical and methodological implications of a broader perspective on mathematical argumentation. *Mediterranean Journal for Research in Mathematics Education, 7*(2), 107–119.

Inglis, M., Mejia-Ramos, J. P., & Simpson, A. (2007). Modelling mathematical argumentation: The importance of qualification. *Educational Studies in Mathematics, 66*(1), 3–21.

Knipping, C. (2003). Argumentation structures in classroom proving situations. In M. A. Mariotti (Ed.), *Proceedings of the 3rd Congress of the European Society for Research in Mathematics Education*. Bellaria, Italy: ERME.

Krummheuer, G. (1995). The ethnography of argumentation. In P. Cobb & H. Bauersfeld (Eds.), *The emergence of mathematical meaning: interaction in classroom cultures* (pp. 229–269). Hillsdale: Erlbaum.

Lakoff, G. (1987). *Women, fire and dangerous things*. Chicago: Chicago University Press.

Pedemonte, B. (2007). How can the relationship between argumentation and proof be analysed? *Educational Studies in Mathematics, 66*(1), 23–41.

Peirce, C. S. (1932). *Collected Papers of Charles Sanders Peirce, Vol. 2: Elements of Logic*. In C. Hartshorne, & P. Weiss (Eds.) Cambridge, MA: Harvard University Press.

Piaget, J., & Inhelder, B. (1958). *Growth of logical thinking*. London: Routledge & Kegan Paul.

Pinto, M., & Tall, D. O. (1999). Student constructions of formal theory: giving and extracting meaning. In O. Zaslavsky (Ed.), *Proceedings of the 23rd Conference of the International Group for the Psychology of Mathematics Education* (Vol. 4, pp. 65–73). Haifa, Israel: PME.

Pinto, M., & Tall, D. O. (2002). Building formal mathematics on visual imagery: a case study and a theory. *For the Learning of Mathematics, 22*(1), 2–10.

Raman, M. (2002). *Proof and justification in collegiate calculus*. Unpublished doctoral dissertation, University of California, Berkeley.

Rasmussen, C., Stephan, M., & Allen, K. (2004). Classroom mathematical practices and gesturing. *Journal of Mathematical Behavior, 23*, 301–323.

Saussure (compiled by Charles Bally and Albert Sechehaye). (1916). *Course in General Linguistics (Cours de linguistique générale)*. Paris: Payot et Cie.

Sfard, A. (1991). On the dual nature of mathematical conceptions: reflections on processes and objects as different sides of the same coin. *Educational Studies in Mathematics, 22*, 1–36.

Stephan, M., & Rasmussen, C. (2002). Classroom mathematical practices in differential equations. *Journal of Mathematical Behavior, 21*, 459–490.

Tall, D. O., Gray, E., Bin Ali, M., Crowley, L., DeMarois, P., McGowen, M., et al. (2001). Symbols and the bifurcation between procedural and conceptual thinking. *Canadian Journal of Science, Mathematics and Technology Education, 1*, 81–104.

Tall, D. O. (ed). (1991). *Advanced mathematical thinking*. Dordrecht, Holland: Kluwer.

Tall, D. O. (2002). Differing modes of proof and belief in mathematics. In *International Conference on mathematics: understanding proving and proving to understand* (pp. 91–107). Taipei, Taiwan: National Taiwan Normal University.

Tall, D. O. (2004a). The three worlds of mathematics. *For the Learning of Mathematics, 23*(3), 29–33.

Tall, D. O. (2004b). Thinking through three worlds of mathematics. *Proceedings of the 28th Conference of the International Group for the Psychology of Mathematics Education* (Vol. 4, pp. 281–288). Bergen, Norway: PME.

Tall, D. O. (2005). The transition from embodied thought experiment and symbolic manipulation to formal proof. In M. Bulmer, H. MacGillivray & C. Varsavsky (Eds.), *Proceedings of Kingfisher Delta'05: Fifth Southern Hemisphere Symposium on Undergraduate Mathematics and Statistics Teaching and Learning* (pp. 23–35). Australia: Fraser Island.

Tall, D. O. (2006). A life-time's journey from definition and deduction to ambiguity and insight. *Retirement as process and concept: a festschrift for Eddie Gray and David Tall* (pp. 275–288). Prague. ISBN 80-7290-255-5.

Toulmin, S. E. (1958). *The uses of argument*. Cambridge: Cambridge University Press.

Toulmin, S. E., Rieke, R., & Janik, A. (1979). *An introduction to reasoning* (2nd ed.). New York: Macmillan.

Weber, K. (2004). Traditional instruction in advanced mathematics courses: a case study of one professor's lectures and proofs in an introductory real analysis course. *Journal of Mathematical Behavior, 23*, 115–133.

Weber, K., & Alcock, L. (2005). Using warranted implications to understand and validate proofs. *For the Learning of Mathematics, 25*(1), 34–38.

Yackel, E. (2001). Explanation, justification and argumentation in mathematics classrooms. In M. van den Heuval-Panhuizen (Ed.), *Proceedings of the 25th Conference of the International Group for the Psychology of Mathematics Education, vol 1* (pp. 9–23). Utrecht, Holland: PME.

Zeeman, E. C. (1960). Unknotting spheres. *Annals of Mathematics, 72*(2), 350–361.

Chapter 11
Historical Artefacts, Semiotic Mediation and Teaching Proof

Maria G. Bartolini Bussi

11.1 Introduction

This chapter presents two examples where physical artefacts have been introduced to encourage young children and secondary students to practice validation. The first involves toothed wheels linked together where the turning of one causes the turning of the other in the opposite direction; the other uses mechanical devices representing and constructing parabolas. The background theoretical framework, presented below, is based on activity theory (Vygotsky 1978), which highlights the use of signs in a social context and is part of a much wider framework of mathematical thinking where artefacts and signs are in the foreground. Bartolini Bussi and Mariotti (2008) presents details and additional examples. Signs include not only words and symbols but also gestures, facial expressions, drawings and other ways of communicating. When a learner is given a mathematical task, even if specific artefacts are called into play, it is not evident that the resulting signs are related to mathematical signs; however, a major aim of teaching is to foster the construction of this relationship.

11.2 Elements of the Theoretical Framework

Here, I will elaborate the seminal idea of semiotic mediation, introduced by Vygotsky (1978), in order to capture a specific kind of classroom activity: *the long-term processes started and controlled by the teacher, who aims at making students learn mathematical meanings and procedures by means of suitable tasks requiring the use of certain artefacts*. This is illustrated in the following diagrams.

The first diagram (Fig. 11.1) contains two different planes: the plane of a pupil's activity (upper) and the cultural plane of mathematics (lower). The artefact that has

M.G. Bartolini Bussi
Department of Mathematics, Università di Modena e Reggio Emilia, Modena, Italy
e-mail: bartolini@unimore.it

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_11,

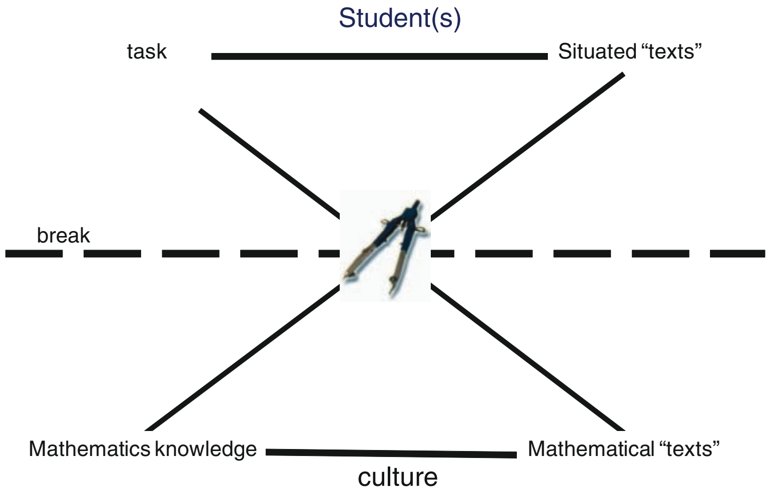


Fig. 11.1 A common situation

the potentiality to link the two planes is represented here by a compass. For a detailed example of compass use in solving construction problems with primary school pupils, see Bartolini Bussi et al. (2007). However, even when a task requiring the use of the compass is solved, the pupil may remain unaware of the link between the compass and Euclid's definition of a circle. Hence, the plane of the pupil's solving process and the plane of mathematical culture may stay separated from each other. The teacher is responsible for constructing multiple links between the two planes, first by choosing a task meaningful for mathematical knowledge, and second by fostering the development of the pupils' own situated texts, produced in the problem-solving process, into mathematical texts that refer explicitly to mathematics culture. To describe this process I say that *the teacher uses the artefact as a tool of semiotic mediation* (Fig. 11.2).

I use the word *artefact* in a very general way to encompass oral and written forms of language; texts; physical tools used during the history of arithmetic (abaci, mechanical calculators etc.) and geometry (ruler, compass etc.); tools from ICT; manipulatives, etc. In the examples considered in this chapter, the artefacts are all taken from the Laboratory of Mathematical Machines (MMLab: www.mmlab.unimore.it), a well-known research center for the teaching and learning of mathematics by means of instruments (Bartolini Bussi 1998; Ayres 2005; Maschietto 2005; Maschietto and Martignone in press). They are everyday mechanisms and toys with toothed wheels (Fig. 11.3); sets of large toothed wheels to be assembled in order to reproduce gears (Fig. 11.4); large reconstructions of ancient geometric models of conic sections, in wood, plexiglas and taut threads (Fig. 11.5) and reconstructions of small tools able to draw arcs of conics (Fig. 11.6).

Whenever an artefact is offered to a user in order to accomplish a given task, some *utilization schemes* emerge: this term follows Rabardel's instrumental genesis (1995), which introduces a distinction between *artefact* and *instrument*.

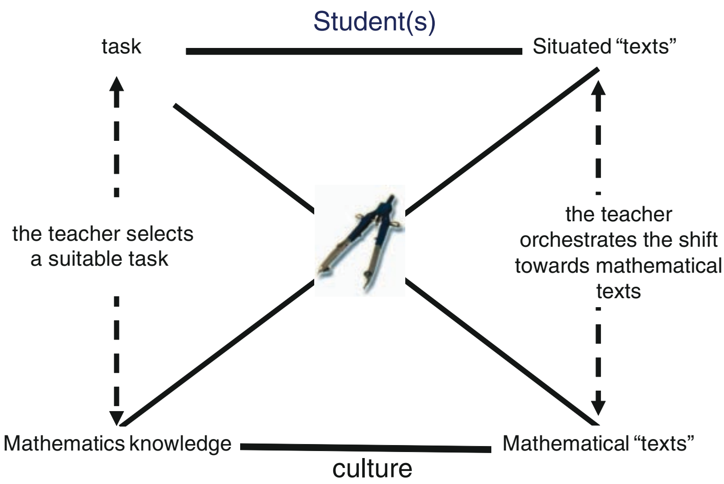


Fig. 11.2 A modified situation

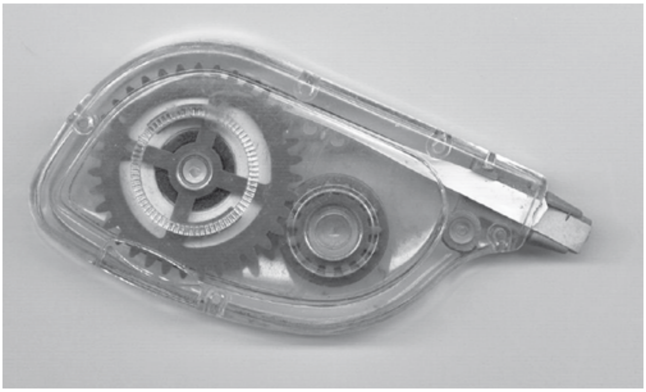


Fig. 11.3 The roller corrector

According to Rabardel, an *artefact* is the material or symbolic object, whilst the *instrument* is a mixed entity made up of both artefact-type components and schematic components (*utilization schemes)*. When using an artefact to accomplish a particular task, the user progressively elaborates the utilization schemes. Thus the instrument is a construction by the individual; it has a psychological character and is strictly related to the context within which it originates and its development occurs. The elaboration and evolution of instruments is a long and complex process Rabardel calls *instrumental genesis*. It can be described by means of two complementary processes:

1. *Instrumentalization:* the emergence and evolution of the different components of the artefact (e.g. the progressive recognition of its potentialities and constraints).
2. *Instrumentation*: the emergence and development of the utilization schemes.

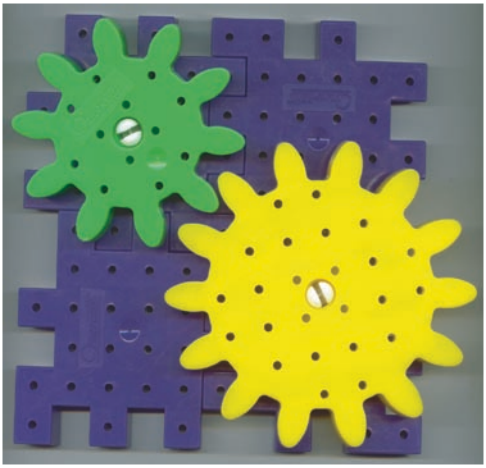


Fig. 11.4 Assemblage of toothed wheels from Georello (Quercetti)

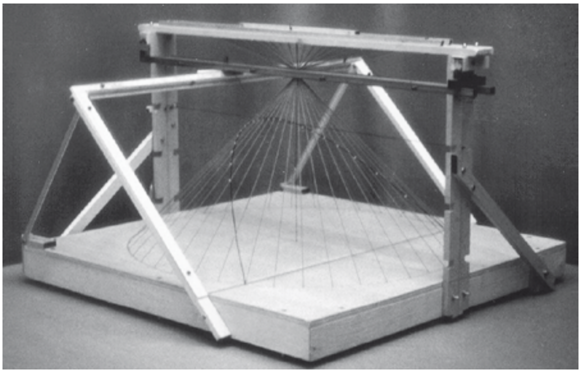


Fig. 11.5 Model of orthotome (the ancient name for parabola)

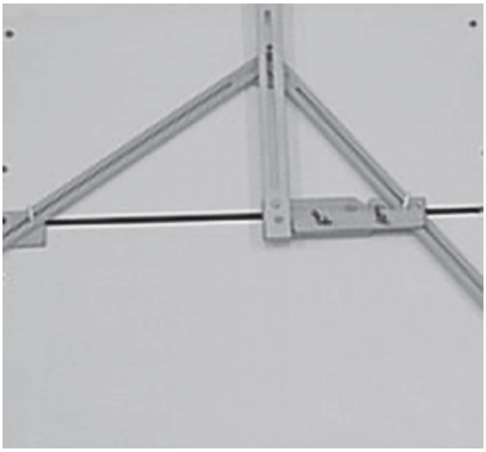


Fig. 11.6 Cavalieri's tool to draw an arc of parabola

These two processes will be illustrated in the two examples below.

On the other hand, processes of semiotic mediation are very complex and involve several subjects. The complexity is well-described in a very short excerpt by Hasan (2002, p. 4), who, presenting semiotic mediation in the linguistic field, emphasizes the need of taking into account:

1. *Someone who mediates; i.e. a* ***mediator****;*
2. *Something that is* ***mediated****; i.e. a content/force/energy released by mediation;*
3. *Someone/something subjected to mediation; i.e. the "****mediatee****" to whom/which mediation makes some difference;*
4. *The* ***circumstances*** *for mediation; viz.,*

   (a) *The* ***means*** *of mediation; i.e. modality;*
   (b) *The* ***location****; i.e. site in which mediation might occur.*

In the empirical studies described in this chapter, the *mediator* is the teacher, the *mediatees* are the pupils, the object of mediation (*mediated*) the idea of validation, the *site* of mediation is the mathematics classroom, the *modality* of mediation is described in the didactical cycles (below) with an intense recourse to physical artefacts.

When semiotic mediation by means of artefacts comes into play, the processes appear to be long-term, lasting weeks or even months. The structure of such teaching sequences may be outlined as an iteration of a cycle where different kinds of activities, aimed at developing the complex semiotic process described above, take place:

1. *Activities with artefacts*: students are faced with tasks to be carried out with the artefact.
2. *Individual production of signs* (e.g. facial expressions, gesturing, speaking, drawing, writing and the like): Students are engaged in different activities centered on semiotic processes, (i.e. the production and elaboration of signs, related to the previous activities with artefacts).
3. *Collective production of signs* (e.g. narratives, miming, collective production of texts and drawings): Collective discussions play a crucial role, specifically one particular type of collective discussion – *Mathematical Discussion* (Bartolini Bussi 1996), a "polyphony of articulated voices on a mathematical object that is one of the motives for the teaching-learning activity" (p. 6).

11.3 First Example: Gears in Primary School Classrooms

In this teaching experiment, the tasks include the exploration of gears and of trains of toothed wheels (everyday objects, toys and ad hoc designed artefacts), the production of interpretative and predictive hypotheses concerning their functioning and the justification of these hypotheses by arguments. In particular, I examine the process of producing early "theorems" about gears.

A *theorem* means (Mariotti this book) a system of three interrelated elements: a *statement* (i.e. the conjecture produced through experiments and argumentations), a *proof* (i.e. the special case of argumentation that is accepted by the mathematical community) and a reference *theory* (including deduction rules – i.e. metatheory – and postulates). In this case, the theory consists in only one postulate, taken from Hero (Mechanics, book 1): *Two circles in gear by means of teeth turn in opposite directions. One turns right, the other turns left* (Carra de Vaux 1988). In this ancient text, the words refer to the observer's viewpoint above two horizontal wheels geared together: instead of left and right, today one would say anticlockwise and clockwise, but these words could not be used before clocks were invented.

Next, I shall sketch a reconstruction of the teaching experiment (illustrated in detail by Bartolini Bussi et al. 1999), interpreting the long-term process according to the above theoretical framework.

11.3.1 The Didactical Cycles

The observed classroom is a paradigmatic case of many primary (and even secondary) classrooms that have implemented similar didactical cycles from Grade 2 on. In short, the initial steps of the activity are the following:

First didactical cycle (*mechanisms and gears*):

1. Individual or small-group activity with everyday artefacts with gears inside (e.g. toys carried by the pupils; kitchen tools like salad shakers, corkscrews, eggbeaters);
2. Individual production of oral, written and graphical description of the artefacts functioning;
3. Mathematical discussion of the individual signs produced in the previous activity

In this cycle a transparent roller corrector (Fig. 11.3 above) plays a special role. When pupils are asked to describe (by means of different systems of signs) the functioning of these artefacts, instrumentalization begins:

1. The presence and the mesh of teeth is emphasized, and the early drawings show teeth that are out of proportion with the wheel itself.
2. The round shape of the wheel is not taken for granted, and drawings and cardboard models of squared wheels are produced in some classrooms; special activities are required to overcome this representation, which conflicts with the need to keep the center of the wheel fixed.

Second didactical cycle (gears in the foreground):

1. Individual or small group work with large prototypes of "generic" gears, produced by the pupils by means of isolated toothed wheels (Fig. 11.4 above) that are different from yet evocative of the toys and everyday objects.
2. Representation of the functioning (as above).
3. Collective discussion.

When pupils are asked first to use these prototypical simple gears and later to describe their functioning, instrumentation takes place (see Fig. 11.7):

1. When the focus is on one large wheel (rather than on the mechanism as a whole or even as a black box), particular utilization schemes emerge; because it is natural to drive a wheel by either gripping it in the palm or pushing it with a finger, two different utilization schemes emerge, together with particular drawings and verbal expressions, which imply global or pointwise modelization.
2. When the focus shifts to the motion, the teeth do not appear important and simplified drawings emerge, where the toothed wheels are replaced by circles.
3. When the attention is captured by two wheels in gear with each other (as in Fig. 11.4 above) other utilization schemes emerge, for example:

    a. Rotating one wheel (according to the global or the pointwise model) and following the other with one's eyes.
    b. Rotating both wheels with a hand each and perceiving the ease or the resistance of the motion, according to the direction of rotation of each.

It is quite easy, in this case, to "discover" Hero's postulate, which the pupils themselves state with emphasis on the opposite directions of rotations, using pairs of arrows (see Fig. 11.7, case 5; and Fig. 11.11 below).

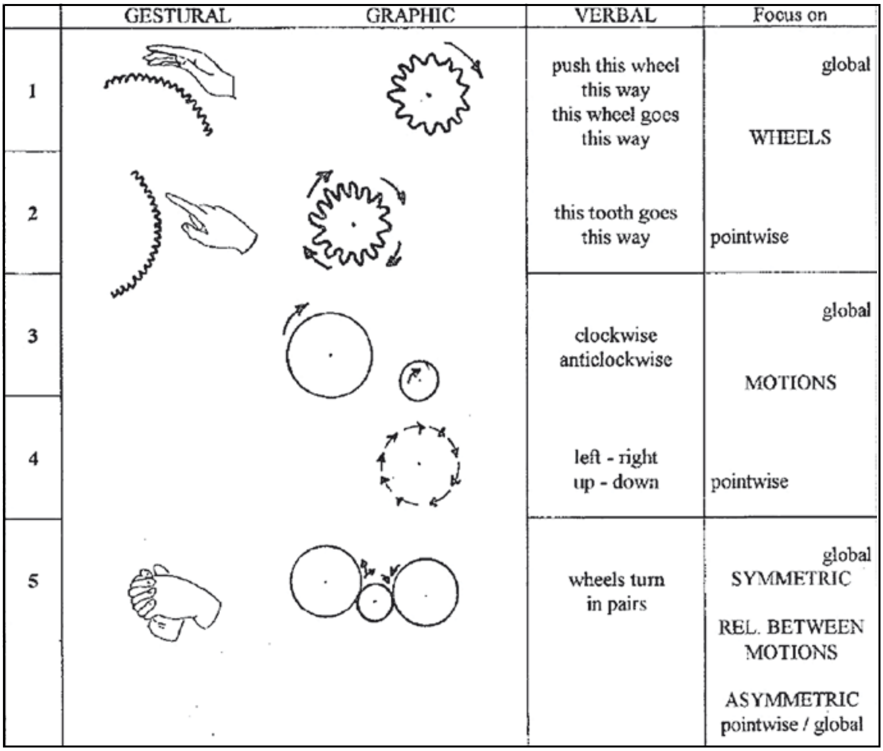


Fig. 11.7 Signs and gears: extracted from Bartolini Bussi et al. (1999)

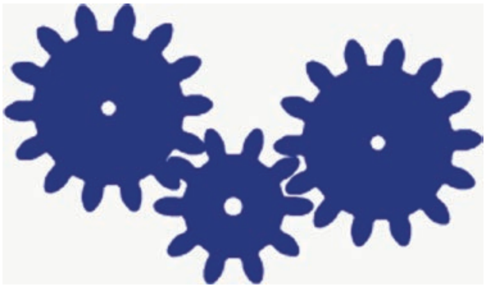


Fig. 11.8 A line of wheels

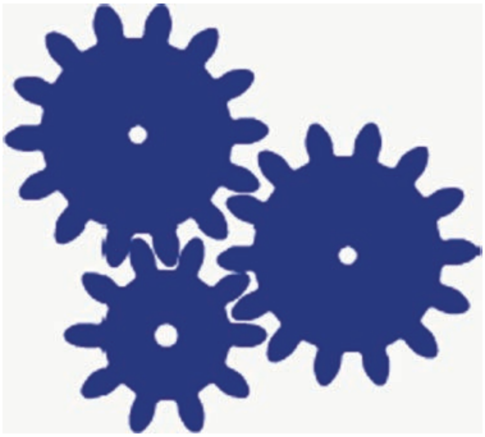


Fig. 11.9 A "clover" of wheels

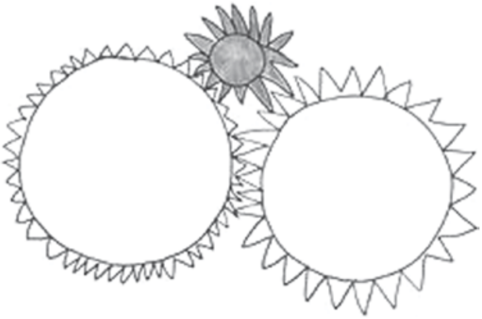


Fig. 11.10 In this case, when the two wheels are in gear the above one is broken, as it cannot follow both wheels

Fig. 11.11 If the A wheel turns left, the B wheel turns right. How can the other wheel turn? It cannot turn right because of the B wheel and it cannot turn left because of the A wheel. Hence they cannot turn

11.3.2 A Crucial Task

From Grade 3 on, a crucial task has been introduced in a further didactical cycle and has proved to be within the reach of the pupils:

We have often met planar wheels in pairs. What if there were three wheels in a set? How could they be positioned? You must always give the necessary explanations and write down your observations.

The pupils start analyzing the easy situation of a line of three toothed wheels (Fig. 11.8). Then many pupils "move" the wheels (that are drawn on the paper) and come to the situation of the Fig. 11.9, where wheels are arranged in a "clover" (this reference was used by the pupils). Primary school pupils may produce different answers concerning the inevitable locking of the gear in Fig. 11.9:

1. Some pupils recognize the locking but give no explanation.
2. Some reproduce the situation with available wheels and observe (empirically) that it does not work.
3. Some acknowledge a conflict (see Fig. 11.10).
4. Some construct a theoretical argumentation that makes explicit reference to Hero's postulate (Fig. 11.11).

The argument of Fig. 11.10 evidences a conflict, while the last argument (Fig. 11.11) has the structure of a logical proof. The statement (*they cannot turn*) is justified with reference to the postulate (*If the A wheel turns left, the B wheel turns right),* not to any experiment. The reasoning is structured as a proof by contradiction: the possibility of movement is imagined (in a mental experiment: *If the A wheel turns left…*), but, combined with the postulate, gives rise to a contradiction. The shared knowledge is later transformed into a collective text (see Bartolini Bussi et al. 1999), where the "theory of planar gears" is reconstructed.

11.3.3 Discussion

The artefacts used (toys, everyday tools and, above all, modular prototypes of physical gears) are tools of semiotic mediation. What has been *mediated*? Surely some pieces of mathematics knowledge (that have been officially fixed in the "theory of planar gears"). Yet the mediation also comprised a theoretical attitude that fosters and gives values to mental experiments and to validation based on *texts*. We had evidence of that, when the theory of planar gears was constructed collectively. In most classrooms (in Grade 4 and 5) the pupils wished to extend the validity of the statements to include any number of wheels: this need has a "theoretical" nature, as concrete gears have only a specific number of wheels. The pupils stated in the discussion that it is necessary to distinguish between the concrete gears built on the table and the "mental" gears imagined in the mind, which could contain infinitely many wheels.

The function of the concrete artefacts, in this case, has been twofold:

1. They have allowed the production of the postulate, with the conviction that it can be assumed as a sound basis of the subsequent theory.
2. They have fostered the intense semiotic activity that nurtured the construction of the syntax of the functioning of trains of gears.

11.4 Second Example: Conics (and Conic Sections) in Secondary-School Classrooms

About 200 mathematical machines, working reconstructions from the history of geometry, are available in the Laboratory of Mathematical Machines (the MMLab). A mathematical machine is a tool that forces a point to follow a trajectory or to be transformed according to a given law. The prototypes of the two most important categories present in the MMLab are the standard geometric compass (that forces a point to go on a circular trajectory) and the Durer glass used as a perspectograph (that transforms a point into its perspective image). Several teaching experiments have been carried out in classrooms at all school levels, with mathematical machines offered by the MMLab, according to the framework of semiotic mediation illustrated in this chapter. (For a wide collection of examples see Bartolini Bussi and Maschietto 2006). Here I focus on a particular example concerning conic sections and conics in secondary school. The original classroom experiment was developed for Grade 12 (Bartolini Bussi 2005) and has since been applied to design laboratory exploration for Grade 11, 12 and 13 classrooms[1]. In the following, the structure of the experiment will be recalled and shortly revisited according to the theoretical framework of semiotic mediation.

In the classical era, mathematicians studied conic sections in three-dimensional space in order to detect properties expressed by proportions or metric properties (e.g. focus, directrix properties). Later (seventeenth century) geometers used both kinds of properties to construct tools for drawing conics.

In secondary mathematics teaching, referring to the three-dimensional approach to conic sections is common but does not seem really effective. We studied this

[1] The availability of mathematical machines in a mathematics classroom cannot be taken for granted. Hence, there is the risk that such a teaching experiment cannot be reproduced for lack of tools. This is the main reason why some years ago the MMLab was opened to classrooms, under the guidance of laboratory operators. The person responsible for this activity is Michela Maschietto. The activity has been designed in order to offer a 2-h reconstruction (a short one) of the classroom experiments with mathematical machines. An average of 1300–1500 secondary students a year come with their mathematics teacher to experience the mathematics laboratory hands-on. These numbers are demanding, yet represent a tiny proportion of the whole population. Hence, our research group aims at disseminating this activity by offering schools travelling exhibitions, ready-made kits and work-sheets. A long documentary on a typical classroom visit (in Italian), broadcast by the national network RAIeducational (Explora scuola), is available at http://www.explora.rai.it/online/amministrazione/uploads/asx/97302_exp.asx.

phenomenon some years ago, even at the university level. We found that students after several university courses in mathematics seemed correctly convinced that the transverse section of a cone on a suitable plane formed an ellipse; yet they were unable to argue against wrong, naïve statements concerning the shape of the section. In particular, we asked them to react to the historically documented statement (Dürer 1525) that a conic section is egg-shaped because the width of the cone near the vertex is narrower than the width near the base (see Bartolini Bussi and Mariotti 1999): however, they could argue for the true symmetry of the section, which seemed to conflict with that false perception.

Actually, in standard classrooms, the anecdotal reference to conic sections shifts immediately to the metric definition (by focus and directix properties) and to its translation into the analytic frame, with the production of canonical equations of conics. In principle, secondary-school students do not lack the knowledge necessary to carry out the study of three-dimensional conic sections (i.e. the properties of similar triangles and of proportions), as far as finding a synthetic description, as shown by a case study of the parabola in a grade 12 classroom (Bartolini Bussi 2005). The exploration of physical, tangible artefacts fosters the production of statements within the frame of elementary geometry. Yet, the students lack a fully-fledged theory that includes three-dimensional elementary geometry up to the study of conic sections. Hence, by means of an intentional anachronism, it is appropriate to translate the statements about proportions into algebraic equations (see below) that represent conics in a Cartesian system of coordinates and are familiar to students. A summary of the experiment follows.

11.4.1 The Didactical Cycles

The two initial didactical cycles of the experiment concern conic sections and conic drawing devices.

First didactical cycle (conic sections)

A large model (Fig. 11.5) was available for small group work: it is a model of an orthotome, that is the section of a right-angled cone by means of a plane perpendicular to a generatrix. The main steps are as follows:

1. Short historical introduction by the teacher.
2. Small-group activity with the orthotome model: the aim was conjecturing the property of the section (i.e. the characterization, by means of proportions, of the position of any point of the section) and proving it in the frame of elementary geometry.
3. Interpretation of the property as an equation in a suitable Cartesian system of coordinates.
4. Written report by the group to explain the process.
5. Discussion with the teacher of the report(s), in order to frame the outcomes of the process into a broader historical approach.

A rigorous proof of the property of the section draws on the comparison of some similar triangles that belong to different planes (see Fig. 11.12).

In the base circle,
DE: EB = EB: FE.
As VHA is similar to EAF,
AV: FE = HA: AE = 2 HA: 2 AE = IA: 2 AE = DE: 2 AE.
AV: FE = DE: 2 AE.
2 AE · AV = FE · DE = EB · EB.
2 AV · AE = EB · EB.

This relationship describes the property of point B on the conic section. The same property holds for point C. The last equation represents what the Greek geometers called the "symptom" of an orthotome (i.e. a characteristic relation to describe the position of points).

In modern notation, posing:
AV = p; AE = x; EB = y
this may be written:
$y^2 = 2px$

which is the familiar canonical equation for a parabola.

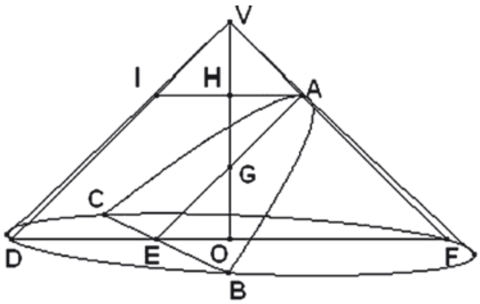


Fig. 11.12 The orthotome with labels

The students were able, with some help from the teacher, to exploit the historical context and the artefact and to link the discovered property to the canonic equation for a parabola. Because of space constraints, I do not describe this long process here (see Bartolini Bussi 2005, for details). One helpful observation: the shift from the statement about proportions to the equation is delicate, because it involves the shift from a particular case (the point B on the base plane of the wooden model) to a general case (because x and y are variables). In the classroom the students expressed this without words through a meaningful gesture: bringing the hands close to the wooden model, embracing it and pretending to move the wooden base plane up and down. This motion is imaginary, because the model is static, heavy and firmly set on the table; yet it allows them to "raise" the points B and C to a whatever height on the cone.

Second didactical cycle (conic drawing devices)

In the original experiment, the second didactical cycle involved small-group exploration of a conic drawing device, followed by whole-class discussion under the teacher's guidance, in order to prove that the drawn arc was actually a part of a conic. This part of the original experiment was later transposed to the MMLab[2] and tested many times during classroom visits for hands-on activity. In the laboratory, multiple copies of small (40 cm × 40 cm) drawing tools are available, allowing several groups to work on the same model. Here, I present only the case with Cavalieri's tool (Fig. 11.6)[3].

A small group of students is given a copy of the tool with a sheet of paper stuck on the wooden board. The exploration sheet contains a drawing (Fig. 11.13a) of the tool and the following guided task[4]:

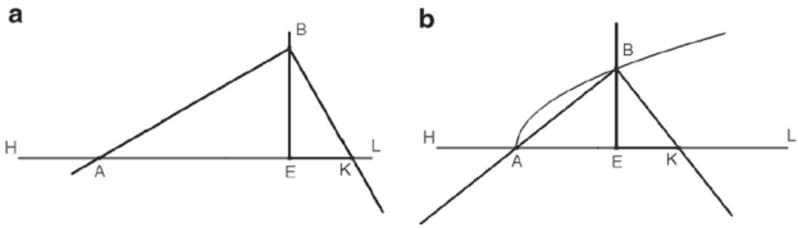


Fig. 11.13 13a (left) and 13b (right): two states of Cavalieri's tool[5]

Exploration Sheet: Cavalieri's tool[6]

1) *How many bars are there in the linkage?*
2) *Which figures are formed by the bars?*
3) *Move the linkage. How do the vertices of the figures behave during the motion?*
4) *How many degrees of freedom do the vertices have?*
5) *Which parts of the linkage are unchanged during the motion?*
6) *Insert the lead refill into the hole in B and trace an arc, moving the linkage. Do you know what curve it is? Why?*

---

[2] In a short visit (less than 2 h), the exploration of three-dimensional models is carried out by the laboratory operator during the historical introduction.

[3] The interested reader may download a Cabri simulation from the website (in Italian): http://associazioni.monet.modena.it/macmatem/lauree%20sc/Caval.htm, by clicking on "simulazione" on the right.

[4] In a mathematics classroom, if more time is allotted, more freedom can be left for students' exploration.

[5] In Fig. 13, the point A and the length of the bar KE are fixed; K is dragged back and forth in the rail HL, pulling KEB and forcing the fissured side BA of KBA to rotate around A. Fig. 13a is taken from the exploration sheet. Fig. 13b shows the tool in another state, after a short sliding of K on the horizontal rail HL with the dependent rotation of BA around A; also the path of B during the motion (i.e. an arc of parabola) has been drawn, i.e. the same drawing that students produce in the step 6. Fig. 13b is not taken from the exploration sheet, but is added here for the reader's understanding: the same letters as in Fig. 12 have been used for the sake of clarity.

[6] This exploration sheet has been designed and tested by the staff of the Laboratory of Mathematical Machines (Michela Maschietto with Carla Zanoli, Rossana Falcade, Francesca Martignone).

7) *Name x the variable line segment* AE*; y the variable line segment* EB*; p the constant line segment* EK. *Write the relationship between x*, *y and p looking at the right-angle triangle* ABK.
8) *Do you know the curve given by the above equation?*

The first two Questions address the instrumentalization process, as they concern the emergence of the different components of the artefact. From then on, instrumentation is called into play. A first conjecture on the curve may be produced as an answer to Question 6, where also a justification (*why*?) is required. However, the students are not expected here to construct a rigorous proof of their conjecture, because it is not easy to relate the functioning of this tool to the usual focus-directrix definition of a parabola. Actually, the suggested path towards justification draws (Questions 7 and 8) on the analytical frame, as secondary school students are supposedly more familiar with the equation for a parabola (Fig. 11.13).

The triangle ABK is right-angled. Hence there is the proportion:

AE: EB = EB: EK

that may be written also as:

$y^2 = 2px$

which is the canonic equation for a parabola.

For the students, the proof that the conic section of the static model of the orthotome (Figs. 11.5 and 11.12) and the arc drawn by B during the motion of Cavalieri's tool (Figs. 11.6 and 11.13) are parts of the same curve rests on the fact that the equation is the same, when EK = 2 AV (Fig. 11.14).

In the case of the orthotome, the shift from the properties of the particular points B and C to any point of the section requires a mental experiment (miming the motion of the base plane up and down). However, in the case of Cavalieri's tool, the

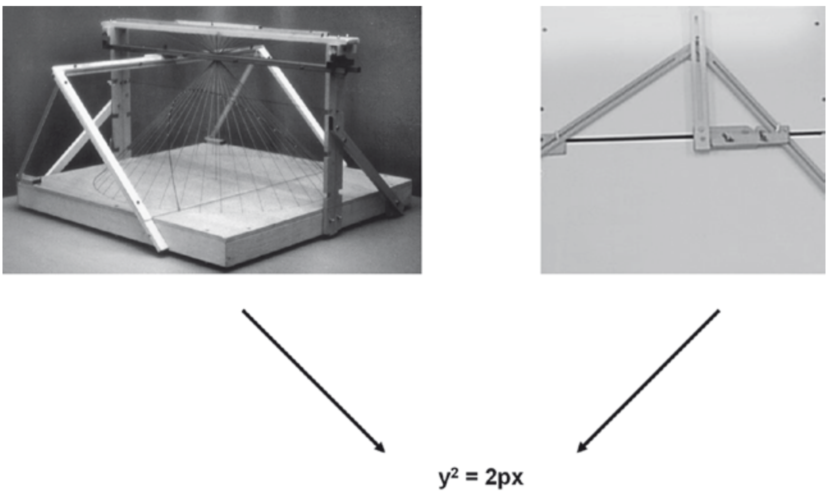


Fig. 11.14 From synthetic to analytic perspective

real motion of the tool, while the triangles ABK and BEK remain right-angled, allows one to realize "infinitely many" true experiments[7].

11.4.2 Discussion

The artefacts used are tools of semiotic mediation. But what has been *mediated*? Besides some mathematics knowledge, concerning the synthetic and analytic theories of conics, there are other important objects of mediation, which can be reconstructed from the analysis made by Bartolini Bussi (2005): the dynamic interpretation of either dynamic or static objects, in order to propose conjectures and to guide the construction of early proofs and the ability to shift from one setting (the spatial setting of conic sections, the plane setting of conic drawing devices or the algebraic setting of conic equations) to another, with a continuous change of focus. What they know about conics allows students to move to and from the individual sections and settings, using the most advantageous tools for proving.

In this case, the concrete artefacts have had many functions:

- They have offered the contexts for historical reconstruction, for dynamic exploration and for the production of a conjecture.
- They have offered continuous support during the construction of a proof framed by elementary geometry.
- They have given a geometrical meaning to the parameter "p" that appears in the conic equation.

On this last point, students are always astonished to see that the parameter p of the equation, which is traditionally defined as the distance between focus and directrix, has also other geometrical meanings: it is twice the distance from the vertex of the parabola to the vertex of the right-angled cone in the orthotome; it is the length of the constant side EK in Cavalieri's tool. Hence, to obtain a parabola of a different width, it is necessary to change either the distance AV or the length EK.

11.5 Conclusion

The two examples (gears in primary school and models and tools for conics in secondary school) show contexts where physical, and tangible artefacts are used by the teacher as tools of semiotic mediation: the main object mediated is the pair conjecture-validation. The treatment of the two examples is consistent with the discussion of the genetic approach to proof discussed by Jahnke (2007), although Jahnke makes no explicit reference to semiotic mediation and didactical cycles. In

[7] It is beyond the scope of this chapter to analyze the similarities and differences that emerge in the exploration of ancient tools and present dynamic geometry environments.

particular, the single postulate of Hero's theory is an example of what Jahnke calls a principle (or hypothesis).

The different levels of schools studied (Grades 3 and 4 and Grades 11, 12 and 13) allow us to place these examples at two different points of the long path towards formal proof. In their chapter, Tall and Mejia-Ramos (this book) distinguish three different worlds of mathematics:

– the *conceptual-embodied* (based on perception of and reflection on properties of objects).
– the *proceptual-symbolic* that grows out of the embodied world through actions (such as counting) and symbolization into thinkable concepts such as number, developing symbols that function both as processes to do and concepts to think about (procepts).
– the *axiomatic-formal* (based on formal definitions and proof) which reverses the sequence of construction of meaning from definitions based on known concepts to formal concepts based on set-theoretic definitions.

The examples of this chapter belong to the first (gears) and the second (conics) worlds. They tackle two widespread misunderstandings, shared by many mathematics teachers:

1. Young pupils can empirically verify but not theoretically validate mathematical statements.
2. Manipulation of tangible objects can be a starting point but inhibits validation for secondary-school students.

The two examples show that teachers may successfully introduce physical artefacts into mathematics classrooms at both the primary and secondary levels as tools of semiotic mediation, and that they can mediate mathematical content as well as the process of mathematical validation.

Acknowledgments Research funded by MIUR (PRIN 2007B2M4EK: "*Instruments and representations in the teaching and learning of mathematics: theory and practice*").

References

Ayres, A. (2005). Le Macchine Matematiche at the Laboratory of Mathematical Machines in Modena. *Bullettin of the British society for the History of Mathematics*, *6* (Autumn), 9–14.

Bartolini Bussi, M. G., & Maschietto, M. (2006). *Macchine Matematiche: dalla storia alla scuola*. Milano: Springer Italia.

Bartolini Bussi, M. G. (1996). Mathematical discussion and perspective drawing in primary school. *Educational Studies in Mathematics, 31*, 11–41.

Bartolini Bussi, M. G., & Mariotti, M. A. (in press). Semiotic mediation in the mathematics classroom: Artefacts and signs after a Vygotskian perspective. In L. English et al. (Eds.) *Handbook of International research in mathematics education*. Philadelphia, PA: Taylor & Francis Group, LLC.

Bartolini Bussi, M. G. (1998). Drawing instruments: theories and practices from history to didactics. *Documenta Mathematica*, Extra Volume ICM98 (3), 735–746.

Bartolini Bussi, M. G. (2005). The meaning of conics: historical and didactical dimension. In C. Hoyles, J. Kilpatrick, & O. Skovsmose (Eds.) Meaning in mathematics education. Dordrecht: Kluwer.

Bartolini Bussi, M. G., Boni, M., Ferri, F., & Garuti, R. (1999). Early approach to theoretical thinking: gears in primary school. *Educational Studies in Mathematics, 39*, 67–87.

Bartolini Bussi, M. G., & Mariotti, M. A. (1999). Semiotic mediation: from history to mathematics classroom. *For The Learning of Mathematics, 19*(2), 27–35.

Bartolini Bussi, M. G., Boni, M., & Ferri, F. (2007). Construction problems in primary school a case from the geometry of circle. In P. Boero (Ed.), *Theorems in school: from History, Epistemology and Cognition to classroom practice* (pp. 219–248). Rotterdam: Sensepublisher.

Carra de Vaux, B. (1988). *Les Mécaniques ou L'Élévateur de Héron d'Alexandrie*. Paris: Les Belles Lettres.

Dürer, A. (1525). *Underweysung der Messung mit Zirkel und Richtscheit*. French translation: Peiffer J (1995). *Géométrie*. Paris: Ed. Seuil.

Hasan, R. (2002). Semiotic mediation, language and society: three exotropic theories – Vygotsky, Halliday and Bernstein. In J. Webster (Ed.) *Language, society and consciousness: the collected works of Ruqaya Hasan*, Vol. 1. Equinox, London, available on line in http://lchc.ucsd.edu/MCA/Paper/JuneJuly05/HasanVygHallBernst.pdf (last seen March 20, 2008).

Jahnke, H. N. (2007). Proofs and hypotheses. *ZDM – The International Journal of Mathematics Education, 39*(1–2), 79–86.

Mariotti, this book.

Maschietto, M. (2005). The laboratory of mathematical machines of Modena. *Newsletter of the European Mathematical Society, 57*, 34–37.

Maschietto, M., & Martignone, F. (in press). Activities with the mathematical machines: pantographs and curve drawers. *Proceedings of the 5th European Summer University on the History and Epistemology in Mathematics Education,* Univerzita Karlova, Prague (Czech Republic).

Rabardel, P. (1995). *Les hommes et les technologies – Approche cognitive des instruments contemporains*. Paris: A. Colin.

Tall and Mejia-Ramos, this book.

Vygotskij, L. S. (1978). *Mind in society. The development of higher psychological processes*. Cambridge: Harvard University Press.

Chapter 12
Proofs, Semiotics and Artefacts of Information Technologies

Maria Alessandra Mariotti

12.1 Introduction

This paper discusses some aspects of long-term teaching experiments, carried out with the goal of introducing pupils to proof. I will present two experimental examples of contexts for approaching proof centred on using a computer-based environment, structuring my discussion around the notion of semiotic mediation and its derived didactic model (Mariotti 2002; Bartolini Bussi and Mariotti, 2008).

The experiments are part of a joint research program on semiotic mediation in the mathematics classroom (Bartolini Bussi and Mariotti in press); we adopted the paradigm of research for innovation in the mathematics classroom (Arzarello and Bartolini Bussi 1998). In this paradigm, practice and theory nurture each other in a complex interlaced process. We carried out successive teaching experiments on a single class of students with a single teacher over the ninth and tenth grades. We designed our teaching experiments with strict and continuous collaboration with the teacher, with whom we designed the pedagogical plan and analysed students' behaviours and productions. Initially our implementation of the innovative didactic strategies was driven by a number of vague pedagogical assumptions. During any teaching experiment, we tried to formulate, cyclically refine and clarify our theoretical hypotheses. Thus, over several years, we developed a theoretical framework that clarifies and formalizes our initial, vague intuition (Mariotti 1996), placing this framework within a Vygotskian approach based on the key notion of semiotic mediation.

M.A. Mariotti (✉)
Dipartimento di Scienze Matematiche ed Informatiche, Università di Siena,
Piano dei Mantellini 44, Siena, 53100, Italy
e-mail: mariotti.ale@unisi.it

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perspectives,* DOI 10.1007/978-1-4419-0576-5_12,

12.2 Artefacts and Signs in a Vygotskian Perspective

12.2.1 Tools of Semiotic Mediation

Vygotsky explicitly addressed the role of tools and their function as a source of knowledge in a broader perspective that sees the evolution of human cognition as an effect of social and cultural interaction. Elaborating on the Vygotskian seminal idea of semiotic mediation, we set up a pedagogical model to describe and explain the functioning of artefacts' use in the teaching/learning process. The following short description of the model is strictly limited to clarifying the discussion on the two experimental examples in this paper. (For a full discussion and more references, see Bartolini Bussi and Mariotti, 2008).

Vygotsky (1978) used the semiotic lens to interpret individual knowledge construction, in Vygotskian terms *internalization*, as a social endeavour. His basic assumption is that the internalization process is essentially *social* as well as directed by *semiotic* processes related to communication involving the production and interpretation of signs, in what can be called *interpersonal space* (Cummins 1996).

A fundamental Vygotskian hypothesis proposes that shared meanings are generated within the social use of artefacts in the accomplishment of a task (that involves both a mediator and a mediatee). On the one hand, these meanings relate to the accomplishment of the task, in particular to the artefact used; on the other hand they may relate to particular content. In other terms, a semiotic potential resides in any artefact consisting in the double semiotic link that the artefact has with both the personal meanings that emerge from its use, and the academic knowledge evoked by that use insofar as this can be recognized by an expert. These semiotic relationships hinged in the artefact may become the object of an a priori analysis involving in parallel a cognitive and an epistemological perspective, which can lead to the identification of "the semiotic potential of an artefact with respect to particular educational goals" (Bartolini Bussi and Mariotti, 2008, p. 758). In this respect any artefact, belonging either to the set of new technologies or to the set of ancient technologies, may serve as a valuable educational tool, although identifying this potential might require different approaches (see Bartolini Bussi this volume).

Exploiting the artefact's usefulness requires the expert – for instance, the mathematics teacher – to be aware of its potentialities, in terms of both the emergent mathematical meanings and the emergent personal meanings. On the one hand, this means orchestrating didactic situations where students face designed tasks for which they are expected to mobilize specific schemes of artefact utilization and consequently to generate personal meanings. On the other hand, it means to orchestrate social interactions with the aim of making the personal meanings that have emerged during the artefact-centred activities develop into the mathematical meanings that constitute the teaching objectives. "Thus any artefact will be referred to as *tool of semiotic mediation* as long as it is (or it is conceived to be) intentionally used by the teacher to mediate a mathematical content through a designed didactical intervention."

(Bartolini Bussi and Mariotti, 2008, p. 758). According to the semiotic mediation theory, the complex semiotic processes of creation and evolution of personal meanings towards mathematical meanings can be developed through the design and implementation of the "Didactical Cycle" (Bartolini Bussi and Martiotti, 2008, p. 758 ff), an iterative cycle of the following activities: activities with the artefacts, individual production of signs, and collective production of signs.

Thus, the analysis of an artefact's semiotic potential encompasses analysing both the personal and mathematical meanings (Leont'ev) related to the artefact as well as the possible tasks which can be accomplished with it. What makes this analysis significant from a didactic point of view is its results' consistency with specific educational goals. The discussion of the following examples concerns the common educational goal consisting in introducing students to a theoretical perspective and the common feature of exploiting the semiotic potential of a computer-based environment.

12.3 Example 1: The Semiotic Potential of a Dynamic Geometry Environment

My first example concerns a particular Dynamic Geometry Environment (DGE), *Cabri-géomètre* (Laborde and Bellemain 1998) and its didactic potentialities. My aim is to highlight this DGE's semiotic potential with respect to the educational goal of introducing students to the idea of mathematical proof.

In a DGE, the user can construct figures with the tools in the menus and test the robustness of these figures through what researchers (Arzarello et al. 2002; Olivero 2002) have defined as the "dragging test". These figures can then be interpreted as constructions within Classical Euclidean Geometry. The starting point of our analysis lies in this evident, immediate relationship between the *Cabri* figures and their corresponding geometrical constructions. That relationship can be elaborated, from the point of view of semiotic mediation, through both epistemological and cognitive analyses leading to the definition of the *Cabri* artefact's semiotic potential.

Euclidean Geometry, traditionally referred to as "ruler and compass geometry", gives a central role to construction problems whose theoretical nature is clearly stated, in spite of their apparent practical objective – that is, the drawing which can be produced on a sheet of paper following the solution procedure. As Vinner clearly pointed out in his review of Martin's book on geometric construction (Martin 1998), "The ancient Greek undertook a challenge which in a way represents some of the most typical features of pure mathematics as an abstract discipline. It is not related to any practical need." (Vinner, 1999, p.77). In fact, as Euclid's Elements show, the use of ruler and compass generates a set of axioms defining a theoretical system, within which the correctness of the construction is *validated* by a Theorem.

Since their appearance, DGEs have triggered a new revival of the field of geometrical constructions, providing virtual tools that simulate the drawing tools of classic Geometry: lines, circles and other figures can be drawn and made to intersect each other, nicely reproducing on the screen what for centuries was drawn on sand, papyrus, or paper. However, compared to the classic world of paper-and-pencil figures, the novelty of a DGE consists in the possibility of direct manipulation of its drawn figures through the use of the mouse. As a consequence, the stability ("robustness", as some authors call it, e.g., Jones 2000, p. 58) of the drawn figure in respect to mouse-dragging constitutes the natural test of correctness for any construction task in the *Cabri* environment, in which "Dragging points of their constructions disqualifies purely visual strategies, by illustrating how constructions can be 'messed-up' […]" (Healy, Hoelzl, Hoyles and Noss 1994, p. 14).

In fact, the core of the dynamics of a DGE figure, as realized by the dragging function, consists in preserving its intrinsic geometric relationships. The elements of any figure in a DGE are related according the hierarchy of properties determined by its construction procedure. That hierarchy of properties corresponds to a relationship of logical conditionality. The set of tools in a DGE is arranged to correspond to the set of constructing tools in Euclidean Geometry (Laborde and Laborde 1995). This correspondence allows the control "by dragging" to be put in relationship with "proof and definition" within the system of Euclidean Geometry (Mariotti 2000; Jones 2000; Stylianides and Stylianides 2005).[1]

In sum, the *Cabri* tools stand in a double relationship: on the one hand, to the construction task that can be realized through them, resulting in a figure on the screen; and, on the other hand, to the geometrical axioms and theorems that can be used to validate the corresponding construction problem within Euclidean Geometry theory. Hence, the semiotic potential of the *Cabri* environment resides in the relationship between the meaning emerging from the use of its virtual drawing tools for solving construction problems controlled by the dragging test, and the theoretical meaning of a geometrical construction as it is defined within Euclidean Geometry in relation to a given set of axioms.

Exploiting this semiotic potential, of the *Cabri* artefact became the key pedagogical assumption inspiring a long-term teaching experiment. The pedagogical plan was designed following the structure of a Didactic Cycle (for details see Mariotti 2000, 2001). The teaching sequence consisted in activities involving the use of the artefact and semiotic activities aimed at individual and social elaboration of signs (Bartolini Bussi and Mariotti 2008; Bartolini Bussi this volume).

Activities in the Computer lab consisted primarily of a construction task, which asked students:

---

[1] Actually a DGS provides a larger set of tools, including for example "measure of an angle", "rotation of an angle" and the like, which implies that its whole set of possible constructions is larger than that attainable only with ruler and compass, (see Stylianides and Stylianides 2005, for a full discussion).

1. to produce a *Cabri* figure corresponding to a Geometric figure;
2. to write the description of the procedure used to obtain the *Cabri* figure;
3. to produce a justification of the "correctness" of the construction.

Thus, the task was composed of two types of requests, the former corresponding to acting with the artefact, the latter to reporting on such actions through written text, which consisted of both describing and commenting on the procedure carried out. The request of *justifying* the solution made sense with respect to the *Cabri* environment, corresponding to the needs not only of validating one's own construction but also of explaining and understanding why the figure on the screen passed the dragging test.

After reporting, the students compared their different solutions in collective discussions that became true "Mathematics Discussions" (Bartolini Bussi 1998; Mariotti 2001), focused on the evolution of the meaning of the term *construction*. At the beginning, "construction" made sense only in the field of experience of *Cabri*, that is in relation to using particular *Cabri* tools and to passing the dragging test. Later, its meaning slowly evolved to include the theoretical meaning of geometrical construction.

Such evolution could be accomplished exploiting the correspondence between Euclidean Axioms and specific *Cabri* tools and their modes of use. Starting from an empty menu, under the guidance of the teacher, the students discussed the choice of the appropriate tools as well as a set of Construction Axioms constituting the first core of Geometry Theory. Then, as long as new constructions were produced, the corresponding new theorems were validated and added to the Theory. In a parallel process of evolution, pupils participated, both in the development of a Geometry system and in the enlargement of a corresponding *Cabri* menu. In so doing, they not only appropriated the new theorems but also became aware of how Theory develops. Results of longstanding teaching experiments attested to the emergence of intermediate meanings rooted in the artefact's semantic field, and their evolution into mathematical meanings consistent with Euclidean Geometry (for details, see Mariotti 2001).

The present teaching experiments were designed, implemented and repeated for several years. Our analysis of our data led us to reflect upon the *Cabri* artefact's semiotic potential and, consequently, to refine the epistemological analysis related to what we had generically called a theoretical perspective. Thus, we achieved a more articulated description of the semiotic relation linking the use of particular tools in *Cabri* and the mathematical meanings related to them. In particular, classroom experiences highlighted the importance of rooting the sense of proof in the sense of theory. The constrained world of the DGE was effective in developing and interlacing these two meanings. In a DGE, using any single tool mediates the idea of applying an Axiom, while the set of available tools mediates the meaning of Theory, its conventionality and its evolutionary nature. Moreover, exploiting the possibility of personalizing the menu by allowing students to select the tools to be used made it possible for the students to experience the establishing and developing of Geometry Theory. The epistemological considerations arising from classroom observations, in particular the importance of focusing on the developmental dimension of a Theory, led us to further elaborate our educational goal and its epistemology.

12.4 Theoretical and Meta-Theoretical Considerations

12.4.1 A Didactic Definition of Theorem

In the field of mathematics education, we are used to the current literature considering the issue of proof in itself. This habit, although comprehensible with respect to mathematical practice, reveals its limits when one takes an educational stance. Generally speaking, it is impossible to grasp the sense of a *mathematical proof* without linking it to two other elements: a *statement* and an overall *theory*; that is, a proof is a proof when there are both a statement which it supports and a theoretical framework within which this support makes sense.

This concept of *theoretical validation*, which becomes automatic and unconscious for the expert, may be difficult for novices to grasp. However, remaining ignorant of this way of thinking and its complexities will only cause them more problems. In particular, the confusion between an *absolute* and a *theoretically situated* truth, corresponding to the two main functions of proof – explication and validation – may have serious consequences (for full discussion, see Mariotti 2006).

Thus, in order to express the contribution of each component involved in a theorem, we introduced the following characterization of a Mathematical Theorem, where a proof is conceived as part of a system of elements:

> The existence of a reference theory as a system of shared principles and deduction rules is needed if we are to speak of proof in a mathematical sense. Principles and deduction rules are so intimately interrelated that what characterises a Mathematical Theorem is the *system of statement, proof and theory* (Mariotti et al. 1997, 1, p. 182).[2]

In traditional school practice, the last component of the Theorem, the theory within which the proof makes sense, is largely neglected. Except for the case of Geometry, the theoretical context in which theorems are proved normally remains implicit. This is often the case, for instance, in Calculus courses and textbooks, where theorems are proved but very rarely is the axiomatic reference system explicitly stated.

It is important to remark that what is shortly referred to as Theory has a twofold component. First, Axioms and already-proved Theorems constitute the means of supporting the single steps of a proof; second, meta-theoretical rules assure the reliability of the specific way to accomplish this support, governing how Axioms and Theorems belonging to a Theory can be used to validate a new statement.

Actually, as Sierpinska clearly pointed out, acting at a meta-theoretical level constitutes the very essence of a theoretical perspective:

> [T]heoretical thinking is not about techniques or procedure for well-defined actions, […] theoretical thinking is reflective in that it does not take such techniques for granted but

[2]This definition has been widely used and further elaborated, generating subsequent interpretation models for both conjecturing and proving (Mariotti 2006; Pedemonte 2002; Mariotti and Antonini 2007; Antonini and Mariotti 2008).

> considers them always open to questioning and change. [...] Theoretical thinking asks not only, *Is this statement true?* but also *What is the validity of our methods of verifying that it is true?* Thus theoretical thinking always takes a distance towards its own results. [...] theoretical thinking is thinking where thought and its object belong to distinct planes of action (Sierpinska 2005, pp. 121–23).

In the school context, the complexity of this meta-theoretical level seems to be ignored[3]. Schools commonly take for granted that students' way of reasoning is spontaneously adaptable to the sophisticated functioning of a theoretical system. Thus, not much is said about meta-theory, in particular about deduction rules and their functioning in the development of a Theory.

There are at least two aspects of acting at a meta-level that teachers need to make explicit: the acceptability of some specific deductive means, and the fact that no other means, except those explicitly shared, is acceptable. Leaving these two aspects implicit, teachers give students no access to any control on their arguments; the control remains completely in the teacher's hands, resulting in students' generally feeling confused, uncertain and incomprehending.

12.5 Example 2: a Microworld for Algebra Theory

The second example illustrates the key elements of a research project to design a microworld that could offer a semiotic potential consistent with our epistemological and didactic analysis. In other words, we focused on the need for an environment where the use of specific tools could contribute to the evolution of the meaning of Theorem as the unity of the three components Statement, Proof and Theory. First, I will briefly explain in what sense I will speak of Algebra as a Theory and then I will illustrate how we designed the microworld in order to provide tools for semiotic mediation of the idea of Theorem in relation to Algebra Theory.

12.5.1 Algebra as a Theory

Since antiquity, Geometry has been considered a prototype of theoretical systematization of mathematical knowledge, the archetype of what in modern terms is called an Axiomatic. By contrast, Algebra found its systematization relatively late in history. Moreover, there is no tradition of a theoretical approach to Algebra at the pre-university level, where the study of Algebra is often reduced to its operative aspects of "symbolic calculation", thus neglecting any relational interpretation of this new way of calculating and hence arousing no suspicion that this part of Algebra might be a Theory.

---

[3]An exception is mathematical induction, which is explicitly treated, and to which a specific training is devoted. However, very rarely is mathematical induction presented in comparison to other modalities of proving, which are commonly considered natural and spontaneous ways of reasoning.

In fact, symbolic calculation can be interpreted within an Algebra theory of equivalence that originates from the numerical context but achieves a new interpretation as soon as some basic equivalence relations are stated as axioms.

Within the numerical context, we can state an equivalence relation between two numerical expressions. These expressions can be defined as equivalent if, and only if, the respective computations yield the same result. On this basis, the equivalence relation can be extended to all similar algebraic expressions.

Substituting any expression or sub-expression by an equivalent one makes it possible to operate on symbolic expressions preserving the equivalence. Extending the original meaning of "calculation" from the domain of numbers to that of algebraic expressions, what is usually called algebraic or symbolic calculation consists in transforming an algebraic expression into a new, algebraically equivalent one.

In the numerical context, the basic properties of operations – for instance, the commutative property of addition or the associative property of multiplication – express the equivalence of two numerical expressions, conceived as computing procedures. In the numerical context, these properties do not play any operative role; they state trivial truth, but are not directly employed to achieve the computation.

On the contrary, within the algebraic context, the operations' properties assume an operative role; they become *rules of transformation*. The chain of equivalence that originates from the subsequent application of these rules finally transforms any symbolic expression into an equivalent one.

In other terms, the set of equivalences, stating the basic properties of addition and multiplication, may function as an axiomatic system for a local Algebra Theory, within which *symbolic calculation* can be interpreted as a *proving process*, validating the equivalence between two algebraic expressions (Cerulli 2004).

Unfortunately, to discuss why such a theoretical approach may contribute to developing an effective alternative to the traditional approach to Algebra (Cerulli and Mariotti 2002; Kieran and Drijvers 2006) lies beyond the scope of this paper[4]. Here, I concentrate on how we tried to design a microworld affording semiotic potential with respect to our theoretical perspective on algebraic symbolic calculation.

12.6 Reconstructing the Semiotic Potential

Starting from the analysis above, we planned to design a prototype microworld that could offer tools of semiotic mediation for developing an Algebra Theory. Without becoming too technical, I will focus on the general principles inspiring the design,

[4] See the current literature. Using the *operational-structural* terminology of Sfard (1991), one can say that the operational character transforming algebraic formula and expressions shows to be persistent, while the absence of "structural conceptions" appears evident (Kieran 1992, p. 397). On the contrary, a structural conception becomes crucial in order to grasp the meaning of "symbolic calculation", in particular if one considers the change that the term 'calculation' has to achieve when passing from the numerical to the algebraic context.

explaining how we use epistemological and cognitive analysis was used to identify key features of the microworld.

To make semiotic mediation possible, meant reproducing the complex relationship among the different mathematical meanings related to the notion of Algebra Theory in a consistent way. Thus, acting on the objects in the microworld would generate meanings that could be related to the notions of Algebra Theory and of Theory in general.

A formula stating the equivalence between two algebraic expressions constitutes the generic statement of our Theory; the substitution of an expression with an equivalent one constitutes the basic deduction rule, through which any equivalence may be derived from another according to the transitive property of equivalence. Accordingly, the basic elements of the microworld, *L'Algebrista* (Cerulli 2002) were algebraic expressions and the basic actions on them could be accomplished through specific commands represented by icons on a tool bar "Buttons" to be activated by a click of the mouse. The Buttons are grouped in a toolbar and identified by icons representing algebraic properties, each corresponding to the statement of an axiom (see Fig. 12.1).

The mode of use (utilization scheme, in Rabardel's 1995, terminology) of any Button is very simple: after a formula is selected the click on the Button results in the substitution of that formula with the corresponding one. Each Button corresponds to one of the basic equivalences, stating the properties of addition and multiplication, constituting the basic set of *Axioms* to start with[5]. As a consequence, the microworld offers elements referring to *any single axiom* and to the application of the basic *deduction rule*. Furthermore, transforming any algebraic expression into another one by using the Buttons, corresponds to proving the equivalence of expressions within an Algebra theory: so, any transformation chain refers to a proof and, once proved, any equivalence refers to a Theorem.

The specificity of acting within a theoretical domain is explicitly represented by the action of "entering" the microworld by the command *Insert Expression* (Ita. *Inserisci Espressione*). This command initializes the application. The status of the selected expression changes: from being a string of characters, it becomes an object of the microworld on which the user can acte through the available Buttons. When an expression is *inserted*, its new instance incorporates some changes: every multiplication is represented with a dot ("•"), so either stars ("2*3") or spaces ("a 2") are substituted with a dot ("2•3 + a•2"); every subtraction is transformed into a sum, so expressions like "2–3" are substituted with "2 + (−3)"; analogously, every division is transformed into multiplication. *L'Algebrista* does not know subtraction and division: this follows from our precise didactical choice to allow pupils to work in a "commutative environment".

Figure 12.2 shows an example of a transformation procedure. Once introduced into the microworld, the expression is transformed using the Button of Commutative

[5] For instance, the statement a + b = b + a. For brevity reasons, I will not enter into details in the description of axioms and definitions of the Theory. I would rather concentrate on the meta-theoretical aspects.

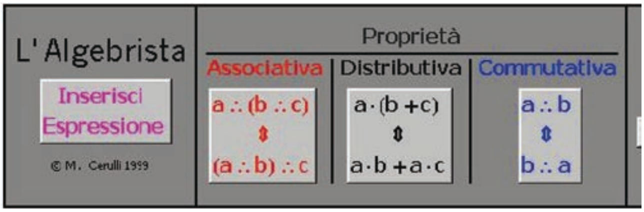


Fig. 12.1 The toolbar for *l'Algebrista*

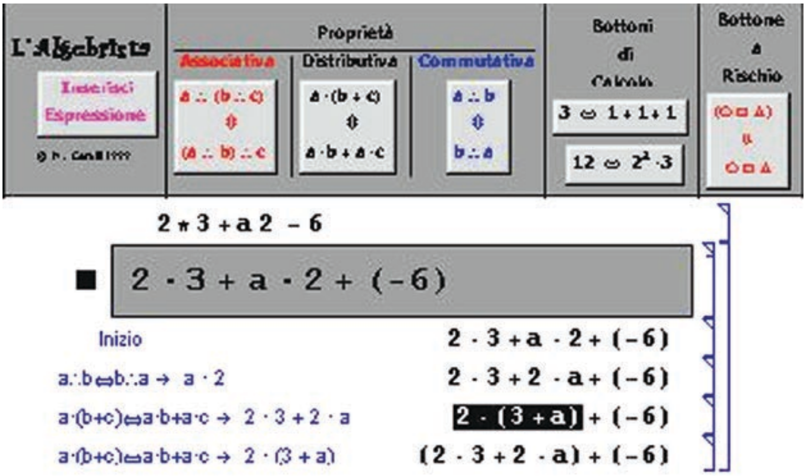


Fig. 12.2 The user writes the expression to work with («2 * 3+a 2−6» in the example), then, after selecting it, clicks the button *Inserisci Espressione*. Thus *L'Algebrista* creates a new working area where the Buttons are active. The trace of the buttons used is displayed in *blue*

property of addition and the Button of Distributive property. This corresponds to the transformation according to the corresponding Axioms in the Algebra Theory.

Acting in the microworld requires the user to become aware of the property that is to be used at each single step. Hence, this acting offers the opportunity of becoming aware of the basic deductive rule, usually implicit, that leads one to transform one expression into an equivalent one. An active experience of the axioms and the deductive rules that are in play when a symbolic calculation is performed generates a rich system of meanings referring to algebraic calculation as a deduction chain within a Theory. On the one hand, the constraints defined by the Buttons available in the tool bar correspond to the constraints defined by the axioms available in the theory. On the other hand, the effect of the commands on the expressions corresponds to the effect of the deductive rule of substitution. A trace of the deductive steps is displayed on the screen when the commands are activated and the sequence of transformation progresses step-by-step (Fig. 12.2).

In sum, the semiotic potential of the designed microworld is based on the following interpretation of some of the microworld's elements in terms of mathematical meanings:

1. Expressions in *L'Algebrista* refer to algebraic expressions;
2. Buttons/icons refer to axiom statements and definition statements of an Algebra Theory;
3. The functioning of Buttons commands refers to the application of axioms according the basic deductive rule;
4. Transforming an expression, using the available commands refers to proving within the stated Algebra Theory.

Taking into account these correspondences, we designed a set of tasks to be accomplished in the microworld in order to make students' use of the artefact and personal meanings emerge. The main task consisted of comparing two or more expressions: students were asked to establish whether the expressions were equivalent or not and in either case prove it. On the basis of the functioning of the microworld, the students collectively elaborated the mathematical meaning of proving an equivalence relation by a sequence of applications of the axioms. As long as the corresponding Algebra Theory is collectively built, symbolic calculations enlarge its meaning, including the meaning of proving process for an equivalence relation between two expressions.

Figure 12.3 shows an example of students' solution to a comparison task. In order to illustrate how much the meaning of proving has become detached from the microworld, I selected an example concerning a paper-and-pencil task. The student,

I think the 1st and the 3rd are equivalent, but not the 2nd, because applying the properties they become equal, while the 2nd does not.

I applied the distributive property.

I applied the distributive property on these two pieces.

I added the two equal terms **–a*b –a*b** and I cancelled its result with its opposite obtaining **0** for the 1st theorem.

I cancelled also **+b*b** with its opposite and as it was **–2b*b** I obtained **–b*b.**

At this point the 3rd expression is equal to the 1st expression.

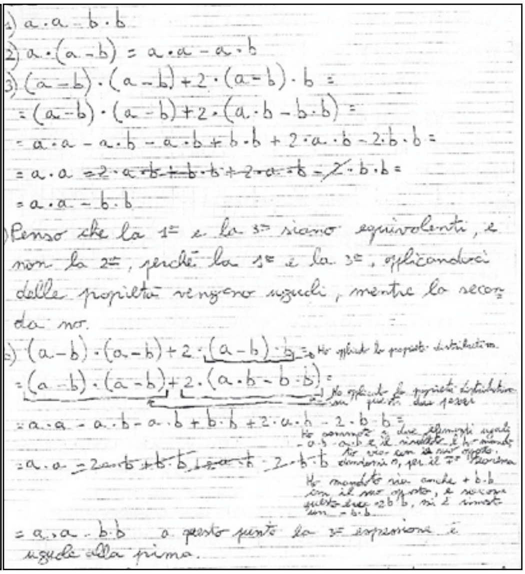


Fig. 12.3 Exemplar of student's solution of a comparison task. A translation is reported on the left

Fig. 12.4 The meta menu

asked to evaluate the equivalence between different expressions, first checked the equivalence by calculation. Once he made his conjecture, he provided a proof, which is based on the properties of the operations (i.e. Axioms) and a Theorem.

Although not directly mentioned, the artefact in which the meanings are rooted is clearly evoked in the text produced. In particular, underlining the part of the expression that has to be transformed evidently derives from the selection mark used in the microworld, while the subsequent application of axioms and theorems is described step-by-step as takes place in the microworld. (More examples from specific teaching experiments using of *L'Algebrista* can be found in Mariotti and Cerulli 2001; Cerulli 2002; Cerulli and Mariotti 2002, 2003.

The text in Fig. 12.3 clearly shows the distinction between the phase of conjecturing and the phase of proving. In the argument supporting the conjecture, the student refers to the properties generically, but in the proving process the student makes explicit the single properties used and the specific theorem applied (the 1st Theorem, as the student writes). Making the distinction between the roles played by axioms and by theorems of the Theory constitutes a crucial point in the evolution of a theoretical perspective. The following section discusses the semiotic potential of the designed artefact in this respect.

12.7 Development of the Theory

The exploitation of the artefact's semiotic potential is based on exploiting the correspondence between the activities in the microworld and their counterparts in the Algebra Theory. A new equivalence produced by transforming an expression into another one through sequential applications of the available Buttons, can be interpreted as proving a new statement about the equivalence of two expressions in the Algebra Theory. That means that the Theory now includes a new Theorem that can be used to prove new statements.

The act of enlarging the Theory by assuming new means of proving constitutes a delicate point in the development of a theoretical perspective: as soon as a statement is proved, its new status within the elements of the Theory has to be recognized, as does the fact that it can be applied in the same manner as the axioms. Coping with this delicate issue suggested that we could design an extension of the microworld providing semiotic potentialities with respect to the mathematical meaning of *change of theoretical status for a statement*. We gave this new environment and its management menu the clearly evocative name *Meta Menu* (see Fig. 12.4).

12.7.1 The Meta Menu

The new environment was designed to offers tools to be used to act on the set of the available Buttons, that is on the set of Buttons corresponding to the Theory itself.[6]

The first tool is called *Theorem maker* (Ita. *Il Teorematore*). It activates an environment where the user can create new Buttons. Once created, a new Button can be used to transform an expression into another. Any new Button can be selected and used, in addition to the others, to transform expressions in L'Algebrista, according to the basic substitution rule.

Moreover, a second tool designed in the Meta menu, *Palette Personalizer* (Ita. *Personalizza Palette*), allows a new Button to be included in a separate menu (called Palette) that will appear next to the main menu.

We designed both the *Theorem Maker* and the *Palette Personalizer* after an a priori analysis of their semiotic potential, as related next.

12.7.2 The Theorem Maker

Consistent with the previous analysis, we designed the tools of the *Meta Menu* to provide a counterpart to the mathematical notions of *changing the status of a statement* and *enlarging the Theory*.

The change of status of a statement finds a referent in the functioning of the *Theorem Maker*: when a new Button is to be created, the user has to enter the *Theorem Maker* environment and re-write the statement according to specific formatting constraints.

A statement's attaining the status of *Theorem* corresponds to its overcoming the context in which it was produced. In other terms, the equivalence relationship has to acquire the role of a transformation scheme to be applied through the substitution rule. The move from the standard environment, where expressions are treated, to

[6]Recall that by *Theory* we mean set of axioms, definitions and theorems that have a counterpart in the collection of Buttons available in the microworld.

the *Theorem Maker* environment, where Buttons are created, represents the move from interpreting a formula as an equivalence between algebraic expressions to interpreting a formula as a new transformation scheme.

Gaining further generality that allows the use of a formula according the substitution rule requires that the domain of interpretation of a letter be extended from the domain of numbers to the whole domain of algebraic expressions. The need for different levels of interpretation for an algebraic expression finds a counterpart in the features and functioning constraints of the *Theorem Maker's* environment.

The editor offers different fonts for the editing of a new Button (see Figs. 12.5 and 12.6). Each font corresponds to a different level of generality to be assigned to the formula when it will be used after activating the Button. If the standard font is used, the formula will be used as is: the substitution will be possible not only if the formula's structure is recognizable but also if the formula contains the same single letters. On the contrary, if the special font is used, the formula will be interpreted at its highest level of generality. In other words, in the *Theorem Maker* environment, the act of assigning such generality to a formula is represented by the use of special editing Buttons: the selection of a font corresponds to the choice of the level of generality.

Fig. 12.5 The theorem maker environment

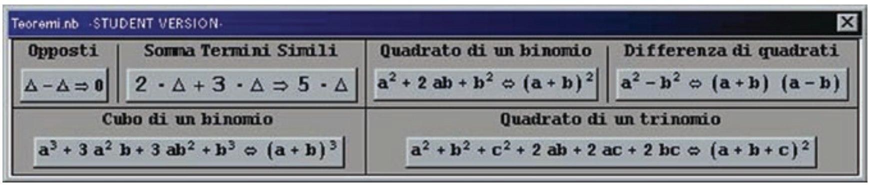


Fig. 12.6 A palette of theorem buttons, as created by our pupils during a teaching experiment

12.7.3 The Palette Personalizer

The act of enlarging the Theory – that is, increasing the set of "theoretical means" available for proving – has a counterpart in the *Palette Personalizer* (see Fig. 12.7). In this environment, the user can define a new menu that will appear next to the main menu active in any predefined *Theory* (*"Teoria #*"). The user can group and place new Buttons within a particular Palette, to which the user can assign a name. The user can create different Palettes, for instance a single Palette for each Button, but no new Button can be added to any pre-defined *Theory*. The separation between the bar of commands corresponding to the set of *Axioms* and any new Palette was designed with the aim of expressing the difference between basic assumptions and new acquisitions.[7]

The location of a Button in the pre-defined menu of a *Theory* represents its status as an *Axiom*, while its location in a Palette represents its status as a *Theorem*. Thus, the organization of a palette of Buttons and their constraints corresponds to a classification of the statements according to their status in an *Algebraic Theory*.

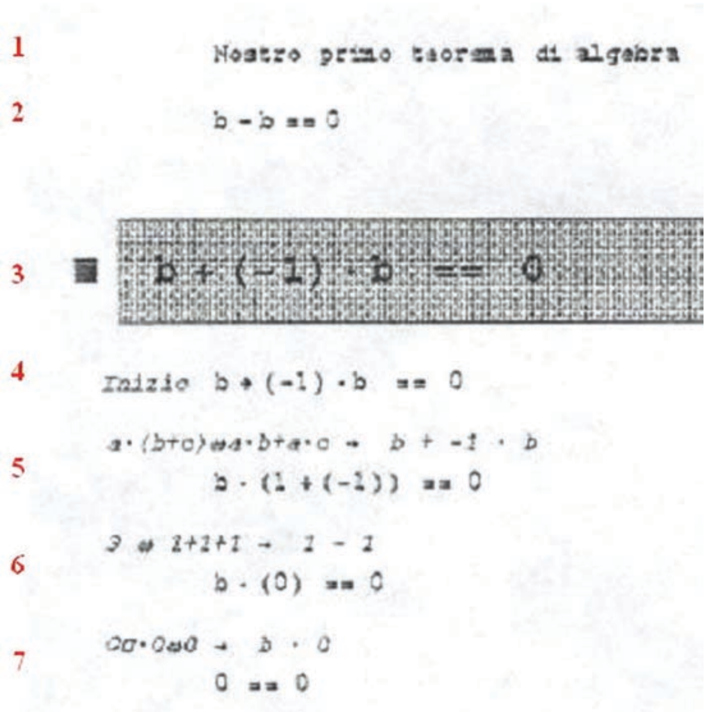


Fig. 12.7 Marco (9th grade) the proof of the first theorem

[7]Because of its reference to specific sets of axioms, any pre-defined Theory cannot be modified.

12.8 The Mediation of the Meta Menu

12.8.1 The Case of the First Theorem

The Palette of a predefined *Theory* contains no rule of symbolic calculation except those related to the basic properties of the sum and the multiplication. We designed one of the first activities proposed to the pupils in order to make them face the need for summing monomials, particularly, for cancelling two "opposite monomials", when there were no corresponding buttons in *L'Algebrista*. The following example is drawn from one of the teaching experiments carried out after the realization of the prototype microworld *L'Algebrista* (More details on this example and the teaching experiments can be found in Cerulli 2002, p. 120 ff).

The students carried out the computations on paper. However, during the collective discussions the impossibility of the realization of the corresponding transformation within *L'Algebrista* emerged; consequently there also emerged the impossibility of accepting such computations as proofs. At this point, the teacher suggested that the students enter the microworld and look for a proof. Correct chains of transformation were obtained (one is shown in Fig. 12.7, where the numbers of the lines are introduced for the reader's convenience). Marco (Grade 9) entered the microworld, through the command *Insert expression*; as a consequence, the expression was re-written as "b + (−1)·b==0". Then Marco applied the available Buttons until the last line (7) presented the identity 0==0. At this point, the transformation process stops and it can be stated that the initially questioned equivalence actually holds. The subsequent collective discussion allowed pupils to share different chains of transformations leading to this equivalence and finally to agree that the new, proven statement could be utilized as a step in a chain of transformation for expressing this particular status of the new equivalence. The teacher then introduced the mathematical term *Theorem*. Because of its importance the students decided to make this *Theorem* available as a Button of the microworld, using the Meta Menu.

A new Button was created and added to the set of Buttons available.

The long discussion, as well as the interest created by the proof of this equivalence, led pupils to perceive the *Theorem* as the result of collective endeavour; pupils often refer to it as "our first *Theorem*".

The following protocol (Fig. 12.8) shows how Marta (Grade 9) used this new *Theorem*. Marta wanted to prove the equivalence between the expression on Line 1 and the expression on Line 3. As she clearly explains, the first step is achieved by using an *Axiom* while the second step is achieved by using the first *Theorem*, which she calls "our *Theorem*" (Ita.: "*nostro Teorema*").

The insertion of a new button in *L'Algebrista*, corresponds to the enlargement of the *Theory* by a new *Theorem*; consequently, not only can one use it to prove new equivalences but also it provides a shortcut in the proof. The following example shows the idea of enlarging the *Theory* can emerge.

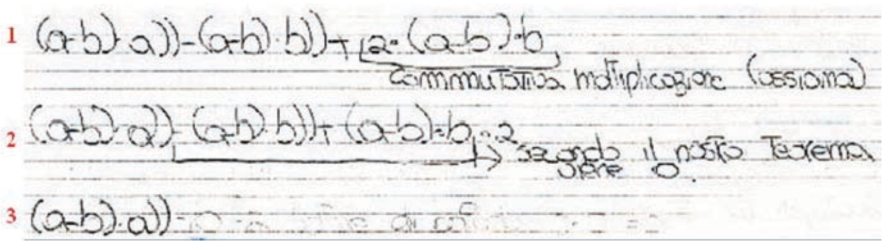


Fig. 12.8 Marta's proof: line 1, Marta writes "commutative [property] of multiplication (*Axiom*)"; line 2, Marta writes "according to our *Theorem* this becomes 0"

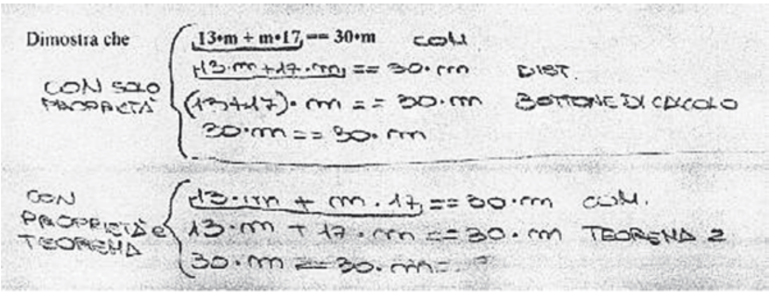


Fig. 12.9 Elena's proofs. She provides two proofs. At each step of the chain, Elena indicates what *Axiom* or *Theorem* she used to transform the expression: "*com*" stands for commutative property; "*dist*" stands for distributive property; the "button of computation"(ita. "*bottone di calcolo*") is a command that calculate only sums of numbers

We asked pupils to prove the equivalence between two expressions representing the same two monomials. After this activity with *L'Algebrista* and a collective discussion, the class agreed to add the *Theorem* of the sum of monomials to the *Theory* and the corresponding Button to *L'Algebrista*. Then we asked the students for a written proof of the equivalence: $13 \bullet m + m \bullet 17 == 30 \bullet m$.

Although the task did not explicitly ask for more than one solution, Elena (Grade 9) produced two different proofs of the given statement (Fig. 12.9). She achieved the first proof by using axioms, as she says "only with properties" (ita. "con solo proprietà"); she produced the second proof by also using a theorem, which she calls "Teorema 2" (following the social practice in the class of naming theorems according to the chronological sequence of their official introduction into the Theory). At each step of the chain, Elena indicates what axiom or theorem she used to transform the expression. As in the previous student protocols, the terms used, and the underlining of the parts of the expressions to be transformed, provide a clear trace of actions in *L'Algebrista*.

12.9 Conclusions

The examples discussed here, representing two different phases of a long-term research study, centre on the use of two artefacts as tools of semiotic mediation (Bartolini Bussi and Mariotti 2008). The theoretical framework considers any artefact in respect to a double relationship: first, the relation of the artefact to the meanings emerging from its use to solve a task and second, the relation of the artefact to the mathematical meanings evoked by that use. We call this double semiotic relationship the *semiotic potential* of an artefact with respect to particular mathematical knowledge. Furthermore, we assume that the teacher can exploit this double link to achieve educational goals related to the evoked mathematical meanings. This construct of semiotic mediation showed its effectiveness in firmly framing our analysis to assure the integration of the epistemological, cognitive, and didactic perspectives.

The discussion here shows how to coordinate the epistemological and cognitive perspectives concerning the mathematical notion of proof and the specific artefact used in the classroom. Beside the rich source of precedents in history (cf. the discussion by Bartolini Bussi this volume), new technologies seem to provide powerful means to shape artefacts to fit this specific purpose.

The first example addressed the issue of identifying the semiotic potential of a particular artefact, the DGE *Cabri*. The analysis of its semiotic potential carried out a posteriori, included discussion of how the functionalities of *Cabri* tools in solving a construction task could be referred to theoretical aspects of Geometrical Construction and consequently, how *Cabri* offers a semiotic potential to introduce pupils to proof.

The second example addressed the issue of designing a particular artefact as a tool of semiotic mediation with the specific educational goal of developing the idea of *Algebra Theory*. In other words, in contrast to what we had done in the case of *Cabri*, we developed our analysis of this second artefact's semiotic potential a priori, coordinating the identification of the key elements related to the mathematical meanings to be fostered with identification of the key features of the microworld to be designed. The possible correspondence between the use of a set of commands and the application of the axioms of a theory inspired the design of a symbolic manipulator, *L'Algebrista*.

The design of *L'Algebrista* exploited the parallel between commands and axioms, so that acting through commands in the microworld would directly correspond to proving within a Theory. In this sense we designed the domain of "transformations of expressions in *L'Algebrista*" to provide a semantic domain for the notion of *Theorem* as the triplet Statement, Proof, Theory. Furthermore, we designed the *L'Algebrista* environment to offer mediation tools for two crucial aspects of the functioning of a *Theory*: the idea of a statement's theoretical status as *Axiom* or *Theorem*, and the idea of *Theory* enlargement. Both these elements belong to the meta-theoretical level and, in spite of being so crucial for a genuine sense of Theory, are quite hard to access directly. However, the use of specifically designed environments, such as that in *L'Algebrista*, offers a semiotic potential that, properly exploited, can effectively support the development of such delicate and crucial meanings.

Acknowledgments Research funded by MUIR (PRIN 2005019721: Meanings, conjectures, proofs: from basic research in mathematics education to curricular implications).

References

Antonini, S., & Mariotti, M. A. (2008). Indirect proof: what is specific to this way of proving? *ZDM*, Berlin/Heidelberg: Springer, *40*(3), 341–344.

Arzarello, F. (2006). Semiosis as a multimodal process. *Relime*, *Vol. Especial*, 267–299.

Arzarello, F., & Bartolini Bussi, M. G. (1998). Italian trends in research in mathematics education: a national case study in the international perspective. In J. Kilpatrick, & A. Sierpinska (Eds.) *Mathematics education as a research domain: a search for identity* (Vol. 2, pp. 243–262). The Netherlands: Kluwer.

Arzarello, F., Oliverio, F., Paola, D., & Robutti, O. (2002). A cognitive analysis of dragging practices in Cabri environments. *ZDM Mathematics Education, 34*(3), 66–72.

Bartolini Bussi, M. G. (1998). Verbal interaction in mathematics classroom: a Vygotskian analysis. In H. Steinbring, M. G. Bartolini Bussi & A. Sierpinska (Eds.), *Language and communication in mathematics classroom* (pp. 65–84). Reston, Virginia: NCTM.

Bartolini Bussi, M. G. (this volume).

Bartolini Bussi, M. G., & Mariotti, M. A. (2008). Semiotic mediation in the mathematics classroom artefacts and signs after a Vygotskian. In L. English et al. *Handbook of international research in mathematics education*, LEA 2nd edn. (pp. 746–783).

Carpay, J., & van Oers, B. (1999). Didactical models. In Y. Engeström, R. Miettinen & R. Punamäki (Eds.), *Perspectives on activity theory*. Cambridge: Cambridge University Press.

Cerulli, M. (2004). Introducing pupils to algebra as a theory: L'Algebrista as an instrument of semiotic mediation. Ph.D Thesis in Mathematics, Università di Pisa, Scuola di Dottorato in Matematica.

Cerulli, M., & Mariotti, M. A. (2002). *L'Algebrista: un micromonde pour l'enseignement et l'apprentissage de l'algèbre. Science et techniques éducatives, vol. 9, Logiciels pour l'apprentissage de l'algèbre* (pp. 149–170). Lavoiser, Paris: Hermès Science Publications.

Cerulli, M., & Mariotti, M. A. (2003). Building theories: working in a microworld and writing the mathematical notebook. In *Proceedings of the 2003 joint meeting of PME and PMENA* (Vol. 2. pp. 181–188).

Cummins, J. (1996). *Negotiating identities: Education for empowerment in a diverse society*. Ontario, CA: California Association of Bilingual Education.

Healy, L., Hoelzl, R., Hoyles, C., & Noss, R. (1994). Messing up. *Micromath, 10*(1), 14–16.

Heath, T. (1956). *The Thirteen Books of Euclid's Elements*. New York: Dover.

Jones, K. (2000). Providing a foundation for deductive reasoning: students' interpretations when using dynamic geometry software and their evolving mathematical explanations. *Educational Studies in Mathematics, 44*, 55–85.

Kieran, C. (1992). The learning and teaching of school algebra. In D. A. Grouws (Ed.) *Handbook of research on mathematics teaching and learning*. N.C.T.M.

Kieran, C., & Drijvers, P. (2006). The co-emergence of machine techniques, paper-and-pencil techniques, and theoretical reflection: a study of CAS use in secondary school algebra. *International Journal of Computers for Mathematical Learning, 11*(2), 205–263.

Laborde, J.-M., & Bellemain, F. (1998). *Cabri Geometry II*. Dallas, Texas: Texas Instruments Software.

Laborde, C., & Laborde, J.-M. (1995). What about a learning environment where Euclidean concepts are manipulated with a mouse? In A. A. DiSessa, C. Hoyles, R. Noss & L. D. Edwards (Eds.), *Computers and exploratory learning*. Berlin: Springer.

Leont'ev, A. N. (1976 orig. Ed. 1964). *Problemi dello sviluppo psichico*. Editori Riuniti and Mir.

Luria, A. R. (1976). *Cognitive development its cultural and social foundations*. Cambridge, Massachusetts: Harvard University Press.

Mariotti, M. A. (1996). Costruzioni in geometria, su. *L'insegnamento della Matematica e delle Scienze Integrate, 19B*(3), 261–288.

Mariotti, M. A. (2000). Introduction to proof: the mediation of a dynamic software environment. *Educational Studies in Mathematics, 44*(1 and 2), 25–53.

Mariotti, M. A. (2001). Justifying and proving in the cabri environment. *International Journal of Computer for Mathematical Learning, 6*(3), 257–281.

Mariotti, M. A. (2002). Influence of technologies advances on students' math learning. In L. English et al. *Handbook of international research in mathematics education* (pp. 695–723) Lawrence Erbaum Associates.

Mariotti, M. A. (2006). Proof and proving in mathematics education. In A. Gutiérrez & P. Boero (Eds.), *Handbook of research on the psychology of mathematics education* (pp. 173–204). Rotterdam, The Netherlands: Sense Publishers.

Mariotti, M. A., & Cerulli, M. (2001). Semiotic mediation for algebra teaching and learning. In Maria van den Heuvel-Pnhuizen (Ed.) *Proceedings of the 25th PME conference* (Vol. 3, pp. 343–351). Freudenthal Institute, Utrecht University, The Netherlands.

Mariotti, M. A., Bartolini Bussi, M., Boero, P., Ferri, F., & Garuti, R. (1997). Approaching geometry theorems in contexts: from history and epistemology to cognition. In Erkki Pehkonen (Ed.) *Proceedings of the 21st PME Conference* (Vol. 1, pp. 180–195). University of Helsinki, Helsinki, Finland.

Martin, G. E. (1998). *Geometric constructions*. New York: Springer.

Olivero, F. (2002). The *proving process within a dynamic geometry environment*. PhD thesis, University of Bristol.

Pedemonte, B. (2002). Etude didactique et cognitive des rapports de l'argumentation et de la démonstration, *co-tutelle Università di Genova and Université Joseph Fourier, Grenoble*.

Rabardel, P. (1995). *Les hommes et les technologies – approche cognitive des instruments contemporains*. Paris: A. Colin.

Radford, L. (2003). Gestures, speech, and the sprouting of signs: a semiotic-cultural approach to students' types of generalization. *Mathematical Thinking and Learning, 5*(1), 37–70.

Sierpinska, A. (2005). On practical and theoretical thinking. In M. H. G. Hoffmann, J. Lenhard & F. Seeger (Eds.), *Activity and sign – grounding mathematics education. Festschrift for Michael Otte* (pp. 117–135). New York: Springer.

Stylianides, G. J., & Stylianides, A. J. (2005). Validation of solutions of construction problems in Dynamic Geometry Environments. *International Journal of Computers for Mathematical Learning*, *10*, 31–47.

Vinner, S. (1999). The possible and the impossible. *ZDM*, *99/2*, 77.

Vygotskij, L. S. (1978). *Mind in society. The development of higher psychological processes*. Cambridge, MA: Harvard University Press.

Part III
Experiments, Diagrams and Proofs

Chapter 13
Proof as Experiment in Wittgenstein[1]

Alfred Nordmann

Ludwig Wittgenstein famously declared that we should let the proof show us what was proved (e.g., PI II: xi, and PG II: V, 24). He also suggested that one can regard proof in two ways: namely, as a picture or as an experiment. In this paper I establish that, consequently, the proof also shows us in two different ways what is proved. This difference helps explain why interpreters of Wittgenstein's concept of proof have offered bewilderingly divergent accounts. However, the proposed reconciliation of these different interpretations poses a new problem for the philosophy of mathematics: Is it indeed the case that every proof can be regarded in both ways? Though he appears to take it for granted, Wittgenstein does not make this explicit or subject it to systematic questioning.

Briefly put, the two ways of regarding proof can be contrasted thus: On the one hand, a proof can and ought to be regarded as a picture that meets the requirement of being surveyable (Mühlhölzer 2005), as exemplified by a calculation on a sheet of paper. Here, what was proved serves as an identity-criterion for the proof; indeed, only the proof as a surveyable whole can tell us what was proved. On the other hand, a proof can be regarded as an experiment, necessarily so if one wants to understand the productive and creative aspects of proof. In analogy to scientific experiments, proof as experiment refers to the experience of undergoing the proof, as exemplified by *reductio ad absurdum* or negative proof.[2] Here, the conclusion of the proof does not add a conclusion to the premises but leads to the rejection of a premise and changes the domain of the imaginable. The proof shows us what was

A. Nordmann
Institut für Philosophie, Technische Universität Darmstadt,Darmstadt, Germanye-mail: nordmann@phil.tu-darmstadt.de

[1] This paper originated in an attempt to understand Wittgenstein's argument in the *Tractatus Logico-Philosophicus* – his "proof" that "there is indeed the inexpressible" (Nordmann 2005; TLP 6.522). An intermediary sketch appeared in a German web publication (Nordmann 2006). The present version benefited from a seminar on proof at Darmstadt Technical University (with Ulrich Kohlenbach and Johannes Lenhard) and from the workshop in Essen. However, as far as philosophy of mathematics is concerned, it still stands somewhere near the bottom of a steep learning curve.

[2] For the purposes of this paper, the terms *reductio ad absurdum*, negative or indirect proof will be used interchangeably.

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_13,

proved in that it implicates us in a certain experience at the end of which we see things differently: that is, we evaluate certain commitments, mathematical procedures or hypotheses differently and therefore, in a sense, live in a different world.[3]

If proof as picture is exemplified by written calculation and proof as experiment by *reductio ad absurdum*, the new problem for philosophy of mathematics comes to this: Can every proof be regarded as a calculation and as a *reductio ad absurdum*? Might one say, for example, that the discovery, establishment, and reenactment of a proof displays the experiential structure of a *reductio*-argument and leads one to see the world differently, but that the very same proof can be a picture written down in a surveyable manner for the validation of the proper logical relations between its various lines or propositions?

Given the heterogeneity of methods of proofs and their technical expansion far beyond individual human experience and surveyability, it might be neither feasible nor necessary to show that everything accepted as proof can indeed be regarded in both ways. Even Wittgenstein's suggestion that it holds for broadly shared normative conceptions of proof turns out to be challenging and fruitful enough. Hence, I will limit myself to establishing the complementary ways of regarding proof and, in particular, to explicating the oft neglected dimension of proof as experiment.

13.1 Proof as Picture

For the account of proofs as pictures, I need to merely refer the reader to Felix Mühlhölzer's exposition (2005). Mühlhölzer asks what Wittgenstein means when he demands that proofs be surveyable. He answers, in brief: Surveyability is a necessary condition for a proof being a proof[4]; it is a shared feature of proofs and pictures that permits reproducibility and an identity-criterion for what the proof is a proof of.[5] Taking the notion of proof as a picture literally (as Wittgenstein does), obviously implies that a proof is reproducible with certainty in its entirety: Rather than repeatedly "go through" the proof to see whether one can always reproduce its result, one can

[3] This complementarity has repercussions on a metamathematical level. Mathieu Marion points out that Wittgenstein had to rely on some doctrinal position and did rely on a constructivism of sorts: "There is no free lunch in these matters, those who think so do not know what is at stake" (Marion 2004: 221). Though the notion of proof as experiment relies on a moderate constructivism (see notes 10 and 14 below), the oscillation between proof as picture and proof as experiment indicates why Wittgenstein nevertheless did not have to commit to a foundational theory of mathematics.

[4] Mühlhölzer acknowledges that the later Wittgenstein was aware, of course, that many accepted proofs are not surveyable (2005: 58 f.). How, then, could Wittgenstein argue in RFM III, 2 that the non-surveyable figure of a proof only becomes a proof when a change of notation renders it surveyable? Mühlhölzer (and Wittgenstein) suggest that to consider something an identifiable proof is just to render it in such a notation. (See below on the availability of the identity criterion only within a surveyable picture or sufficiently rich notation.)

[5] Mühlhölzer thus puts "proof as picture" in the place of "proof as grammatical or linguistic rule"; to serve as a paradigm is one feature of the proof as picture. In contrast, accounts according to which proofs establish and modify linguistic rules or paradigms do not require the notion of a picture at all (Frascolla 1994). These latter accounts, however, are haunted by rule-following arguments and their attendant difficulties.

reproduce it by copying the picture wholesale or "once and for all" (RFM III: 22). When recreating certain initial conditions, natural scientists must wait and see whether the same thing happens every time. Not so when a mathematician copies a picture or a surveyable proof and obtains the initial set-up together with the result, "the proof must be capable of being reproduced by mere copying" (RFM IV: 41). Obviously, this sets proof as a picture apart from a scientific experiment: "To repeat a proof means, not to reproduce the conditions under which a particular result was once obtained, but to repeat every step *and the result*" (RFM III: 55). Reproducibility, in other words, is tied to contemporaneous visibility (Mühlhölzer 2005: 68): All the symbols are arranged on paper or a reel of film and one can reproduce this arrangement in a purely formal fashion, without relying on causal or temporal processes.

It is less easy to grasp how surveyability offers an identity criterion for proofs. Surely, it is not enough for proofs to merely "look alike" to be considered identical, especially since new notations can introduce transformations that allow us to see a sameness of proof in a difference of signs.[6] Mühlhölzer argues *ex negativo*: In order to "establish the identity of proofs at the foundational level, the procedures of our normal counting, or similar procedures, are necessary." In other words, one has to go beyond the foundational level to the proof as a sufficiently detailed picture in order to *see* identity. For example, one cannot establish identity for all proofs that are generated in the same way so that the type of generation of proof secures identity among tokens. Since a proof would be different if it had another result, one can determine identity only at the level of the tokens, the pictures themselves (Mühlhölzer 2005: 60, 80). So, even believing that something is proven by the application of some principles or rules, one can be convinced and convince others only by the surveyable picture that is produced through the application of these rules. No matter what stands "behind" our proofs, the proof thus becomes a proof only within a notational system that can show us what was proved.[7]

To be a proof a proof needs to be convincing, of course. This account of surveyability leaves open whether and when seeing is not only necessary but also sufficient to produce conviction. For this, one has to conceive seeing as an activity of sorts, whether the act of accepting the picture as a paradigm or the act of studying relations between symbols. Either way, we see not just the symbols but also what the symbols yield; that is, how symbols lead to other combinations of symbols (Mühlhölzer 2005: 72). Of course, this way of looking at symbols is how one looks at calculations.[8]

[6] If likeness or similarity were our guide, one might be stuck with the consequence that the color of the ink might be a criterion for the identity of a proof. Also, "looking alike" does not suffice, because it may take a kind of inferential procedure to ascertain that two sequences of strokes indicate the same number (Mühlhölzer 2005: 81); where such inferences are needed, the criterion of surveyability is not fulfilled. That's why the use of numerals can yield a proof where the use of strokes in the place of numerals produces merely a non-surveyable "figure of a proof".

[7] Here is one sense in which we can let the proof (as picture) show us what was proved. Inversely, if a proof is to induce a modification of concepts, rules, or paradigms, this is explained by the substitution of one picture for another. Especially, Wright (1980, 1991) adopts this replacement account of mathematical change.

[8] Mühlhölzer does not dwell on the fact that calculations are the standard case of proofs as surveyable and reproducible pictures but he appears to suppose as much (e.g. 2005: 72, 83 f.). Calculations play a central role especially in the interpretations of Wrigley (1993) and Frascolla (1994, 2004).

13.2 Proof as Experiment

According to Mühlhölzer, when he relates proof and picture Wittgenstein:

> alludes to a beautiful thought which he has already developed in Part I (and which he will develop further in Part VI) of the *Remarks*: that the real, temporal process of proving a mathematical theorem may very well be comparable to an experiment, but that the proof itself rather resembles the *picture* of such an experiment, in which the experiment is frozen, as it were, into something nontemporal. (Mühlhölzer 2005: 68)

Here, Mühlhölzer notes a complementarity overlooked by most readers of Wittgenstein's *Remarks*, many of whom take the consideration of experiments merely as a way to dissociate mathematics from empiricism and natural science: It is thought to be characteristic of mathematical proof that it is not an experiment (Frascolla 1994, Ramharter and Weiberg 2006; Weiberg 2008). Even Mühlhölzer describes that complementarity in rather weak terms. Although his paper explores Wittgenstein's suggestion that "the proof *is* a picture," the quoted passage speaks of proof *resembling* a picture and being *comparable* to an experiment. By stressing that a proof *is* a picture and also that it *is* an experiment, I would like not only to highlight that these are complementary aspects of proof for Wittgenstein but also to show that the complementarity is necessary.[9] This necessity is not due to foundational considerations, a theory of proof or the like, but arises simply from the fact that mathematicians move about in notational systems.[10] That they creatively produce a proof (experiment) and render it as a configuration of symbols (picture). To their readers, the proof appears as something to be gone through and re-enacted (experiment) or as something to be surveyed and seen (picture). Any movement in a notational system is an experience unfolding in time (experiment) and yields at any given moment a formal structure in space (picture). By enacting and reenacting proofs as experiments, mathematicians effect the modification of concepts; by surveying and

[9] Indeed, it would appear that Mühlhölzer requires a stronger notion of complementarity in order to arrive at a full account as sketched in note 4 above: Proofs as experiments are not surveyable and as such only figures or schemes of proof; they become surveyable and thus properly "proofs" only as they are rendered in an appropriate notation.

[10] This emphasis on notational systems places Wittgenstein in the proximity of formalism. To the extent, however, that the movements within a notational system go beyond the application of formal transformation rules, Wittgenstein also moves beyond formalism (Mühlhölzer 2008; Floyd 2008). Of the various extant reconstructions of Wittgenstein's philosophy of mathematics, the one proposed here is closest in letter and spirit to one of the earliest ones (Klenk 1976). Later interpretations tend to hold Wittgenstein answerable to question of realism vs. anti-realism, Platonism vs. formalism, constructivism, or empiricism, and to Kripke's discussion of rule-following. In contrast, see Klenk (1976: 124–126): "Wittgenstein is neither a finitist nor a radical conventionalist; he is willing to admit the full spectrum of mathematical techniques and results, and he has been able to do so without giving up the fundamental properties of mathematical propositions: their objectivity and necessity. […] Since Wittgenstein rejects the idea that mathematical statements refer to mathematical objects, for him these statements carry no ontological commitment at all, and he is thus able to enjoy the best of both worlds: the full range of classical mathematics, but without the ontological burden that usually goes with it."

beholding the proof as picture, they ascertain its certain and complete reproducibility and identity. This duality underwrites the oft-cited passage in which Wittgenstein compares the mathematician to an inventive garden architect who modifies the landscape to create the formal paths and tracks that the viewer then simply follows (RFM I: 167).[11]

In this duality of aspects, proof as picture and proof as experiment are strictly separate: " "The proof must be surveyable" really means nothing but: The proof is no experiment" (RFM III: 39). When a proof is surveyable, we see the entire garden path from beginning to end; whereas, in an experiment and in going through a proof, we may question whether the path will reliably take us from beginning to end (RFM I: App. 2, 2). "And thus I might say: The proof doesn't serve me as experiment but as the picture of an experiment" (RFM I: 36). Here again, Wittgenstein asserts surveyability as a necessary condition for proof. He makes clear, however, that this is not the whole story. If proof is a picture of an experiment, then proof is first of all an experiment that is distinguished from other experiments by becoming transformed into a picture. This transformation is possible because the proof is a movement among signs that culminates in a pictorial configuration of these signs.[12]

But what kind of movement among signs is a proof, and how does the recognition of this experimental movement account for the creativity and productivity of proof or for the way in which it effects a modification of concepts? Wittgenstein elucidates this primarily in reference to *reductio* arguments or negative proof. To the complementarity of proof as picture and proof as experiment therefore corresponds the complementarity of calculation and negative proof. Calculation exemplifies the proof as a picture or paradigm that works to establish identity, definition, and substitution. The *reductio* argument or negative proof exemplifies the proof as an experiment that probes commitments and establishes the connection between inference and decision. Yet, it is misleading to say that we look at *reductio* arguments differently than we look at calculations and their manner of yielding results. More appropriately, we should say that we don't *look* at them as *reductio* arguments or negative proofs at all; instead, we should say that we rehearse, enact, or go through *reductio* arguments: We undergo a negative proof just as we undergo an experience.

---

[11] Wittgenstein may be referring to just this duality when he speaks of experiment (invention, creation, experience) and calculation (survey of the tracks that have been laid) as the poles between which human activities move (RFM VII: 30). Klenk also speaks of "two aspects of proof": "the fact that we are brought to a new way of looking at things [proof as experiment], and given a new prescription of our language [proof as picture]" (1976: 82).

[12] Indeed, what distinguishes mathematics from empirical science is just this: In mathematics, there is no shift of medium as one moves from the experiment to its representation; the experiment takes place in the very same notational system which pictures it (RFM I: 36, cf. I: 165). This would indicate why no inductive process is required to judge the reproduction of proofs as pictures (compare Wright 1980: 466).

In order to substantiate all this, I present a somewhat more detailed reconstruction of Wittgenstein's reflections on *reductio* arguments and negative proofs.[13] Already in the *Tractatus,* Wittgenstein juxtaposed calculation and experiment:

> 6.233 To the question whether we need intuition [*Anschauung,* perception] for the solution of mathematical problems it must be answered that language itself here provides the necessary intuition [*Anschauung*, perspicuity].
>
> 6.2331 The process of *calculation* brings about just this *Anschauung*.
> Calculation is not an experiment.

If language itself provides the necessary perspicuity, a calculation is no experiment, because it does nothing to change the language or how things are seen. Instead, a calculation serves only to articulate and clarify relations within the notational system. After thus assimilating mathematics to logic in the *Tractatus*, Wittgenstein came to reconsider his early work and to introduce the notion of language games in the context of a broadened conception of mathematical practice (Epple 1994). Some language games are conservative and serve primarily to guarantee a result, others are experimental and might introduce change.

> "Proof must be surveyable" really serves to direct our attention at the difference between the notions: "to repeat a proof," "to repeat an experiment." To repeat a proof means, not to reproduce the conditions under which a particular result was once obtained, but to repeat every step *and the result*. (RFM III: 55)

The distinction applies to the difference between a calculation and a *reductio ad absurdum*. As we have seen above, the calculation assures reproducibility and identity of the proof by reproducing the result along with the "*compulsion* to preserve it" (RFM III: 55), a compulsion exerted by the proof in that it serves as a paradigm within the notational system. In contrast, the *reductio ad absurdum* provides the conditions under which the result could be obtained again and again but each time without necessity, since the *reductio* proves only that the conjunction of its various, more or less hypothetical premises cannot be maintained insofar as it leads into contradiction. If the *reductio* argument results in the denial of just one element of the conjunct, and if the selection of this element involves a decision, the repetition of the reductio argument does not necessarily include the repetition of the result.[14]

[13] The following reconstruction is adapted from Nordmann (2006).

[14] In his discussion of *reductio* arguments Wittgenstein nowhere distinguished between two cases that are often held apart. First, there are *reductio* arguments that feature among their premises only one explicitly hypothetical assumption. Since all the other premises are deeply entrenched axioms and theorems, the contradiction is here taken to force the denial of the hypothesis. In the second kind of *reductio* argument, the other premises or background assumptions are only taken to be relatively more secure than the hypothesis. In this case, the contradiction calls into question only the conjunction of all those assumptions and hypotheses, leaving at least a residue of choice in the determination of the conclusion. Wittgenstein did not recognize this distinction and thereby indicated that the language which provides perspicuity is always assumed and always subject to change, including even its deeply entrenched axioms and theorems (see VC: 181). Wittgenstein was not thereby arguing the finitist claim that we are constantly deciding whether to change the language or not, let alone that we ought to consider it as merely contingent; on the contrary, it is part of our natural history that we implicitly commit ourselves again and again to a received use of language (see RFM I: 118, IV: 11, or I: 63).

> If one considers a proof as an experiment, the result of the experiment is at any rate not what one calls the result of a proof. The result of calculation is the sentence with which it concludes; the result of the experiment is: that I was led by these rules from these sentences to that one. (RFM I: 162)

Here, proof and experiment are not opposed to each other. Instead, Wittgenstein invites us to consider the proof as a proof (a surveyable picture) or to consider the proof as an experiment (pictured by the proof as proof). Since these are two ways of considering proof rather than two types of proof, they cannot be distinguished as necessary on the one hand versus empirical on the other. The experiments of the mathematician and of the empirical scientist have in common that both researchers don't know what the result will be, but they differ in that the mathematician's experiment immediately yields a surveyable picture of itself – so that showing something and showing its paradigmatic necessity can collapse into a single step, which the empirical scientist's does not.[15]

> *Wittgenstein*: [...] Suppose I say, "I have found that the prime numbers often come in pairs." Is this the result of an experiment? – Here it looks just like an experiment. I didn't know what the result would be, and I found out by going through some divisions.
>
> *Wisdom*: In this case you have shown it not by experiment but by proof.
>
> *Wittgenstein*: Yes – but why do we say this here? – There is no difference between showing that they come in pairs and showing that they *must* come in pairs, just as there is no difference between showing that 17 is a prime number and showing that it *must* be a prime. [...] It has often been said – and there is something true in it and something absurd – that a mathematician sometimes makes what one might call experiments, and then proves what he has found out by experiment. But is this true? Is not the figure itself – the curve or the division – a proof? (LFM: 121)

This rather open-ended exchange hints at the "beautiful thought" mentioned by Mühlhölzer (2005: 68): "A proof, one could say, must originally have been a kind of experiment – but is then simply taken as a picture" (RFM III: 23). The picture of the proof would thus embody the compulsion by which the result was obtained and must be obtained again and again. When written down, a *reductio ad absurdum* also becomes such a picture and becomes a commitment to a certain use of signs where the axioms and theorems are clearly set off against the mere hypothesis denied by the conclusion. The pictured experiment thus displaces the experience of the experiment; that is, "that I was led by these rules from these sentences to that one" and that I thus came to reject the hypothesis.

> *Wittgenstein:* […] What is indirect proof? An action performed with signs. But that is not quite all. There is a further rule telling me what to do when an indirect proof has been

[15] See note 12 above and compare Bloor (1997: 41 ff.) If I understand correctly, Bloor offers the following account: Wittgenstein's "assimilation of calculation to experiment" cannot be understood in terms of empiricism versus Platonism but it can be understood if one looks at the establishment of social institutions, such as the "institution of measuring", where facts become standards and standards are facts under self-referential conditions. Mathematicians act within a system of signs that represents their actions; therefore, if they use something as the measure of something, it *is* the measure of that thing (cf. RFM I: 161-165, III: 67-77).

given. (This rule may read, for example: If an indirect proof has been given, the assumptions from which the proof starts are to be deleted.) *Here nothing is self-evident. Everything must be said explicitly.* […]

Waismann: [...] You could retain the refuted proposition by changing the stipulation regarding the application of indirect proof, and then our proposition would no longer be refuted.

Wittgenstein: Of course we could do that. We should then have destroyed the character of the indirect proof and only its schematic representation would remain. (VC: 180 f.)

By going behind the mere schematic representation and appreciating the character of proof as an action performed with signs, Wittgenstein considers it as a structured experience undergone by the person who invents or re-enacts a proof. A somewhat more detailed example helps to introduce this notion:

Suppose that we have a method of constructing polygons [...]. We are only allowed a ruler and a pair of compasses whose radius is fixed. We draw two diameters at right angles to one another in a circle; this gives us an inscribed square. We then draw arcs from the intersection points of the drawn diameters. Whether we call this bisecting or not doesn't matter. This is what we do. Thus we get the octagon, for instance. Similarly we could get a polygon with 16 sides, and so on.

Now someone is asked to produce the 100-gon this way. At first he goes on trying and trying, keeps on bisecting smaller and smaller angles and doesn't get any satisfactory result. Then in the end we prove to him that the 100-gon cannot be constructed in this way.

It seems as if we first of all made an experiment which showed that Smith, Jones, etc. could not construct a 100-gon in that way, and then a mathematician shows that it can't be done. We get apparently an experimental result, and then prove that it could not have been otherwise at all.

But there is something queer about this: For how could the man try to do what could not be done? (LFM: 86 f.)

Like all *reductio*-arguments and, indeed, like all mathematical proofs, this proof is an impossibility proof: In light of background assumptions, commitments, or rules it proves impossible to hold on to an intention, to claim a possibility, or to assert a proposition. In the ideal case, this impossibility manifests itself in the form of a contradiction, but it can also manifest itself in the form of defeat: "It can't be done."[16] Either way, such impossibility proofs raise the fundamental question whether one can even try to do what turns out to be impossible. Wittgenstein never questions that it is impossible even to conceive a contradiction (see already TLP 3.03 and 5.61). How then can it be so easy to posit, think through, even insist for a while on a set of premises that turns out to be contradictory? Wittgenstein expresses this concern in the following passage:

The difficulty which one senses in regard to *reductio ad absurdum* in mathematics is this: What goes on in this proof? Something mathematically absurd, and hence unmathematical?

[16] The difference between these cases can be as inconsequential as that between showing that "17" is a prime and that it must be a prime.

> How can one – one would like to ask – even hypothesize what is mathematically absurd? That I can assume what is physically false and lead it to absurdity creates no difficulties for me. But how to think what is so-to-speak unthinkable?! (RFM V: 28)[17]

The question admits of only one answer: No one is thinking the unthinkable. In the case at hand, we might just be misunderstanding or misapprehending the conjunction of premises because we cannot fully survey the situation that will lead us from the beginning of our experiment to a contradiction. In other words, we are not yet seeing the proof as a proof. However, the term "misunderstanding" might give rise to a misunderstanding of its own, because it suggests that the mistake or misapprehension is avoidable. We should more appropriately say that we do not and cannot understand the conjunction of premises until we have undergone the experience and conducted the proof as experiment. What makes the proof a proof is precisely that it leads us to see the impossibility even of trying what we set out to do only a little while ago: The proof effects a revision of the domain of the imaginable.

> The question arises: Can't we be mistaken in thinking that we understand a question?
>
> For many mathematical proofs do lead us to say that we *cannot* imagine something which we believed we could imagine. (E.g., the construction of the heptagon.) They lead us to revise what counts as the domain of the imaginable. (PI: 517)

What we were once able to imagine (the construction of a 100-gon) has now moved into the domain of the unimaginable. Indirect proofs or *reductio* arguments bring about just such revisions. This is neither the discovery of something new nor the mere exhibition of a meaning that is implicit in the conjunction of premises. Instead, it is a critical intervention or an action that alters the language and thus the form of intuition that provides perspicuity.[18]

Using as his example the impossibility of trisecting an angle by geometrical means, Wittgenstein details how this critical intervention unfolds: where our original confidence originates, when we encounter defeat and finally how we arrive at the insight that we wanted something unimaginable. Here, the revision of the domain of the imaginable consists in the experiment changing "our idea of trisection":

> Again, the importance of the proof that trisection is impossible is that it changes our idea of trisection. – The idea of trisection of an angle comes in this way: that we can bisect an angle, divide into four equal parts, and so on. And this leads to the problem of trisecting an angle. You are led on here by *sentences*. You have the sentence "I bisect this angle" and

[17] Michael Nedo shows how this passage originally appeared in Wittgenstein's manuscript 126 in the context of a sustained discussion of G.H. Hardy's *Course of Pure Mathematics*. Hardy would open an indirect proof with "suppose, if possible, that …" (Nedo 2008: 86-97; Hardy 1941: 6).

[18] Proof as picture displays the relation between sentences, showing how certain sentences are transformed to yield others (conclusions). Proof as experiment does not add or subtract sentences but concludes with a new way of looking at sentences. As we will see, this new way of looking at sentences alters the language by probing certain linguistic commitments and thus by playing off one part of language against another, without presupposing a strict separation between the prose that surrounds a formal mathematical core and the proofs themselves. (On prose vs. proof, see e.g., RFM IV: 27; cf. Floyd 2008 vs. Lampert 2008.)

> you form a similar expression: "trisecting". And so you ask, "What about the sentence, 'I trisect this angle'?" […] If we had learned from the beginning the series of constructions of *n*-gons, then nobody would ever have asked whether the heptagon is constructible. It's none of these, that's all.
>
> […] The problem arose because our idea at first was a different idea of the construction of *n*-gons, and then was *changed* by the proof. (LFM: 88 f.)

One quickly recognizes in this account a central theme of Wittgenstein's critique of language in the *Tractatus* as well as in the *Philosophical Investigations*. Led on by language, we imagine that every noun is a name, that every grammatical sentence pictures a fact. This is how we move so effortlessly from "This door is blue" to "This person is good" or from expressions of fact to expressions of value. However, had we learned from the beginning the proper sectioning of angles, the series of constructions of *n*-gons, or the way in which truth-conditions make for meaningful sentences, nobody would ever have asked whether trisection is possible or whether an absolute value is expressible in our language. If one wants to know how this shift from what can be imagined to what is unimaginable came about, one needs to understand what was proven. Also, inversely, if one wants to know what was proven, one must understand the revision in the domain of the imaginable that was effected by the proof. Thus, "let the *proof* teach you *what* was being proved" (PI II: xi).[19]

In an indirect or negative proof, one begins with something conceivable and ties it to a specific employment of signs. As we attempt to trisect an angle or to construct a 100-gon, we commit ourselves to certain rules of construction and then discover that they leave out the case of trisection or of the 100-gon; in other words, the rules simply don't provide for those[20]:

> The proof might be this: we go on constructing polygons and being very careful to observe certain rules. We should then find that the 100-gon is left out. If we want to construct the *n*-gon in that way, *n* has to be a power of 2. The last power of 2 before 100 is 64, after that is 128, and so 100 is left out. This would have the result of dissuading intelligent people from trying this game. (LFM: 87)

[19] This temporal and experiential dimension (only the proof can tell you what was proven) is not sufficiently appreciated by Jaako Hintikka's incisive critique of Wittgenstein. Hintikka recognizes that Wittgenstein rejects "the idea that statements of the *possibility* of geometrical constructions [the domain of the imaginable] belong to the same language game as the constructions themselves" (Hintikka 1993: 37). But why should they (as Hintikka assumes they should) belong to the same language game in the first place? The tools and rules that constitute the game are not surveyable while certain pictures constructible within the game are. And thus, I can be mistaken in what I understand and do not understand, what I can do (what is possible) and what I can't do (what is impossible) in my language.

[20] This is why Timm Lampert insists that, for Wittgenstein, proof is not a matter of logical deduction but of defining operations: Do the rules of construction provide or leave out a certain case? Contrary to Lampert, this does not imply that "mathematics completely dispenses with logic" and that Wittgenstein "rejects the use of certain deduction rules such as *reductio ad absurdum*" (Lampert 2008: 63). He only rejects certain construals of deduction rules.

If people are very careful to observe certain rules and discover that these rules do not allow them to pursue a plan or maintain a hypothesis, they will abandon their plan and deny the hypothesis – as long as they want to stick to their rules.[21] Indeed, by abandoning the plan and denying the hypothesis, they not only revise their conception of what they can hope for or what they can maintain within the game they are playing, they also reaffirm their commitment to the rules of the game itself: "Every proof is as it were a commitment to a specific use of signs." (RFM III: 41).

> The indirect proof says, however: "If you want it like *that*, you may not assume *this*: for *with this* is compatible only the opposite of that which you want to hold on to." (RFM V: 28)

The clause "if you want it like *that*" points to the conditional structure of the indirect proof, and thus to another aspect of the proof as experiment. To enter into the experiment is to be prepared to reevaluate its basic assumptions. An outward sign of this preparedness is the hypothetical beginning of the indirect proof. It places the experiment in the subjunctive mood: "If I were to assume this, what would follow?"[22] The experiment thus involves a sense of possibility that is ready to change or act. Wittgenstein describes this state of readiness in the *Philosophical Investigations*:

> The if-feeling is not a feeling which accompanies the word "if."
>
> The if-feeling would have to be compared with the special 'feeling' which a musical phrase gives us. (One sometimes describes such a feeling by saying: "Here it is, as if a conclusion were being drawn" or "I should like to say, '*hence*....' ", or "Here I should always like to make a gesture –" and then one makes it.) (PI II: vi)

Accordingly, *reductio ad absurdum* corresponds to a structured experience that makes sense. It allows us to shift from an old to a new state, from the wrong way of seeing the world to the right way.[23] But a way of seeing the world stands only at

---

[21] Similarly, the author and readers of the *Tractatus* are committed to certain rules of using sentences to picture facts. Probing these rules, one discovers that they do not provide for the expression of absolute value: This case is omitted by the notational system that is designed to describe the world truthfully (Nordmann 2005). This discovery needs to be actively made, e.g., by running up against a contradiction in TLP 6.41. In recent years, Cora Diamond and James Conant advanced a similar argument: "Thus the elucidatory strategy of the *Tractatus* depends on the reader's provisionally taking himself to be participating in the traditional philosophical activity of establishing theses through a procedure of reasoned argument; but it only succeeds if the reader fully comes to understand what the work means to say about itself when it says that philosophy, as this works seeks to practice it, results not in doctrine, but in elucidation, not in [philosophical sentences] but in [the becoming clear of sentences]. And the attainment of this recognition depends upon the reader's actually undergoing a certain *experience* – the attainment of which is identified in 6.54 as the sign that the reader has understood the author of the work: the reader's experience of having his illusion of sense (in the 'premises' and 'conclusions' of the "argument") dissipate through its becoming clear to him that (what he took to be) the [philosophical sentences] of the work are [nonsense]" (Conant 2000: 196 f.).

[22] See note 14 above regarding the conditional structure also of "direct" proof.

[23] Compare this language to the last remarks of the *Tractatus* (see Nordmann 2005).

the very beginning and end of the experiment. The experiment itself is characterized by Wittgenstein in terms of practical commitment, experiment, movement and change. To the question "What is indirect proof?" he answered, "An action performed with signs." (WVC: 180) The action of the *reductio* argument consists of its showing us something, and what it shows makes sense in the context of action but is not expressed by a sentence as a picture with propositional content and truth-conditions.

> There is a particular mathematical method, the method of *reductio ad absurdum*, which we might call "avoid the contradiction." In this method one shows a contradiction and then shows the way from it. But this doesn't mean that a contradiction is a sort of devil. (LFM: 209)

Quite the contrary, instead of being a sort of devil, the contradiction is an integral turning-point of a structured experience. The *reductio* argument shows the way from the contradiction to the conclusion, and the conclusion exhibits or reveals, in turn, the specific commitment that directs the avoidance of the contradiction.[24] So, the contradiction turns out to be creative: It is the vehicle by which our commitments disclose a new perspective from which to see the world aright.[25]

13.3 Conclusion

In the *Tractatus*, Wittgenstein distinguished between calculation and experiment (6.233 and 6.2331). In his later work, the distinction is that between proof considered as picture and proof considered as an experiment – calculation is an exemplary picture, the *reductio* argument an exemplary experiment. There is something appealing, of course, to the consideration of these two complementary aspects of proof. Pictures seem to be static, experiments dynamic; pictures stand for a synchronic and experiments for a diachronic dimension; pictures are objects in the context of justification and experiments belong to the context of discovery. It is important, however, to resist this easy and appealing view of the complementarity between pictures and experiments.

First of all, pictures and experiments are not aspects of proof. When we *see* a proof, we see a picture. We do not see the proof at all when we are engaged in an experiment. Then, we are trying to do something that, perhaps, cannot be done, and we learn from our failure when we run into a contradiction and use it as a prompt for a creative decision that changes the domain of the imaginable. Only the proof as picture is a proof to behold, but this is not to say that it is static and unchangeable;

---

[24] Wittgenstein identifies this as the reason it makes sense to have multiple proofs of the same proposition. Further proofs do not render the proposition more secure. Each proof highlights some antecedent commitment or some mathematical context that would lead us into contradiction if we were to deny the conclusion (RFM VII: 10; also manuscript 126: 124 f. cited by Nedo 2008: 90).

[25] Louis Caruana identified three instrumental uses of contradictions (Caruana 2004: 232). This one is not among them.

the picture is an object of investigation par excellence, one that allows us to make discoveries about the relation of its elements. We might say, then, that the opposition between picture and experiment expresses well what is only clumsily hinted at by opposing static versus dynamic, synchronic versus diachronic, justificatory versus exploratory aspects of proof.

Indeed, the conception of proof as experiment is most informative to those who are already thinking about invention and change in mathematics but see this change only as the displacement of one picture by another and thereby neglect the experiential structure of change.[26] Accordingly, Wittgenstein's dictum that we should look at the proof in order to know what was proved (PI II: xi; PG II, V: 24) takes on a different meaning for the proof as picture and for the proof as experiment. In a proof considered as a surveyable picture, every step and the result tell us what was proved. Wittgenstein's injunction refers to identity-conditions: A proof with a different result is a different proof, whereas a scientific experiment with a different outcome can still be the same experiment. In a proof considered as an experiment, the experience of failure tells us what was proved, namely that we cannot have this if we want to hold on to that. The proof thus renders salient some piece of our language and some of our commitments, allowing us to settle into a domain of the imaginable. Here, our conclusion dissolves an irritation of doubt by transforming the situation so that our initial problem goes away. This experiential conception of proof moves Wittgenstein into the proximity of pragmatist epistemologies like those of Peirce and Dewey, and yet further from Frege's and Russell's conceptions of language, logic and thought.

References

Bloor, D. (1997). *Wittgenstein, rules, and institutions*. London: Routledge.

Caruana, L. (2004). Wittgenstein and the status of contradictions. In A. Coliva, & E. Picardi (eds.). *Wittgenstein today*. Padova: Il Poligrafo: 223–232

Conant, J. (2000). Elucidation and nonsense in Frege and Early Wittgenstein. In A. Crary, & R. Read (eds.). *The new Wittgenstein*. London: Routledge: 174–217

Cozzo, C. (2004). Rule following and the objectivity proof. In A. Coliva, & E. Picardi (eds.). *Wittgenstein today*. Padova: Il Poligrafo: 185–200

[26] Crispin Wright's (1991) argument against Kripke advances just such a narrow conception, which does not acknowledge the temporary suspension of rules as the domain of the imaginable is changed: "changing and extending [mathematical discourse] […] is a notion of which we can make sense only under the aegis of a distinction between practices which conform to the rules as they were before, and practices which reflect a modification in those rules generated by some pure mathematical development. Unless, then, there is such a thing as practice which is in line with a rule, contrasting with practice which is not, there is simply no chance of a competitive construal of Wittgenstein's positive proposals" (Wright 1991: 88; cf. Wright 1980). Cesare Cozzo presses the issue further: "Can we consistently endorse both the plasticity of meaning and the objectivity of proof?" (Cozzo 2004: 193). This is where one needs to insist that, to become a proof to behold, a proof has to become a surveyable picture without plasticity of meaning.

Epple, M. (1994). Das bunte Geflecht der mathematischen Spiele. *Mathematische Semesterberichte*: 113–133.

Floyd, J. (2008). Wittgenstein über das Überraschende in der Mathematik. In Matthias Kroß (ed.). *Ein Netz von Normen: Wittgenstein und die Mathematik*. Berlin: Parerga: 41–77

Frascolla, P. (1994). *Wittgenstein's philosophy of mathematics*. London: Routledge.

Frascolla, P. (2004). Wittgenstein on mathematical proof. In A. Coliva, & E. Picardi (eds.). *Wittgenstein today*. Padova: Il Poligrafo: 167–184

Hardy, G. H. (1941). *A course in pure mathematics*. London: Cambridge University Press.

Hintikka, J. (1993). The Original Sinn of Wittgenstein's Philosophy of Mathematics. In K. Puhl (ed.). *Wittgensteins Philosophie der Mathematik – Akten des 15. Internationalen Wittgenstein-Symposiums Vol. II*. Wien: Hölder-Pichler-Tempsky: 24–51

Klenk, V. H. (1976). *Wittgenstein's philosophy of mathematics*. The Hague: Martinus Nijhoff.

Lampert, T. (2008). Wittgenstein on the infinity of primes. *History and Philosophy of Logic, 29*: 63–81.

Marion, M. (2004). Wittgenstein on mathematics: constructivism or constructivity? In A. Coliva, & E. Picardi (eds.). *Wittgenstein today*. Padova: Il Poligrafo: 201–222

Mühlhölzer, F. (2005). 'A mathematical proof must be surveyable': what Wittgenstein means by this and what it implies. *Grazer Philosophische Studien, 71*: 57–86.

Mühlhölzer, F. (2008). Wittgenstein und der Formalismus. In Matthias Kroß (ed.). *Ein Netz von Normen: Wittgenstein und die Mathematik*. Berlin: Parerga: 107–141

Nedo, M. (2008). Anmerkungen zu Wittgensteins Bemerkungen über die Grundlagen der Mathematik. In Matthias Kroß (ed.). *Ein Netz von Normen: Wittgenstein und die Mathematik*. Berlin: Parerga: 79–103

Nordmann, A. (2005). *Wittgenstein's Tractatus: an introduction*. Cambridge: Cambridge University Press.

Nordmann, A. (2006). Beweis als Experiment bei Wittgenstein. *Sic et non. Zeitschrift für Philosophie und Kultur*. *Im Netz*, *5*: 11pp.

Ramharter, E., & Weiberg, A. (2006). *"Die Härte des Logischen Muss": Wittgensteins Bemerkungen über die Grundlagen der Mathematik*. Berlin: Parerga.

Weiberg, A. (2008). Rechnung versus Experiment: Mathematische Sätze als grammatische Sätze. In Matthias Kroß (ed.). *"Ein Netz von Normen": Wittgenstein und die Mathematik*. Berlin: Parerga: 17–39

Wittgenstein, L. (1922). *Tractatus Logico-Philosophicus (TLP)*, C. K. Ogden (Transl.). London: Routledge.

Wittgenstein, L. (1958). *Philosophical Investigations (PI)* (3rd ed.). New York: Macmillan.

Wittgenstein, L. (1974). In R. Rhees (ed.). *Philosophical grammar (PG)*. Berkeley: University of California Press.

Wittgenstein, L. (1976). In C. Diamond (ed.). *Wittgenstein's Lectures on the Foundations of Mathematics Cambridge, 1939 (LFM)*. Ithaca: Cornell University Press.

Wittgenstein, L. (1978). *Remarks on the foundations of mathematics (RFM)* (3rd ed.). Oxford: Basil Blackwell.

Wittgenstein, L. (1979). *Wittgenstein and the Vienna Circle: conversations recorded by Friedrich Waismann (VC)*. New York: Barnes and Noble.

Wittgenstein, L. (1982). In A. Ambrose (ed.). *Wittgenstein's Lectures Cambridge, 1932–1935*. Chicago: University of Chicago Press.

Wrigley, M. (1993). The continuity of Wittgenstein's philosophy of mathematics. In K. Puhl (ed.). *Wittgensteins Philosophie der Mathematik – Akten des 15. Internationalen Wittgenstein-Symposiums Vol. II*. Wien: Hölder-Pichler-Tempsky: 73–84

Wright, C. (1980). *Wittgenstein on the foundations of mathematics*. London: Duckworth.

Wright, C. (1991). Wittgenstein on mathematical proof. In A. P. Griffiths (ed.). *Wittgenstein centenary essays*. Cambridge: Cambridge University Press: 79–99

Chapter 14
Experimentation and Proof in Mathematics

Michael de Villiers

14.1 Introduction

Mathematical education at the school or university level often fails to provide students with a sense of how new results can or could be discovered or invented. Quite often, after a teacher has carefully presented theorems and their proofs, students are just given exercises with riders of the type "Prove that .".. This caricature of mathematics can easily create the false impression that mathematics is only a systematic, deductive science. However, as George Polya, Imre Lakatos, and many others have pointed out, mathematics in the making is often an experimental, inductive science.

The main purpose of this paper is to investigate the role of experimentation in mathematics, reflecting on some historical examples and some from my own mathematical experience. I hope this will provide a useful conceptual frame of reference for curriculum designers in mathematics education, as well as a basis for evaluating learning activities and curricula.

By experimentation I mean very broadly all intuitive, inductive or analogical reasoning, specifically when it is employed in the following instances:

(a) Mathematical conjectures and/or statements are evaluated numerically, visually, graphically, diagrammatically, physically, kinaesthetically, analogically, etc.
(b) Conjectures, generalisations or conclusions are made on the basis of intuition or experience obtained through any of the above methods.

Though neither complete nor original, the following list comprises some of the most important functions of experimentation (in no specific order of importance). These functions are quite often closely linked, as I will illustrate in the following discussion and examples:

M. de Villiers (✉)
School of Science, Mathematics & Technology Education, University of KwaZulu-Natal, Durban, South Africa
e-mail: profmd@mweb.co.za

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_14,

- *Conjecturing* (looking for an inductive pattern, generalisation, etc.)
- *Verification* (obtaining certainty about the truth or validity of a statement or conjecture)
- *Global refutation* (disproving a false statement by generating a counter-example)
- *Heuristic refutation* (reformulating, refining or polishing a true statement by means of local counter-examples)
- *Understanding* (grasping the meaning of a proposition, concept or definition or assisting in the discovery of a proof).

14.2 Conjecturing

The history of mathematics is replete with hundreds of cases where conjectures were made largely on the basis of intuition, numerical investigation and/or construction and measurement. A good example, the famous Prime Number theorem, was first formulated about 1792 by Gauss. Using logarithms with numerical evidence obtained from counting prime numbers, Gauss discovered that the number of prime numbers smaller or equal to a number n is always approximately $\frac{n}{\log n}$, and that the approximation improves as n increases. Several mathematicians used Gauss's conjecture at the beginning of the nineteenth century to explore different properties of prime numbers, even though a partial proof of it was only given in 1850 by Chebychev. The conjecture was generally accepted as proved after 1859, when Riemann published a more complete proof. However, there were still some gaps in Riemann's proof, which Hadamard and De La Vallée Poussin filled in, independently of each other, in 1896.

As Hanna pointed out (1983, p. 73), this historical example shows that mathematicians may sometimes, even in the absence of rigorous proofs, accept certain inductively confirmed conjectures as "theorems", especially in an important field of research. Similarly, George Polya strongly emphasised the importance of experimentation in the discovery or invention of new mathematics, quoting one of the most productive mathematicians of all time, Leonhard Euler, in this regard:

> As we must refer the numbers to the pure intellect alone, we can hardly understand how observations and quasi-experiments can be of use in investigating the nature of numbers. Yet in fact ... the properties of the numbers known today have been mostly discovered by observation, and discovered long before their truth has been verified by rigid demonstrations (Euler, *Opera Omnia*, ser.1, vol. 2, p. 459, cited in Polya 1954, p. 3).

In the past few decades, the modern computer, an extremely powerful tool for experimental exploration, has revolutionised mathematical research in several areas, delivering many new, exciting results. One of computer exploration's main advantages is that it provides powerful visual images and intuitions that can contribute to the user's growing mathematical understanding of that particular research area. Furthermore, the computer provides a unique opportunity for the researcher to formulate a great number of conjectures and to immediately test them by varying only a few parameters of the particular situation. In fact, Hofstadter (1985, pp. 366–369) argues that traditional non-computer based research simply could not have arrived so quickly and easily at such a rich, coherent body of new results in so many areas of modern mathematics.

Not surprisingly, in 1991 a successful new quarterly journal, *Experimental Mathematics*, was established. Its main mission is to publish not only finished theorems and proofs but also in the experimental way in which these results have been reached. In other words, it aims specifically to display the dynamic interaction between theory and experimentation in research mathematics (see Epstein and Levy 1995).

Even traditional Euclidean geometry is experiencing an exciting revival, due in no small part to the recent development of dynamic geometry software (DGS) such as *Cabri*, *Sketchpad* and *Cinderella*. In fact, Philip Davis (1995) predicts as a consequence of DGS a particularly rosy future for triangle geometry research. For example, Adrian Oldknow (1995) recently used *Sketchpad* to discover the hitherto unknown result that the Soddy center, incenter and Gergonne point of a triangle are collinear (amongst other interesting, related results). Similarly, I recently experimentally discovered a generalisation of Neuberg's theorem (De Villiers 2002), and rediscovered a beautiful generalisation of the nine-point and Spieker circles of a triangle to respective conics, as well as associated generalisations of the Euler and Nagel lines (De Villiers 2005, 2006).

Reasoning by analogy to arrive at new conjectures is another method unfortunately not demonstrated frequently enough to high school and university students. For example, starting from Viviani's theorem that the sum of the three distances from a point inside an equilateral triangle to the three sides is constant, one can easily conjecture that a similar constancy might be true for any regular polygon. Or, moving into three dimensions, one can conjecture that the sum of the four distances from a point inside regular tetrahedron to its faces is also constant.[1] Polya (1954, 1968, 1981) gives many such examples that can suitably be adapted for mathematics teaching at various levels.

Much is often made of the crucial role of "intuition" in mathematical discovery and invention. Perhaps most significant from an educational point of view, most authors strongly emphasise that intuition depends on "experience" rather than just innate, natural ability. In other words, mathematical intuition mostly develops from the regular handling, exploration and manipulation of mathematical objects and ideas (cf. Davis and Hersh 1983, pp. 391–392; Epstein and Levy 1995). Such experience refers not only to formal logical manipulation but also to experimental exploration of objects and ideas, often over days, months or years. This view obviously has significant implications for designing curricula and learning materials.

[1] In fact, it holds for a tetrahedron with equi-areal faces, and for equilateral or equi-angled polygons.

14.3 Verification/Conviction

Contrary to many mathematics teachers' traditional belief that only proof provides certainty for the mathematician, mathematicians are often convinced of the truth of their results (usually on the basis of experimental evidence) long before they have proofs. Indeed, as I have argued (De Villiers 1990), conviction is often a prerequisite for seeking a proof. If uncertain about a result, one would rather look for a counter-example than for a proof. A person needs to be reasonably convinced of a result's truth before sitting down and possibly spending considerable time and energy generating a proof.

In real mathematical research, personal conviction usually depends on a combination of experimentation and the existence of a logical (but not necessarily entirely rigorous) proof. As mentioned above, a very high level of conviction may sometimes be reached even in the absence of a proof. For instance, Polya quotes Leonhard Euler, who made an important discovery in the algebra of the real numbers and wrote about his empirical certainty:

> It suffices to undertake these calculations and to continue them as far as it is deemed proper to become convinced of the truth of this sequence continued indefinitely. Yet I have no other evidence for this, except a long induction, which I have carried out so far that I cannot in any way doubt the law ... I have long searched in vain for a rigorous demonstration... and I have proposed the same question to some of my friends with whose ability in these matters I am familiar, but all have agreed with me on the truth … without being able to unearth any clue of a demonstration (Euler, *Opera Omni*, ser. 1, vol. 2, p. 249–250, cited in Polya 1954, p. 100).

The history of mathematics bears out that this kind of experimental conviction often precedes and motivates a proof, given the frequent heuristic precedence of results over arguments, of theorems over proofs. For example, Gauss is reputed to have complained: "I have had my results for a long time, but I do not yet know how I am to (deductively) arrive at them" (Arber 1954, p. 47). Paul Halmos (1984) underscores this idea when he describes his own practise as a mathematician:

> The mathematician at work ... arranges and rearranges his ideas, and he becomes convinced of their truth long before he can write down a logical proof. The conviction is not likely to come early – it usually comes after many attempts, many failures, many discouragements, many false starts ... experimental work is needed ... thought –experiments (p. 23).

The practise of first evaluating an unknown conjecture by the consideration of specific cases is probably as old as mathematics itself, and is still actively utilised in modern research. Neubrand (1989, p. 4) for example writes as follows about the proof of Bieberbach's conjecture (1916; now De Branges' theorem, 1984):

> As in many other cases, in this example mathematicians first started with the consideration of special cases, restricted cases, etc., in order to convince themselves of the possibility of the validity of the conjecture.

Furthermore, experimental evidence frequently plays a role not only in the initial formulation of a conjecture but also in continuing efforts to prove a particular result. Let us consider the very simple example of an isosceles trapezoid which has (at least) one opposite pair of parallel sides and equal diagonals. It seems reasonable

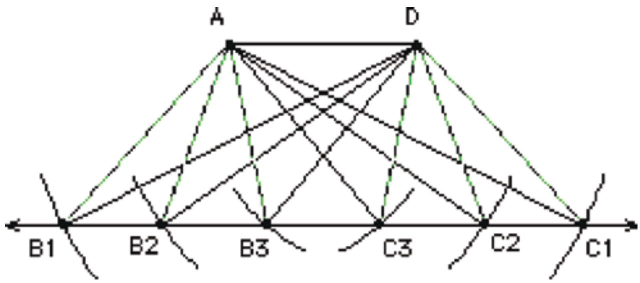


Fig. 14.1 Gaining certainty

to conjecture that these characteristics might be sufficient for defining an isosceles trapezoid. (Invite the reader to first try and prove this before reading further). (However, suppose one does not fairly quickly come up with a proof). One would naturally start wondering whether it is indeed true. Perhaps the conjecture is false and one is trying to prove something that is not true!

However, by accurately (or even roughly) drawing a line segment *AD* and a line parallel to it, and then equal diagonals *AC* and *DB*, as shown in Fig. 14.1, one can intuitively see, even without measurement, that opposite sides $AB_n = DC_n$, irrespective of how or where the diagonals $AC_n = DB_n$ are drawn. Even better, one could do the construction in a DGS environment in order to gain an even higher level of confidence. Now armed with the knowledge that a counter-example cannot be constructed and that the proposition is definitely true, one can with renewed confidence resume looking for a proof.

Of course, experimentation is not always a prerequisite for making conjectures and arriving at solutions. Consider the following example, which might be used in teaching. Students are asked to find the total number of tennis matches played in a knockout singles competition if there are n players. Most students might approach the problem inductively by looking at cases $n=2, 3, 4$, etc. and then generalising. However, a more astute student may, by just thinking carefully about the situation, quickly realise that the total number of matches must be $n-1$, because there can only be one final winner and there must therefore be $n-1$ matches to eliminate the other $n-1$ players.

Suppose no student makes the crucial, initial conceptualisation (looking at the losses rather than the rounds) and all the students proceed to solve the problem the hard way? In such a case, the teacher can still direct their attention to wondering why the answer is one less than the number of players and whether this is a signal that they have missed both the essence of the situation and the opportunity to solve it more elegantly.

Through such activities, students could learn that reflective logical thought may indeed sometimes be more powerful and appropriate than immediately embarking on a quasi-empirical search for patterns.[2]

[2] Of course, sometimes a combination of reflective thought and experimentation is needed. For example, from $3^2 + 4^2 = 5^2$ and $5^2 + 12^2 = 13^2$, we can see that perhaps $7^2 + 24^2 = 25^2$ and guess that similar equations might hold for 9^2; 11^2; 13^2,... Indeed, noting the structure – that, say $13^2 - 12^2 = (13-12)(13+12) = 25 = 5^2$ – gives us a clue for constructing and checking, with a minimum of pain, other instances.

14.4 Global Refutation

In everyday life people often use a kind of fuzzy logic; that is, believing certain things to be true if they are true most of the time and simply ignoring the occasional cases when they aren't true. Unlike everyday life, however, mathematical theorems can have no exceptions; just one counter-example suffices to disprove a mathematical proposition. By "global refutation" I mean the production of a logical counter-example that meets a statement's conditions but refutes the statement's conclusion and thus its validity.

In mathematics at the elementary level, global counter-examples are often produced by experimental testing and perhaps not as frequently by deductive reasoning. Consider the following false conjecture from elementary geometry: "a quadrilateral with perpendicular diagonals is a kite". To construct a counter-example for this statement it is only necessary to check experimentally whether sufficient information is provided for the construction of a kite. If one now constructs two perpendicular diagonals and let the various segments have arbitrary lengths as shown in Fig. 14.2, one easily finds that the constructed figure is not necessarily a kite.[3]

Similarly, one would not use deduction to construct counter-examples for conjectures like "a quadrilateral with equal diagonals is an isosceles trapezoid"[4], or "$6x-1$ is a prime number for all $x=1, 2, 3$, etc.", but experimental testing.

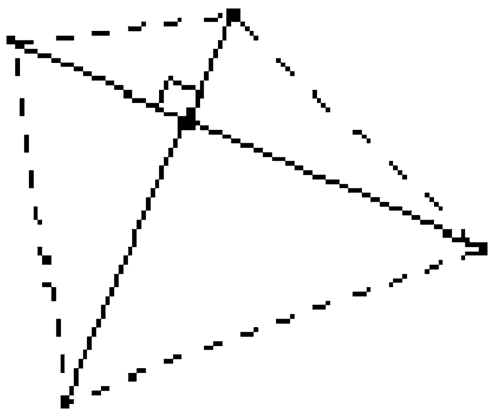


Fig. 14.2 A global counter-example

[3]It would obviously be instructive for students to further examine this conjecture and to identify the additional key property that one of the perpendicular diagonals should bisect the other. The initial inarticulation of hypotheses happens quite regularly with inexperienced students, and necessitating the fostering of a state of mind characterised by acute analysis and thoroughness.

[4]As before, it might be a valuable learning experience to guide students to identify the additional property that the equal diagonals need to cut each other in the same ratio.

There are many examples from the history of mathematics that clearly illustrate how experimental testing generated counter-examples though sometimes taking many years to do so. For example, in the fifth century BC Chinese mathematicians had already made the conjecture that if $2^n - 2$ is divisible by n then n is a prime number (see Kramer 1981, p. 514). If this were true, it would have been valuable for determining the primality of any number n, as then one would only have to divide $2^n - 2$ by n. Approaching the conjecture inductively, one finds that $2^3 - 2$, $2^5 - 2$, $2^7 - 2$ are divisible by the primes 3, 5 and 7, for example, but $2^4 - 2$, $2^6 - 2$, $2^8 - 2$ are not divisible by the corresponding composite numbers 4, 6 and 8.

It turns out that experimental investigation supports the conjecture up to $2^{340} - 2$ (a very large number indeed!). In all these cases, $2^n - 2$ is divisible by n when n is prime and not divisible by n when n is composite. However, this conjecture was finally disproved in 1819, when it was found that $2^{341} - 2$ is divisible by 341, but 341 is not prime, because $341 = 11 \times 31$.

A more contemporary example refers to Lord Kelvin's conjecture, dating from about 1850, that the optimal partition of space into equal volumes with minimal total surface is obtained by warping the tiling of space by truncated octahedra. Everyone seemed satisfied with Kelvin's solution; and it was believed that it would be only a matter of time before a proof of its optimality was produced. However, using a computer programme, *Surface Evolver*, the physicists Weaire and Phelan in 1994 produced a space partition of equal volumes with a considerably smaller surface area than Kelvin's solution (Epstein and Levy 1995; Hales 2000). Yet, it remains unknown whether even theirs is the best possible solution; hence, the Kelvin problem is still open.

Experimental testing is also useful for identifying incorrect assumptions in otherwise completely valid reasoning. Dynamic geometry software is particularly useful in this regard, as a configuration can be easily and quickly dragged into many different variations in order to check the general validity of one's assumptions. Many ingenious geometric paradoxes such as "all triangles are isosceles" can arise by virtue of construction errors or mistaken assumptions in diagrams (cf. Movshovitz-Hadar and Webb 1998). Not only is unravelling paradoxes by pinpointing the precise reasoning behind errors or mistaken assumptions educationally instructive but also as Kleiner and Movshovitz-Hadar (1994) pointed out, paradoxes have historically contributed to the evolution of many parts of mathematics.[5]

In a historical reconstruction, Waterhouse (1994) suggested that Gauss had to have used substantial theoretical argumentation to arrive at the counter-example he gave in 1807 to a conjecture by Sophie Germain. An even more spectacular example is Mertens' conjecture. Despite the fact that this conjecture already in 1963 had

[5] However, not all counter-examples are constructed by experimentation or quasi-empirical testing. For example, since 41 is clearly a factor of $n^2 - n + 41$ when $n = 41$, one might easily notice without any quasi-empirical substitution that it provides an immediate counter-example to the conjecture that $n^2 - n + 41$ is always prime for $n = 1, 2, 3$, etc.

computer-supported evidence for all n up to 10 million, Odlyzko and Te Riele gave an existential proof for the existence of a counter-example in 1984 (without constructing an actual counter-example!).

14.5 Heuristic Refutation

Although mathematics is not an empirical science, it grows and develops, according to Lakatos (1983), similarly to the natural sciences; that is, as a consequence of the quasi-empirical testing of theorems, concepts, definitions, and so forth. New counter-examples necessitate the re-examination of old proofs, and new proofs are created accordingly.

Lakatos (1983) analysed the history of Euler's theorem[6] for polyhedra and dramatised it within a fictional classroom context. Euler first stated in 1750, without proof, that for polyhedra such as the tetrahedron, octahedron, etc. $V-E+F=2$ where V, E and F are respectively the numbers of vertices, edges and faces; he eventually produced a proof in 1752. More rigorous proofs followed in the nineteenth century, by Legendre, Cauchy, Gergonne, Rothe and Steiner. Nevertheless, there continued to be exceptions or "monsters", such as Kepler's star dodecahedron, for which Euler's formula $V-E+F=2$ was not valid. Only towards the latter part of the nineteenth century did topologists finally manage to develop a completely satisfactory proof based on a more precise, general definition of polyhedra. This proof was also valid for Kepler's star dodecahedron and generalised the formula to $V-E+F=2-2g$ (where g is the "genus" of the polyhedron; see Grünbaum and Shephard 1994; Hilton and Pedersen 1994).

Lakatos (1983) attributes the inordinately long delay in resolving the Euler theorem to the contemporary leading mathematicians' not realising that they ought to have closely examined the "proofs" to identify the guilty lemmas immediately after the heuristic counter-examples appeared. Instead, they typically tended to treat the heuristic counter-examples by simply ignoring them or rejecting them as "monsters" and excluding them by definition. According to Lakatos (1983, pp. 137–139) this "monster-barring" process was a direct consequence of the dominant view that deductive proof was always infallible and therefore formal proofs were above scrutiny and unquestionable.

From a Lakatosian viewpoint it is therefore useful to test not only unproved conjectures but also deductively proven results by means of quasi-empirical exploration. Such testing also ought to be encouraged among our students rather than suppressed, because it may bring about new perspectives for further research or contribute to the refinement and/or reformulation of earlier proofs, definitions and

[6] Though Descartes already in 1639 knew of the invariance of the so-called "total angle deficiency" of polyhedra and Euler's formula can be derived from this, there is – according to Grünbaum and Shephard (1994:122) – no historical evidence that Descartes actually saw the connection.

concepts. The Lakatosian view therefore contrasts strongly with the traditional, rationalist view that a formal proof offers an absolute guarantee of a mathematical statement and that hence even a single practical check is superfluous.

However naive, casual or mathematically inexperienced readers of Lakatos often miss his important distinction between global counter-examples and "local" or "heuristic" counter-examples. Whereas the former, like those in the section above on global refutation, completely disprove a statement, the latter challenge perhaps only one step in a logical argument or merely some aspect of the domain of validity of the proposition. Most heuristic counter-examples are therefore not strictly logical counter-examples, because they are after all not inconsistent with the conjecture in its intended interpretation; however, they do spur the growth and refinement of knowledge heuristically.

Therefore, a heuristic counter-example only requires some reformulation of the theorem or its proof, usually leaving the original theorem relatively intact. In other words, the original conjecture (theorem) usually remains valid and true, not at all disproved though perhaps modified, refined and much better understood. An excellent example, if transformed into a learning activity, possibly accessible for senior high-school students but probably more appropriate for undergraduates, is described fully in De Villiers (2000).

In this case, a teacher and his students made the following conjectured generalisation of the Fibonacci series and developed its proof: "A series has the property $1+S_n=T_{n+k+1}$ if, and only if, it is generated by the rule $T_n+T_{n+k}=T_{n+k+1}$, where S_n is the sum to n terms and T_n is the nth term". However, after the surfacing of heuristic counter-examples, the class reformulated the result together with corrected proofs, more precisely as: "If T_n is the nth term and S_n is the sum to n terms of a series, then for all $n>1$: $T_{k+1}+S_n=T_{n+k+1} <=> T_n+T_{n+k}=T_{n+k+1}$".

Generally, mathematical theorems (and theories) exhibit a permanence often denied to proofs, which may change according to the prevailing rigour of the time. For example, Euclid did not prove (or even state as an axiom) the Jordan Curve Theorem – namely, that a closed curve like a circle or triangle has an inside and an outside. Nevertheless, this "hole" in Euclidean geometry does not destroy or invalidate Euclid's work.

However, recent tendencies to derive or develop a (radical) fallibilist philosophy of mathematics education, usually justified from an extreme Lakatosian perspective, are unfortunate. Surely, as mathematics educators, and mathematicians, we ought to know the danger of over-generalising from only one historical case (i.e., Lakatos' study of the Euler formula)!

Nevertheless, "radical fallibilism" appears to have become a dominant, fashionable ideology in current mathematics education (e.g., Ernest 1991; Borba and Skovsmose 1997), with its claim that all mathematics is potentially flawed and always open to correction. Apparently, its underlying, implicit assumption is that the Lakatosian process of proof and heuristic refutation can in principle carry on indefinitely. However, this assumption is really not historically supported. For example, Gila Hanna (1995; 1997) has pointed out that there are many historical cases where the mathematical development has been radically different from the heuristic refutation described by Lakatos. In fact the majority of our rich mathematical inheritance, at least at school and undergraduate level, can be regarded as "rock bottom", as Davis and Hersh (1983, p. 354) pointed out.

> Once a proof is 'accepted', the results of the proof are regarded as true (with very high probability). It may take generations to detect an error in a proof. If a theorem is widely known and used, its proof frequently studied, if alternative proofs are invented, if it has applications and generalizations and is analogous to known results in related areas, then it comes to be regarded as 'rock bottom'!

14.6 Understanding

As mentioned above, the experimental investigation and evaluation of already-proved results can sometimes lead to new perspectives and a deeper understanding or extension of earlier concepts and definitions. Indeed, it is a common practise among mathematicians, while reading someone else's mathematical paper, to look at special or limiting cases to help unpack and better understand not only the results but also the proofs.

As both practitioners and learners of mathematics we need examples to ensure we know what the words, symbols and notations of a proof mean. Ideally, if the abstract theorem applies in multiple places we use multiple examples from different contexts. Apart from deepening understanding, this also adds certainty to a complex proof. To practising mathematicians, doing some examples when reading a proof is not irrelevant. In fact, teachers ought to actively encourage their students to do so. Matching the proof and the working of the example make both clearer and more convincing. One source of possible 'error' in current mathematics is the occasional subtle change in the meaning of terms, symbols, etc. between one mathematical paper and another, which usually only becomes apparent upon examining a few special cases.

Experimentation can sometimes help us more rigorously define our intuitive concepts, in turn leading to new investigations in hitherto uncharted directions. For example, some years ago I was attempting to generalise the interior angle sum formula $(n-2)\bullet 180°$ for simple closed polygons to more complex polygons, with sides criss-crossing each other. In the process, I rediscovered that the concept of "interior" angles of some complex polygons was not intuitively obvious at all (De Villiers 1989). Indeed, I was surprised to find that some "interior" angles of certain crossed polygons are reflexive angles, and could actually lie "outside"!

This counter-intuitive observation would probably not have been possible without experimental investigation. It also helped me to rethink carefully the meaning of interior angles in such cases and eventually come up with a consistent, workable definition of interior angles.

Using this definition, I next made another surprising, counter-intuitive discovery; namely, that the interior angle sum of a crossed quadrilateral is always 720° (see Fig. 14.3a). Indeed, this specific example can be used as a simple, but authentic illustration of a heuristic counter-example, useful for creating productive cognitive

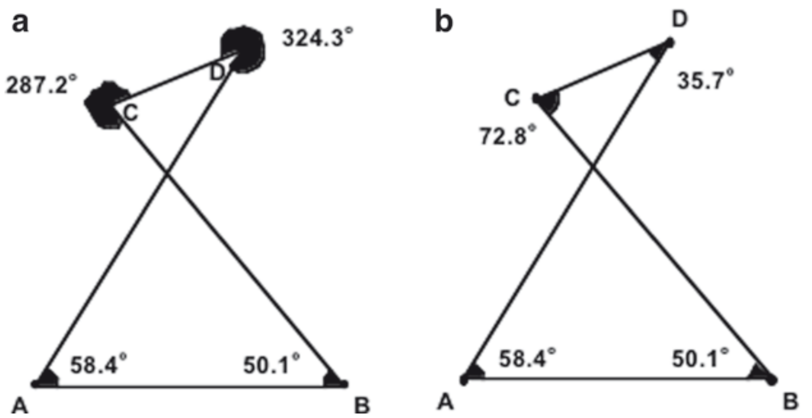


Fig. 14.3 Counter-example or monstser?

conflict in both high-school mathematics students, as well as mathematics teachers (De Villiers 2003, pp. 40–44).

In this learning activity, students who already know and have established the theorem that the sum of the interior angles of any quadrilateral is 360° are confronted with the type of figure shown in Fig. 14.3b. Almost without exception, the students' first reaction is "monster-barring" in defence of the theorem; that is, they bluntly reject such a figure as a quadrilateral. Most commonly, they argue that it can't be a quadrilateral since its angle sum is not 360°. To this argument, some students sometimes respond by saying that we could add the two opposite angles where the two sides *BC* and *AD* intersect, in order to ensure that the angle sum remains 360° (conveniently ignoring that they are now involving 6 angles!).

However, eventually students realise that the validity of the result that the angle sum of convex and concave quadrilaterals is 360° 'is not at stake here' – 'its validity is undisputed – but that we are choosing what to understand by the concepts "quadrilateral", "vertex" and "interior angle", and then to re-examine and define these concepts more precisely and use them in a consistent way. In fact, refutation by heuristic counter-example typically stimulates arguments about the precise meaning of the concepts involved evoking proposals and criticisms for different definitions of these until consensus is achieved (see Lakatos 1983, p. 16).

Mathematically, the situation posited above is then easily resolved by explicitly stating in the formulation of the theorem that for convex and concave quadrilaterals the angle sum is 360°, whereas for crossed quadrilaterals it is 720°, and doing separate proofs for the two possible cases.

Experimental investigation can also sometimes contribute to the discovery of a hidden clue or underlying structure of a problem, leading eventually to the construction or invention of a proof. For example, Fig. 14.4 shows equilateral triangles on the sides of an arbitrary triangle; the lines *DC*, *EA* and *FB* are concurrent (in the so-called Fermat-Torricelli point). Now noting, perhaps by dragging with dynamic geometry, that the six angles surrounding point *O* are all equal can assist one to

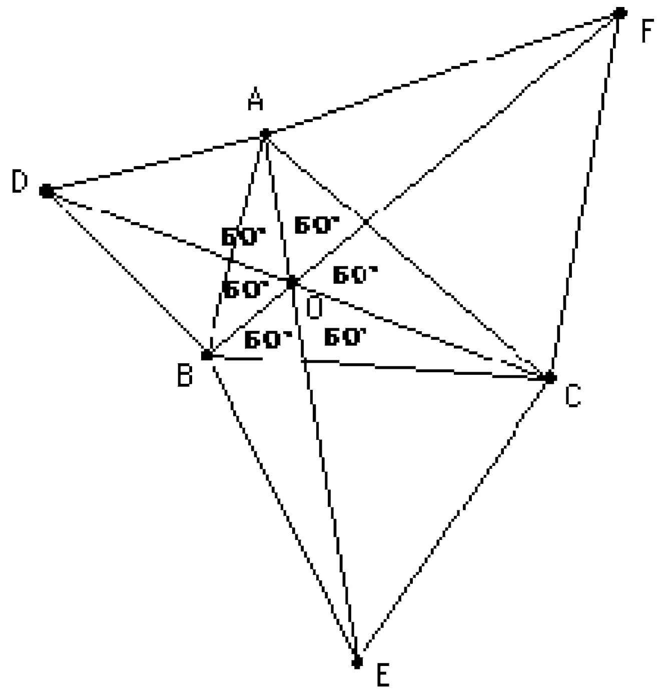


Fig. 14.4 Finding hint for proof

recognise *FAOC*, *DBOA* and *ECOB* as cyclic quadrilaterals (since in each case the exterior angle at *O* is equal to the opposite interior angle). This possibly sets one on the way to constructing a synthetic proof as follows: Construct circumcircles of triangles *BEC* and *CFA* and call their intersection *O*. Then it is easy, using the properties of cyclic quadrilaterals, to prove that *BOF* and *AOE* are straight lines, and that *DBOA* is also cyclic, and hence that *DOC* is also a straight line.

Polya (1968, p. 168) similarly argues that analogy and experimentation can contribute greatly to discovering and understanding proofs:

> ... analogy and particular cases can be helpful both in finding and in understanding mathematical demonstrations. The general plan, or considerable parts, of a proof may be suggested or clarified by analogy. Particular cases may suggest a proof; on the other hand, we may test an already formulated proof by how it works in a familiar or critical particular case.

14.7 Experimental-Deductive Interplay

In everyday research mathematics experimentation and deduction complement rather than oppose each other. Generally, our mathematical certainty does not rest exclusively on either logico-deductive methods or experimentation but on a healthy combination of both. Students should develop a healthy scepticism about both empirical evidence and deductive proofs in mathematics and learn to scrutinise both carefully. Intuitive thought and experimental experience broaden and enrich; they not only stimulate deductive reflection but also can contribute to its critical quality by providing heuristic counter-examples. Intuitive, informal, experimental mathematics is therefore an integral part of genuine mathematics (cf. Wittmann 1981:396).

Schoenfeld (1986, pp. 245–249) described how students used both quasi-empiricism and deduction to solve a problem:

> ... the most interesting aspect of this problem session is that it demonstrates the dynamic interplay between empiricism and deduction during the problem-solving process. Contributions both from empirical explorations and from deductive proofs were essential to the solution ... Had the class not embarked on empirical investigations ... the class would have run out of ideas and failed in its attempt to solve the problem. On the other hand, an empirical approach by itself was insufficient.

However, the limitations of intuition and experimental investigation should not be forgotten. Even George Polya (1954, p. v), famous advocate of heuristic, informal mathematics, warned that intuitive, experimental thinking on its own can be "hazardous" and "controversial". A good example, Cauchy, held to the intuition, popular in the eighteenth century, that the continuity of a function implied its differentiability. However, at the end of the nineteenth century, Weierstrass stunned the mathematical community by producing a continuous function that was not differentiable at any point!

Presumably inspired by Fermat's Last Theorem, Euler conjectured that there were no integer solutions to the following equation (see Singh 1998, p. 178):

$$x^4 + y^4 + z^4 = w^4$$

For 200 years, nobody could find a proof for Euler's conjecture nor could anyone disprove it by providing a counter-example. Calculation by hand and then years of computer sifting failed to provide a counter-example, namely, a set of integer solutions. Indeed, many mathematicians started believing Euler was right, and that it was probably only going to be a matter of time before someone came up with a proof. However, in 1988 Naom Elkies from Harvard University discovered the following counter-example:

$$2682440^4 + 15365639^4 + 18796760^4 = 20615673^4.$$

An even more spectacular example of the danger of relying on only quasi-empirical evidence is the following investigation, adapted from Rotman (1998, p. 3), that I regularly use with mathematics and mathematics education students:

Investigate whether $S(n) = 991n + 1$ is a perfect square or not. What do you notice? Can you prove your observations?

Systematic or random calculator or computer investigation for several n strongly suggests that $991n+1$ is never a perfect square. Even though some are already practising teachers, my students are usually easily convinced about the truth of this

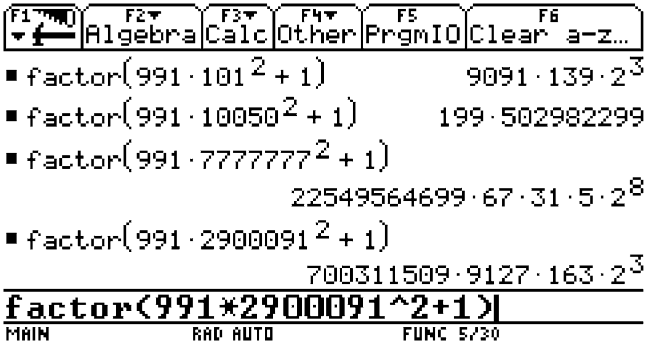


Fig. 14.5 Random calculator testing

conjecture, particularly after random testing of the conjecture with a wide range of numbers, including some very large ones on a TI-92 (see Fig. 14.5).

I then challenge the students to produce proofs; on occasion some have come up with ingeniously devised "proofs". Even after I point out the errors in their arguments, many students remain fully confident in the truth of the conjecture. So it comes as a great shock (and therefore an excellent learning experience!) when I later point out that the statement is true only for all n until[7]:

$$n = 12\ 055\ 735\ 790\ 331\ 359\ 447\ 442\ 538\ 767$$
$$\approx 1.2 \times 10^{29}$$

Despite having so much previous evidence – in fact, far more evidence than there have been days on earth (about 7.3×10^{12} days) – the conjecture finally turns out to be false!

This example also highlights a fundamental difference between mathematics and science of which our students at all levels ought to be made more aware; namely, that science in genera ultimately rests on empirical assumptions (even though deduction plays an important part in the mathematical sciences). We simply assume that the "regularities" we observe, like a stone falling to the ground or the sun rising every day, will always hold. What evidence do we have? None, except that as far as we know such has been the case for millions of years. But this is simply an empirical assumption, not a mathematical proof that it will always be the case.

Nobody today can really be considered mathematically educated or literate, who is not aware that quasi-empirical evidence alone does not suffice to guarantee truth in

[7] This is a specific case of a Pell equation, for which solutions were discovered as an offshoot of theoretical work rather than quasi-empirical testing. For example, one can see with a modest amount of experimentation that $x^2 - dy^2 = 1$ is solvable in positive integers when d is a small positive nonsquare integer, and infer (as Indian mathematicians did in the twelfth century) that it is probably solvable for more general d. This led to ad hoc algorithms that worked pretty well (Bhaskara managed the case $d = 61$), and finally to a theory that produced the present continued fraction treatment, which is guaranteed to churn out a solution (and will do so with $d = 991$ in fairly short order).

mathematics, no matter how convincing it may seem. Inculcating this awareness should therefore be a crucial aim in any mathematics education curriculum at the high school level and higher, and students ought concurrently to be led to experience proof as an empowering, liberating and highly intellectually satisfying endeavour (cf. Hanna 1997). Unfortunately, certain parts of the world have seen a marked decline in the teaching of proof at school level – in some cases, a virtually complete removal of proof, as in the United Kingdom from about the mid 1980s to mid-1990s. This decline perhaps in part results from the increased dominance in mathematics education of a radical fallibilist viewpoint apparently influenced by a superficial interpretation of Lakatos' statement (1983, p. 143) that proof is "the worst enemy of independent and critical thought". However, Lakatos was not criticising proof per se but traditional direct teaching of pre-existing proofs, which, without the proper balance of conjecturing and adequate experimental exploration, is indeed the enemy of independent and critical thought in the classroom.

Besides not providing sufficient certainty, experimental evidence seldom provides satisfactory explanations; that is insights into why something is true in mathematics. In other words, experimental investigation doesn't tell us how a result relates to other results nor how it fits into the general mathematical landscape. Largely for this reason, Rav (1999) has emphasised that proofs, rather than theorems, are in many respects the really valuable bearers of mathematical knowledge. As a result, students need to experience the value of deductive proofs in explaining, understanding, and systematising our mathematical results. In addition, we need to devise specific learning activities to show students how proving results may lead to further generalisations or spawn investigations in different directions, as Rav (1999) described.

The research mathematician Gian-Carlo Rota (1997, p. 190) has similarly pointed out, regarding the recent proof of Fermat's Last Theorem, that the value of the proof goes far beyond mere verification of the result:

> The actual value of what Wiles and his collaborators did is far greater than the mere proof of a whimsical conjecture. The point of the proof of Fermat's last theorem is to open up new possibilities for mathematics. ... The value of Wiles's proof lies not in what it proves, but in what it opens up, in what it makes possible.

Students ought also to be more regularly exposed to multiple quasi-empirical approaches to and multiple proofs of a particular result. Often, mathematicians have delighted in giving additional proofs of their own or other people's theorems. Clearly, the value of these largely involves examining multiple perspectives, gaining a deeper, richer understanding, or opening up for exploration a whole new range of possible analogies, connections, specialisations and generalisations. Moreover, if the only role of proof were to establish certainty, mathematicians would have no interest in alternate proofs (or further quasi-empirical investigations) of existing results or no greater preference for elegant proofs.

In addition, proof not only has a verification function, but also the important functions of explanation, discovery, communication, systematisation, and intellectual challenge (see my detailed discussion, De Villiers 1990, 2003). For example, good proofs enable us to explain and understand why results are true; proving a hard result is intellectually challenging, like solving a puzzle. Proof is also the accepted way of publishing and communicating mathematical results; uniquely, it allows us to systematise these results into axiomatic theories. Take my recent example of the discovery function of proof (De Villiers 2007). After experimentally discovering a result for hexagons with opposite sides parallel, and proving it, I realised upon reflection, that it not only immediately generalises to any hexagons but in fact to any 2*n*-gon! I would not likely have made this discovery by pure experimentation alone but was enabled to make it by the synergistic interplay between experimentation and proof.

14.8 Conclusion

It is simply intellectually dishonest to pretend in the classroom that conviction only comes from deductive reasoning or that adult mathematicians never experimentally investigate conjectures or already-proved results. Why deny students the opportunity to explore conjectures and results experimentally, when we adult mathematicians quite often indulge in such activities in our own research? Even though it may not produce any heuristic counter-examples, such exploration can still help students better understand the propositional meaning of a theorem.

We need to explore authentic, exciting and meaningful ways of incorporating experimentation and proof in mathematics education, in order to provide students with a deeper, more holistic insight into the nature of our subject. Teachers and curriculum designers face an enormous challenge: to illustrate and develop some understanding and appreciation of the functions not only of proof but also of experimentation, namely conjecturing, verifying, global and heuristic refutation, and understanding.

Acknowledgments Reprinted adaptation of article by permission from *CJSMTE*, 4(3), July 2004, pp. 397–418, http://www.utpjournals.com/cjsmte , © 2004 Canadian Journal of Science, Mathematics and Technology Education (*CJSMTE*) .

References

Aigner, M., & Ziegler, G. M. (1998). *Proofs from The Book*. New York: SpringerVerlag.

Arber, A. (1954). *The mind and the eye*. Cambridge: Cambridge University Press.

Borba, M. C., & Skovsmose, O. (1997). The ideology of certainty in mathematics education. *For the Learning of Mathematics, 17*(3), 17–23.

Davis, P. J. (1995). The rise, fall, and possible transfiguration of triangle geometry. *American Mathematical Monthly, 102*(3), 204–214.

Davis, P. J., & Hersh, R. (1983). *The mathematical experience*. Harmondsworth: Penguin Books.

De Villiers, M. (1989). From "TO POLY" to generalized poly-figures and their classification: a learning experience. *International Journal of Mathematical Education in Science and Technology*, *20*(4), 585–603.

De Villiers, M. (1990). The role and function of proof in mathematics. *Pythagoras*, *24,* 17–24 (A copy of this paper can be directly downloaded from http://mzone.mweb.co.za/residents/profmd/proofa.pdf).

De Villiers, M. (2000). A Fibonacci generalization: A Lakatosian example. *Mathematics in College,* 10–29 (A copy of this paper can be directly downloaded from http://mzone.mweb.co.za/residents/profmd/fibo.pdf).
De Villiers, M. (2002). From nested Miquel Triangles to Miquel distances. *Mathematical Gazette*, *86*(507), 390–395.
De Villiers, M. (2003). The Role of Proof with Sketchpad. In *Rethinking Proof with Sketchpad 4*. Emeryville: Key Curriculum Press. (A copy of this paper can be directly downloaded from: http://mzone.mweb.co.za/residents/profmd/proof.pdf).
De Villiers, M. (2005). A generalization of the nine-point circle and the Euler line. *Pythagoras*, *62,* 31–35.
De Villiers, M. (2006). A generalization of the Spieker circle and Nagel line. *Pythagoras*, *63,* 30–37.
De Villiers, M. (2007). A hexagon result and its generalization via proof. *The Montana Mathematics Enthusiast*, *14*(2), 188–192 (A copy of this paper can be directly downloaded from: http://www.math.umt.edu/TMME/vol4no2/TMMEvol4no2_pp.188_192_SA.pdf).
Epstein, D., & Levy, S. (1995). Experimentation and proof in mathematics. *Notices of the AMS, 42*(6), 670–674.
Ernest, P. (1991). *The philosophy of mathematics education*. Basingstoke: Falmer Press.
Fischbein, E. (1982). Intuition and proof. *For the Learning of Mathematics*, *3*(2), 9–18, 24.
Grünbaum, B., & Shephard, G. (1994). A new look at Euler's theorem for polyhedra. *American Mathematical Monthly*, *101*(2), 109–128.
Hales, T. C. (2000). Cannonballs and Honeycombs. *Notices of the AMS*, *47*(4), 440–449.
Halmos, P. (1984). Mathematics as a creative art. In D. Campbell & J. Higgens (Eds.), *Mathematics: people, problems, results* (Vol. II, pp. 19–29). Belmont: Wadsworth.
Hanna, G. (1983). *Rigorous proof in mathematics education*. Toronto: OISE Press.
Hanna, G. (1995). Challenges to the importance to proof. *For the Learning of Mathematics, 15*(3), 42–49.
Hanna, G. (1997). The ongoing value of proof. In M. De Villiers & F. Furinghetti (Eds.), *ICME-8 Proceedings of topic group on proof* (pp. 1–14). Centrahil: AMESA.
Hilton, P., & Pedersen, J. (1994). Euler's theorem for polyhedra: a topologist and geometer respond. *American Mathematical Monthly*, *101*(10), 959–962.
Hofstadter, D. R. (1985). *Metamagical themas: questing for the essence of mind and pattern*. New York: Basic Book Publishers.
Kramer, E. E. (1981). *The nature and growth of modern mathematics*. Princeton: Princeton University Press.
Kleiner, I., & Movshovitz-Hadar, N. (1991). The role of paradoxes in the evolution of mathematics. *American Mathematical Monthly*, *101*(10), 963–974.
Lakatos, I. (1983). *Proofs and refutations*, 4th reprint. Cambridge: Cambridge University Press.
Movshovitz-Hadar, N., & Webb, J. (1998). *One equals zero and other mathematical surprises*. Emeryville: Key Curriculum Press.
Neubrand, M. (1989). Remarks on the acceptance of proofs: The case of some recently tackled major theorems. *For the Learning of Mathematics, 9*(3), 2–6.
Oldknow, A. (1995). Computer aided research into triangle geometry. *The Mathematical Gazette, 79*(485), 263–274.
Polya, G. (1954). *Mathematics and plausible reasoning: induction and analogy in mathematics, vol I*. Princeton: Princeton University Press, *7*(1), 5–41.
Rav, Y. (1999). Why do we prove theorems? *Philosophia Mathematica, 7*(1), 5–41.
Rota, G.-C. (1997). The phenomenology of mathematical beauty. *Synthese, 111*, 171–182.
Rotman, J. (1998). *Journey into mathematics: an introduction to proofs*. NY: Prentice Hall.
Schoenfeld, A. (1986). On having and using geometric knowledge. In J. Hiebert (Ed.), Conceptual and procedural knowledge: The case of mathematics (pp. 225–264). Hillsdale, NJ: Lawrence Erlbaum.
Singh, S. (1998). *Fermat's last theorem*. London: Fourth Estate.
Wittmann, E. (1981). The complimentary roles of intuitive and reflective thinking in mathematics teaching. *Educational Studies in Mathematics, 12*, 389–397.

Chapter 15
Proof, Mathematical Problem-Solving, and Explanation in Mathematics Teaching

Kazuhiko Nunokawa

15.1 One Conception of Mathematical Problem Solving

In this chapter, "mathematical problem solving" is considered as an activity in the following way:

> *Mathematical Problem Solving* is a thinking process in which a solver tries to make sense of a problem situation using mathematical knowledge that he/she has, and attempts to obtain new information about that situation till he/she can resolve the tension or ambiguity about it. (Nunokawa 2005; see also Lester and Kehle 2003)

A problem situation is normally defined as a situation that cannot immediately be related to mathematical knowledge that can play an important role in the final solution. Therefore, problem solvers usually have to spend some time exploring the situation first, sometimes using heuristic strategies (Nunokawa 2000). In their explorations, solvers obtain new information about the situation (e.g. relations among the elements; new aspects or characteristics of the elements) and sometimes make sense of it using the solvers' mathematical knowledge (e.g. "Ah, these triangles are congruent to each other"), which may produce further information about the situation (Nunokawa 1998). Through such explorations, problem solvers can deepen their understanding of the problem situation, even though their understanding does not necessarily lead to the final solution. When problem solvers find that certain features of the problem situation can answer questions in hand and "resolve the tension or ambiguity," they have in mind (subjective) explanations (Giaquinto 2005), which may be formulated into solutions or proofs.[1]

K. Nunokawa
Department of Learning Support, Joetsu University of Education, Joetsu, Japan
e-mail: nunokawa@juen.ac.jp

[1]Corfield (1998, pp. 280–281) proposed that the hard core of mathematical research programmes includes aims of developing good understandings of targeted objects and its positive heuristics are composed of "favoured means" to achieve these aims. Similarly, in mathematical problem-solving processes discussed here, it is usually expected to develop good understandings through favored means.

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_15,

What counts as an acceptable mathematical explanation may depend on social factors of the community to which solvers belong (Ernest 1997; Yackel 2001). Furthermore, what can be seen as a problem to be explained, what can be taken for granted, or how far the work on justifying/validating the related results goes, can vary depending on the (teaching) context which solvers participate in at the time (Bergé 2006). Besides reflecting those social factors, mathematical explanations are expected to show us why the propositions in question are true or why certain mathematical phenomena occur in those situations (De Villiers 2004; Hanna 1995; Hersh 1997; Steiner 1978) and to make mathematical facts more intuitive (Giaquinto 2005). However, in order to do so, it is usually important for solvers to deepen their understanding of problem situations or objects of thought so that they can meet these aims (*cf.* Rav 1999). In the case of problems seeking a proof, this perspective on mathematical problem solving may be seen to be consistent with the notion of "proof as a means of insight" (Reichel 2002). While some researches (e.g. Neuman et al. 2000) included problem solvers' self-explanations as regulation of their actions ("I will do (or am doing) … in order to (or because) …"), the discussion in the rest of this chapter will be confined to the explanations about the mathematical phenomena or facts observed in problem situations. The purpose of this chapter is to reexamine explanation-building processes by relating them to problem solvers' understanding-processes and by referring to existing research studies which analyzed relationships between their exploration, understanding, and explanation in mathematical problem solving.

In the next section, the outline of the process of solving a proof problem will be presented. Then, some features of problem solving processes will be discussed referring to the analysis of this process and other related research. Finally, these features will be used to pose an elaborated conception of explanation-building processes, which will reveal further implications.

15.2 The Process of Solving a Proof Problem

The first example will show a process in which a solver deepened his understanding of a problem situation through his explorations and reached an explanation that satisfied him (Nunokawa 1997). I asked a graduate student to solve the following problem: "A given tetrahedron ABCD is isosceles, that is, AB = CD, AC = BD, AD = BC. Show that the faces of the tetrahedron are acute-angled triangles" (Klamkin 1988).

First, the solver sketched the problem situation (Fig. 15.1). Exploring the situation using this sketch, the solver noticed that all the faces are congruent and that it was enough to show that one of the faces is an acute triangle. After that, he said, "Try to open it up," and drew the nets of the problem situation (Fig. 15.2a–c). Although they were basically the same nets, the ways of drawing them were subtly different from each other.

While making these drawings, the solver said, "Can I make the tetrahedron using four congruent triangles?" and thought that the faces of the given tetrahedron can be any kind of triangle. Then, the solver drew a larger obtuse triangle to make a new net in the same way as Fig. 15.2c. He cut out that net from the worksheet and

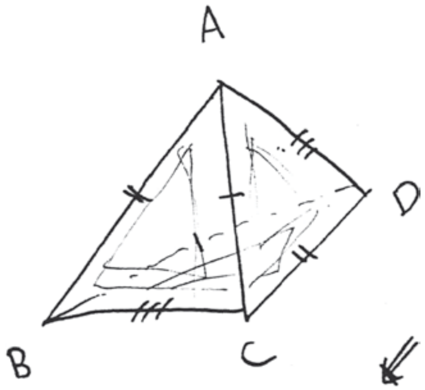


Fig. 15.1 Sketch of the Problem Situation

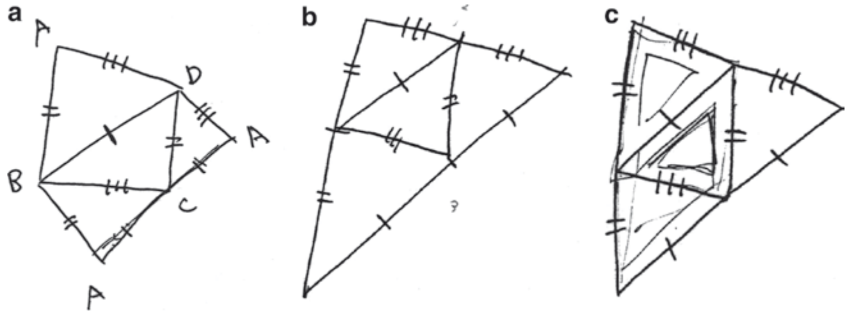


Fig. 15.2 Figure 15.2 (**a**) The problem solver drew four triangles. Then, he added the marks on the sides to represent the equality of opposite sides of the tetrahedron. (**b**) He drew one triangle and added three kinds of marks to its sides. Then, he drew another triangle with the marks on its sides, so on. (**c**) He drew one larger triangle. He drew a short line at the midpoint of one side and added the same mark to each of the halves of that side. He repeated the same operation on the other two sides. He connected those midpoints with lines and added marks to those lines to indicate equal sides

began to fold it (Fig. 15.3). In doing so, the solver noticed that two sides, α and β in Fig. 15.3, could “not stick to each other.”

The solver cut out another net, which was based on an acute triangle, and folded it to construct a tetrahedron. He said, “It is sufficient to pay attention only to the obtuse angle.” Finally, saying, “Examine the boundary case,” he cut out a right-triangle net and folded it (Fig. 15.4). In this case, the folded net became flat even though two sides could stick to each other. After opening and folding this net for a while, the solver said, “I’ve got it,” and began to draw Fig. 15.5 and write down his explanation on the worksheet: “Suppose that ΔBCD is an obtuse triangle, that is, $\angle BDC \geq 90°$. Since $\angle BDA + \angle CDA < 90°$, we cannot make a tetrahedron. Then, ΔBCD must be an acute triangle.”[2]

[2]As discussed next, this solution was backed by a certain operational image: opening the folded net which consisted of acute-triangles (Fig. 15.6). This proof may be a kind of picture proof (Brown 1997) with dynamic components.

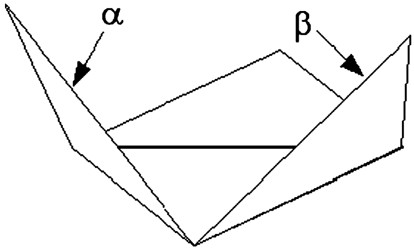


Fig. 15.3 Obtuse Triangle Case

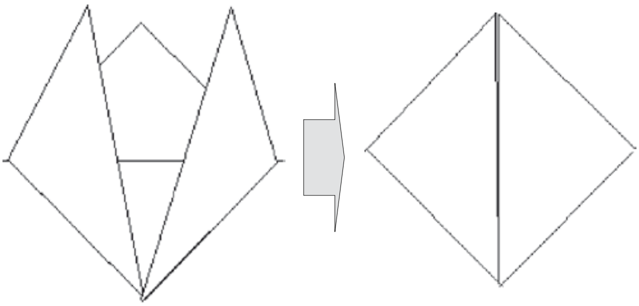


Fig. 15.4 Right Triangle Case

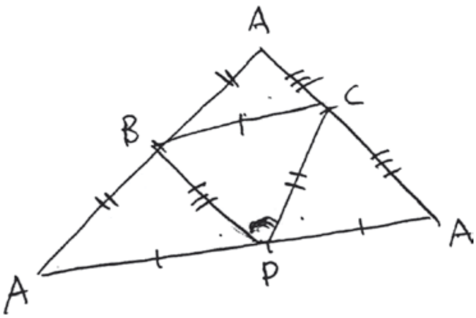


Fig. 15.5 Drawing for Writing the Solution

In the post-interview, the solver explained the reason why the acute-triangle net could become a tetrahedron by folding the net and gradually opening its overlapping part (Fig. 15.6).

As his problem solving proceeded, the solver gained more information about the problem situation concerning isosceles tetrahedrons, and deepened his understanding of the situation. The solver gradually realized that (1) the four faces are congruent; (2) the net can be made from one big triangle by connecting the midpoints of its sides; (3) the net cannot lead to a tetrahedron when four faces are congruent obtuse or right-angled triangles; (4) what is critical is not whether the sides can meet, but whether the faces of the net can overlap. He was able to build explanations based on this understanding. His final explanation reflected this, especially in terms of his final observation of the need for overlapping faces when the net was folded.

15.3 Explorations and Understanding

In this section, some features of the problem-solving process just described are discussed, focusing on the relationship between explorations and understanding.

15.3.1 Explorations Facilitate Understanding

Through his explorations of the problem situation, the solver obtained several pieces of information about the problem situation that were listed at the end of the previous section. Working on the problem situation deepened his understanding, for example, in the following ways.

1. Drawing the situation and operating on the cut-out nets led the solver to changing his view on what is essential in this situation: from the given condition that opposite sides are of the same length, to the property that the tetrahedron consists of four congruent triangles, to the property that the net of the tetrahedron can be folded so that the faces overlap. Here the solver used a certain property, which he found about the situation, as a basic characteristic that can define the situation (Nunokawa 1994b).
2. In using representations, an emergent pattern, which was not intended by the solver in advance, played an important role (Nunokawa 2006). When drawing Fig. 15.2b, the solver noticed that each pair of sides meeting at a vertex turned out to be one long line although he did not draw it intentionally. When the solver folded the right-triangle net, two sides stuck to each other but the net could not become a tetrahedron (Fig. 15.4). This pattern made him notice that it was not critical whether two sides stuck to each other, but whether the two faces overlapped. Consequently, it caused him to think of the movement shown in Fig. 15.6.

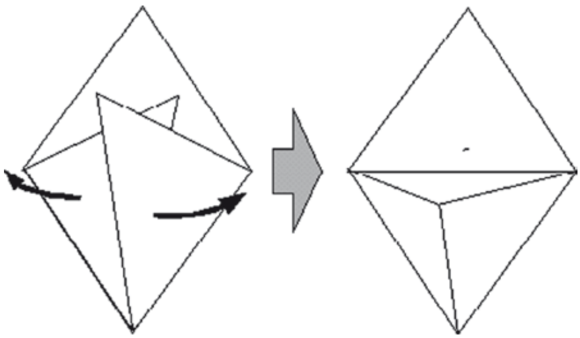


Fig. 15.6 Opening the Overlap

If we consider operating on representations (e.g. physical models) and observing the results to be a kind of experimentation in a mathematical problem solving task, the above analysis presents examples of the "understanding" function of experimentation and of how "mathematical intuition mostly develops from the regular handling, exploration and manipulations of mathematical objects and ideas" (De Villiers, in this volume).

15.3.2 Understandings Support Explorations

On the other hand, his explorations were supported by his understanding of the problem situation.

3. As his problem solving progressed, his ways of explorations also changed. When he found that four faces of the given tetrahedron were congruent and its net became one large triangle similar to each face, this understanding enabled the solver to manage his exploration, in particular, to examine acute-, obtuse- or right-triangle cases easily. The solver could make the net which he wanted to examine only by controlling the initial big triangle. As Figs. 15.1 and 15.2 show, the drawings also changed as his understanding of the problem situation progressed (Nunokawa 1994a, 2006). In fact, in order to make effective drawings, solvers need to understand problem situations to some extent beforehand (Nunokawa 2004). Moreover, interpretation of drawings can also be supported by solvers' understanding of problem situations. For example, when interpreting a drawing in one problem-solving activity, the solver used information obtained through an analytic geometry approach, although he adopted a plane geometry approach at that point (Nunokawa 1996). That is, some pieces of information about problem situations may be useful for supporting solvers' explorations even though they are not used in the final solution.
4. The solver showed doubt about the conclusion to be proved and this state of his understanding led him to examine the problem situation further. As mentioned above, while drawing Fig. 15.2, the solver thought that faces of the given tetrahedron could be any kind of triangle. He may not have understood the worthiness of proving the given conclusion because he was unsure whether it was correct. In this context, the solver examined the obtuse-triangle case by folding the net in that case. This examination made him notice his implicit assumption that, when folding the net, its sides can stick to each other and it will be turned into a solid. His noticing of the implicit assumption triggered the next exploration, in which he found the relationship between the angles of the faces and the possibility of constructing a tetrahedron. This relationship became one of the main ideas in his explanation. In some cases, solvers can realize why conclusions need to be explained and can be motivated to explore to seek explanations only after they understand the problem situation to some extent and realize that the conclusions are non-trivial (Nunokawa and Fukuzawa 2002). These features mean that solvers' understanding of the worthiness of proving conclusions, as

well as the modal qualifiers of conclusions (Inglis et al. 2007), can play an important role in exploring problem situations.

The features discussed in this section imply that while solvers' explorations deepen their understanding of problem situations, the states of their understanding support or influence their exploration activities. That is, there are interactions between explorations and understandings in mathematical problem solving.

15.4 Influences of Understanding and Explanations on Explorations

The previous section shows that an important aspect in mathematical problem solving is that solvers' understandings influence their explorations, as well as that solvers' explorations deepen their understandings. In this section, a few more patterns are presented concerning the former aspect. It is also observed that explanations, as organized understandings, influence subsequent explorations.

15.4.1 Understandings Clarify Bottlenecks

Solvers sometimes realize mathematical gaps underlying their explanations during warranting dialogues (Ernest 1997) with others or themselves. This realization can promote further exploration of the problem situation in order to fill those gaps. Similar processes can be observed even before solvers reach some explanations. As they understand problem situations, solvers may realize which part is a bottleneck that cannot immediately be overcome and set subgoals to explain that particular part (Nunokawa 2001). Newly-set subgoals may direct solvers' subsequent explorations.

15.4.2 Implicit Assumptions Sometimes Suggest Appropriate Directions of Explorations

Solvers sometimes "find" new properties of problem situations based on implicit assumptions they are not aware of. It is sometimes observed, however, that the information about these properties can trigger suitable directions of exploration even in such cases.

For example, when tackling another tetrahedron problem, a solver drew a net of a tetrahedron (Fig. 15.7) and was able to explain that the median lines of two adjacent faces are of the same length, e.g. AM = DM. Since M is the midpoint of BC, he "found" that quadrilateral ACDB is a parallelogram. This finding was based on the implicit assumption that the line AMD is a straight line, which he did not explain at any stage of his problem solving.

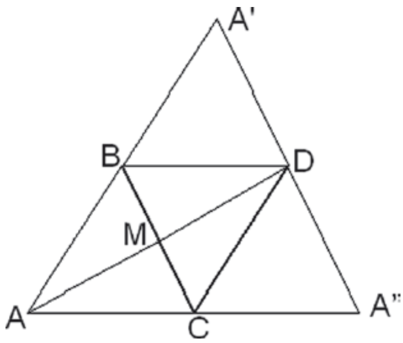


Fig. 15.7 Implicit Assumption in another Tetrahedron Problem

This unverified information made him consider that the polygon ACA″DA′B as one triangle (ΔAA″A′) with AMD as its median. The solver then tried to show that ΔAA″A′ is equilateral, and recalled the mathematical proposition that a triangle whose three medians are of the same length is equilateral, which could have been used in the solution of the problem (Nunokawa 2001). That is, exploration to explain that three medians are of the same length in a certain triangle can be considered a suitable direction to follow in this case, even though this exploration is based on an implicit assumption made by the solver.

15.4.3 Prospective Explanations Direct Explorations

Solvers sometimes imagine prospective explanations in advance, which might direct their subsequent explorations. The solver in Nunokawa (2004) showed such a solution process.

This solver tackled the following problem: "If A and B are fixed points on a given circle and XY is a variable diameter of the same circle, determine the locus of the point of intersection of lines AX and BY. You may assume that AB is not a diameter" (Klamkin 1988, p. 5). In the second half of his solution process, the solver proved that, in the case where AX and BY intersected outside the given circle (Fig. 15.8a), the locus of the intersection point Q becomes a circle because ∠Q is always equal to ∠P. Here, the point P is an intersection point in the special case where X coincided with A and ∠P acted as a fixed benchmark. Then, the solver tried to prove the similar property in the case where AX and BY intersected inside the given circle (Fig. 15.8b). He said, "I can do it in the same way," and began his exploration by searching for an angle which could be used as a benchmark in this case. In other words, when thinking about the second case, the solver had a prospective explanation at the outset and explored the problem situation in order to realize that explanation. Although such a benchmark existed (Nunokawa 2004), he could not find it. When he failed to find it, he almost gave up the idea that he had used successfully in the first case. Eventually, although he

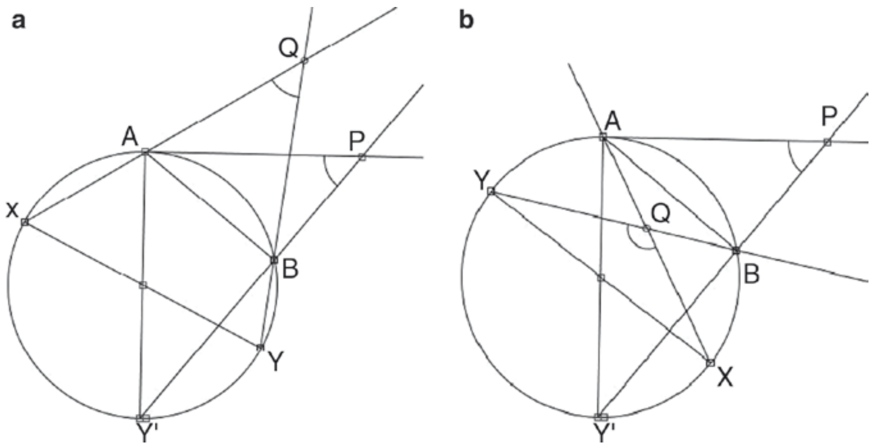


Fig. 15.8 Two cases of the Circle Problem

could not apply that idea to the second case directly, he found a way to reconcile that idea with the second case by showing that $\angle P + \angle Q = 180°$, instead of showing $\angle P = \angle Q$. Based on this, the solver proved that the locus also becomes a circle in the second case.

Here, the explanation found in the first case enabled the solver to have a prospective explanation, which suggested a direction of his exploration in the second case.

15.4.4 Explanations Generate New Objects of Thought to be Explored

When explanations of the phenomena can be obtained, it often becomes clear which aspects of situations or objects of thought are critical to those phenomena. Then we can loosen or exclude non-essential conditions (*cf.* Kvasz 2002) and consider more general problems, theorems or phenomena. De Villiers (2007) called this aspect of proofs or explanations a discovery function and pointed out that such discovery usually happen during the looking-back or reflective stage.

When I gave a lecture at a workshop for junior high school mathematics teachers a few years ago, I used the following problem: "There are two congruent squares. Put one of them on another so that a vertex of the former is located at the center of the latter, and rotate the former around the center of the latter (Fig. 15.9). Prove that the area of the overlapping part is constant." Of course, this problem can be solved by proving that since $\Delta OCM \equiv \Delta ODN$, the area of the overlap is always equal to one quarter of the area of the given square (Fig. 15.10a). After solving this problem, I asked nine of the teachers who participated in that workshop to develop other cases

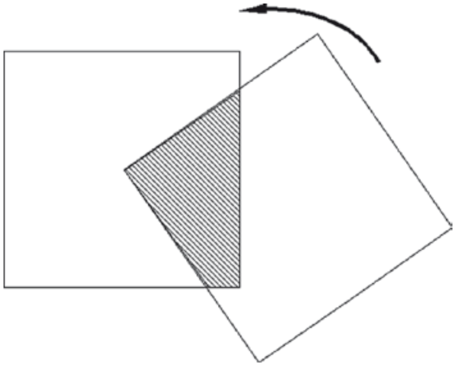


Fig. 15.9 The Problem Situation of the Two-Square Problem

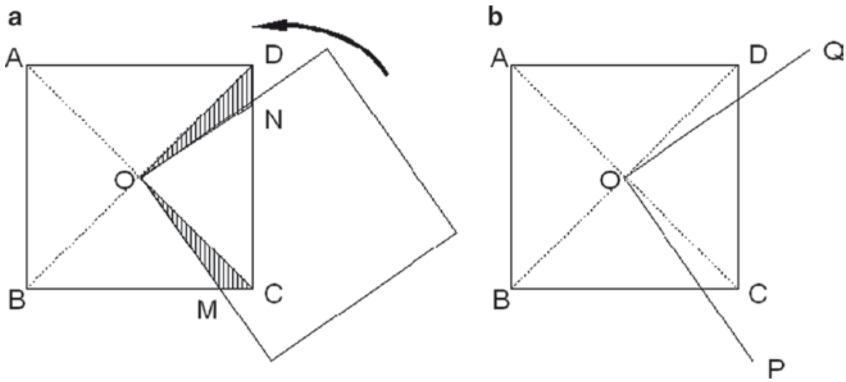


Fig. 15.10 The Mechanism of the Two-Square Problem

where a similar result can hold. While many of them drew two equilateral triangles or two regular pentagons and could not develop new cases, we can make such new cases by checking this proof further. What is essential to this proof is the angle of ∠POQ in Fig. 15.10b.

Because this angle is equal to the "center angle" of the given square (e.g. ∠COD), it can be proved that ∠COP = ∠DOQ. Therefore, if we can take two regular polygons so that (a multiple of) a "center angle" of one figure is equal to the interior angle of another, a combination of these polygons constitutes a similar situation (see Fig. 15.11a, b). If it is unnecessary to adhere to two regular polygons, we can use a polygon and a polygonal line to develop similar cases (Fig. 15.11c). This extension suggests that examination of proofs will not only produce "a proof of a related results" for other objects of the same "families" (Steiner 1983), but also generate a family itself of problem situations or objects of thought to which the original one belongs. Proofs or explanations generate such families and trigger new explorations in which those families become new objects of thought (e.g. how far can this proposition be extended).

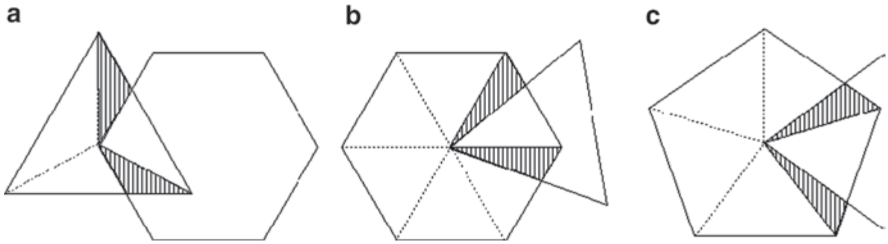


Fig. 15.11 New Family of the Situations

15.5 Explanations and Interactions between Explorations and Understandings

Explanations must be constructed based on solvers' satisfactory understandings of problem situations or objects of thought and their understandings may be deepened through their explorations of those situations or objects. As the above discussion shows, however, it is often observed in mathematical problem solving that solvers' understandings or explanations at a particular moment can direct or influence their explorations. Therefore, it is important to attend to interactions between solvers' explorations and their understandings. This implies that in analyzing the processes where solvers reach their explanations of the phenomena or the propositions in question, our attention should be focused on how solvers' understandings change or improve gradually during those processes, as well as how a state of their understandings at a certain stage enables the explorations they adopt.

On the one hand, some parts of solvers' understandings are validated by mathematical explanations. For example, when he noticed that four faces are congruent in Fig. 15.2, the problem solver validated this property by an explanation as follows: "they are congruent because corresponding sides are of the same lengths." On the other hand, as discussed above, there are parts of their understandings which are not mathematically validated such as the doubt about the worthiness of proving conclusions in problems, the information based on implicit assumptions problem solvers have, and the mere expectations of certain forms of explanations. Some aspects discussed in this chapter can be summarized in the following scheme, Fig. 15.12 where local explanations refer to those for partial properties and full explanation refers to that for the mathematical phenomena in question.

Pedemonte (2007) attempted to analyze "the entire resolution process" of solving proof problems and, in that analysis, paid attention to the structural relationships "between argumentation supporting a conjecture and its proof" (p. 39). The framework presented here directs our attention to the underlying elements which may support both argumentations and proofs (i.e. solvers' understandings of problem situations or objects) and suggests a kind of continuity between them (i.e. the deepening or modification of their understanding; see also Nunokawa (1996)).

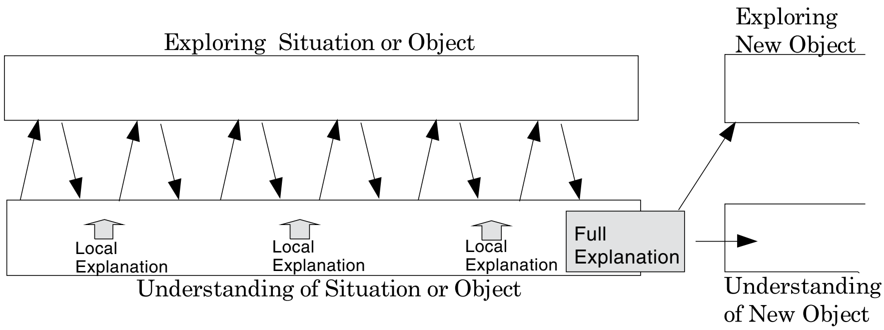


Fig. 15.12 Interactions between Exploration and Understanding in Explanation-Building Process

Proofs or full explanations often have critical ideas in them (e.g. the overlapping of faces in the example in Sect. 15.2, the congruent triangles in the example in Sect. 15.4.4). Following the above framework, it is important for us to investigate how such critical ideas can arise through interactions in Fig. 15.12. Even though such ideas may arise in solvers' "aha" experiences, it is also necessary to investigate how such ideas can emerge in relation to the solvers' understandings and explorations on the way to their full explanations. If mere accumulation of local explanations cannot necessarily lead to a full mathematical explanation or a solution to the problem, it is necessary to investigate how a full explanation or solution can result from local explanations and to pay attention to indirect relationships between full and local explanations as well as direct ones. As discussed in Sect. 15.3.2, even abandoned ideas may influence solvers' later explorations. Some issues can be posed from this standpoint. For example, most of the pictures presented as picture proofs (Brown 1997) or most of the diagrams teachers present to their students are usually final versions of diagrams. Following the above framework, it may be important to investigate how the final versions can emerge through interactions between explorations and understandings and what roles the immature versions of diagrams play in that process.

15.6 Concluding Remarks

Attending to interactions between solvers' explorations and understandings, which also include mathematically unverified information, may make it difficult to establish a clear-cut model of problem-solving or explanation-building. Seeing Epple (1998), "struggling to form a clear picture of the situation" (p. 336) and the use of "intuitive and highly informal techniques" (p. 381) are not unusual in authentic mathematical activities. Therefore, attending to those interactions can be considered an attempt to comprehend the richness and flexibility of human activities aimed at understanding targeted objects and making explanations of the phenomena in question.

References

Bergé, A. (2006). Convergence of numerical sequences: a commentary on "The Vice: Some historically inspired and proof generated steps to limits of sequences" by R. P. Burn. *Educational Studies in Mathematics, 61*, 395–402.

Brown, J. R. (1997). Proofs and pictures. *British Journal for Philosophy of Science, 48*, 161–180.

Corfield, D. (1998). Beyond the methodology of mathematics research programmes. *Philosophia Mathematica, 6*, 272–301.

De Villiers, M. (2004). Using dynamic geometry to expand mathematics teachers' understanding of proof. *International Journal of Mathematical Education in Science and Technology, 35*(5), 703–724.

De Villiers, M. (2007). A hexagon result and its generalization via proof. *The Montana Mathematics Enthusiast, 4*(2), 188–192.

De Villiers, M. (2008). The role and function of experimentation in mathematics. In this volume.

Epple, M. (1998). Topology, matter, and space I: topological notions in 19th-century natural philosophy. *Archive for History of Exact Sciences, 52*(4), 297–392.

Ernest, P. (1997). The legacy of Lakatos: reconceptualising the philosophy of mathematics. *Philosophia Mathematica, 5*, 116–134.

Giaquinto, M. (2005). Mathematical activity. In P. Mancosu, K. F. Jørgensen & S. A. Pedersen (Eds.), *Visualization, explanation and reasoning in mathematics* (pp. 75–87). Dordrecht, The Netherlands: Springer.

Hanna, G. (1995). Challenges to the importance of proof. *For the Learning of Mathematics, 15*(3), 42–49.

Hersh, R. (1997). Prove – once more and again. *Philosophia Mathematica, 5*, 153–165.

Inglis, M., Mejia-Ramos, J. P., & Simpson, A. (2007). Modelling mathematical argumentation: the importance of qualification. *Educational Studies in Mathematics, 66*(1), 3–21.

Klamkin, M. (1988). *USA Mathematical Olympiads, 1972–1986*. Washington, DC: MAA.

Kvasz, L. (2002). Lakatos' methodology between logic and dialectic. In G. Kampis, L. Kvasz & M. Stöltzner (Eds.), *Appraising Lakatos: mathematics, methodology and the man* (pp. 211–241). Dordrecht, The Netherlands: Kluwer Academic Publishers.

Lester, F. K., Jr. & Kehle, P. E. (2003). From problem solving to modeling: the evolution of thinking about research on complex mathematical activity. In R. Lesh & H. M. Doerr (Eds.), *Beyond constructivism: models and modeling perspectives on mathematics problem solving, learning, and teaching* (pp. 501–517). Mahwah, NJ: Lawrence Erlbaum.

Neuman, Y., Leibowitz, L., & Schwarz, B. (2000). Patterns of verbal mediation during problem solving: a sequential analysis of self-explanation. *Journal of Experimental Education, 68*(3), 197–213.

Nunokawa, K. (1994a). Improving diagrams gradually: one approach to using diagramsin problem solving. *For the Learning of Mathematics, 14*(1), 34–38.

Nunokawa, K. (1994b). Solver's structures of a problem situation and their global restructuring. *Journal of Mathematical Behavior, 13*(3), 275–297.

Nunokawa, K. (1996). A continuity of solver's structures: earlier activities facilitating the generation of basic ideas. *Tsukuba Journal of Educational Study in Mathematics, 15*, 113–122.

Nunokawa, K. (1997). Physical models in mathematical problem solving: a case of a tetrahedron problem. *International Journal of Mathematical Education in Science and Technology, 28*(6), 871–882.

Nunokawa, K. (1998). Empirical and autonomical aspects of school mathematics. *Tsukuba Journal of Educational Study in Mathematics, 17*, 205–217.

Nunokawa, K. (2000). Heuristic strategies and probing problem situations. In J. Carrillo & L. C. Contreras (Eds.), *Problem-solving in the beginning of the 21st century: an international overview from multiple perspectives and educational levels* (pp. 81–117). Huelva, Spain: Hergué.

Nunokawa, K. (2001). Interaction between subgoals and understanding of problem situations in mathematical problem solving. *Journal of Mathematical Behavior, 20*, 187–205.
Nunokawa, K. (2004). Solvers' making of drawings in mathematical problem solving and their understanding of the problem situations. *International Journal of Mathematical Education in Science and Technology, 35*(2), 173–183.
Nunokawa, K. (2005). Mathematical problem solving and learning mathematics: what we expect students to obtain. *Journal of Mathematical Behavior, 24*, 325–340.
Nunokawa, K. (2006). Using drawings and generating information in mathematical problem solving. *Eurasia Journal of Mathematics, Science and Technology Education, 2*(3), 33–54.
Nunokawa, K. & Fukuzawa, T. (2002). Questions during problem solving with dynamic geometric software and understanding problem situations. *Proceedings of the National Science Council, Republic of China, Part D: Mathematics, Science, and Technology Education, 12*(1), 31–43.
Pedemonte, B. (2007). How can the relationship between argumentation and proof be analyzed? *Educational Studies in Mathematics, 66*, 23–41.
Rav, Y. (1999). Why do we prove theorems? *Philosophia Mathematica, 7*(1), 5–41.
Reichel, H.-C. (2002). Lakatos and aspects of mathematics education. In G. Kampis, L. Kvasz & M. Stöltzner (Eds.), *Appraising Lakatos: mathematics, methodology and the man* (pp. 255–260). Dordrecht, The Netherlands: Kluwer Academic Publishers.
Steiner, M. (1978). Mathematical explanation. *Philosophical Studies, 34*, 135–151.
Steiner, M. (1983). The philosophy of mathematics of Imre Lakatos. *Journal of Philosophy, 80*, 502–521.
Yackel, E. (2001). Explanation, justification and argumentation in mathematics classrooms. In M. van den Heuvel-Panhuizen (Ed.), *Proceeding of the conference of the International Group of for the Psychology of Mathematics Education* (Vol. 1, pp. 9–24). Utrecht, The Netherlands: PME.

Chapter 16
Evolving Geometric Proofs in the Seventeenth Century: From Icons to Symbols

Evelyne Barbin

16.1 Proof to Convince and Proof to Enlighten

The seventeenth century marked a major change in the meaning of proof[1]. It was a time of widespread dissatisfaction with the geometric proofs of the Greeks. For instance, Arnauld and Nicole listed their objections to Euclid in *The Logic or the Art of Thinking* (1662), writing that Euclid is "more concerned with certainty than with evidence, and more concerned with convincing the mind than with enlightenment."[2] This criticism was a consequence of the development of new methods by geometers in the seventeenth century, in particular the method of Descartes to translate geometry into algebra. The question is whether these new methods could be regarded as proofs. Descartes and Arnauld considered the new methods enlightening because they ahowed explicitly how the results are obtained.

When Descartes introduced the use of algebra in his book on *Geometry* in 1637, it was not his explicit intention to rewrite Euclid. However, following Descartes' method of building "compound" ideas on "simple" ideas, Arnauld considered it to be against the "natural order" to prove Proposition 2 of Book VI of Euclid (on proportionnal lines and parallelism), which is a proposition about simple lines, by using triangles, which are compound ideas. He therefore considered Euclid's *Elements* to be "confused and muddled" and set out to replace the logical order of propositions in Euclid by a new "natural order" based on the cartesian method in his *New Elements of Geometry* (Arnauld 1667). This involved deducing compound things from simple things using simple relations. Simple things are straight lines, compound

E. Barbin (✉)
Centre François Viète d'épistémologie et d'histoire des sciences et des techniques, Université de Nantes, Nantes, France
e-mail: evelyne.barbin@univ-nantes.fr

[1] Barbin, E., 'The meanings of Mathematical Proof', *In Eves' circles*, The Mathematical Association of America, n°34, 1994, p. 44.

[2] Arnauld A., Nicole, P., *La logique ou l'art de penser*, Paris, PUF, 1965, p. 325.

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_16,

things include triangles and circles, while the simple relations include the four operations of arithmetic and the extraction of roots.

A few years later, Lamy published his *Elements of Geometry*, which also followed the method of Descartes, and subsequently gave an updated version of Thales proposition in a later edition.

Our purpose is to examine the treatment of magnitudes [lengths, areas, volumes] in these books. One of the major contribution of the geometry of Descartes is the "arithmetization of magnitudes" accomplished by introducing the notion of a unit in geometry. This makes it possible to perform calculations with magnitudes without the need to interpret them directly as numbers. The main question is how this calculation with magnitudes produces a proof that *enlightens*, rather than reproducing the kind of argument used in Euclidean geometry.

To compare Euclid with Arnauld and Lamy, we analyze five proofs of Thales' propositions on proportion. This proposition was given in two forms in Euclid and reformulated in the seventeenth century to state that, when four magnitudes are proportional, the product of the extremes is equal to the products of the means. We will examine the proof of Proposition 14 in Euclid's *Elements* Book VI, the proof of Proposition 19 in Euclid's *Elements* Book VII, the proof in Arnauld's *New elements of geometry* (1667), the proof in the second edition of Lamy's *Elements of geometry* (1695) and the proof in the fifth edition (1731).

In our analysis we use the terminology of Peirce to distinguish between icon, index, symbol, and diagram[3]. Peirce (1992–1998) gives various definitions in his writings; here we use the following: "An *icon* is a sign fit to be used as such because it possesses the quality signified."[4] For instance, Fig. 16.1 shows an icon for a parallelogram.

"An *index* is a sign which denotes a thing by focusing attention on it."[5] In Fig. 16.2, A, B, C and D are indices for the corners.

"A *symbol* is a sign that refers to the object that it denotes by virtue of a convention, usually an association of general ideas, which operates to cause the symbol to be interpreted as referring to that object."[6] For instance, in the discussion following, the letters AC are used as a symbol for the parallelogram in Fig. 16.2. "A *diagram*

Fig. 16.1 Icon for a parallelogram

[3] This classification is interesting to study mathematical writing and its understanding, but it is not often used. For instance, Fischbein (1993) uses psychological idea of mental images.

[4] Peirce, C. S., 'New Elements', 1904, *The Essential Peirce, Selected Philosophical Writings*. vol. 2, p. 307.

[5] Peirce, C. S. , 'The Regenerated Logic', 1896, *Collected Papers of Charles Sanders Peirce*, vol. 3, p. 434.

[6] Peirce, C. S., 'A syllabus of certain Topics of Logic', 1903, *The Essential Peirce, Selected Philosophical Writings*. Vol. 2, p. 292.

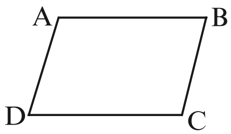


Fig. 16.2 Parallelogram with indices ABCD

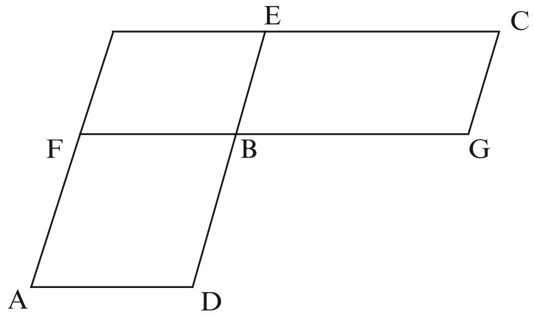


Fig. 16.3 Equiangular parallelograms

represents a definite form of relation. [...] A pure diagram is designed to represent, and to render intelligible, only the form of that relation. Consequently, diagrams are restricted to the representation of a certain class of relations; namely, those that are intelligible."[7] So, ideas of icon, index, symbol and diagram are respectively linked with those of resemblance, existence, convention, and relation.

16.2 Euclid's Book VI: Geometrical Icon and Diagrammatic Proof

Proposition 16 of Book VI states that "if four straight lines are proportional, the rectangle contained by the extremes is equal to the rectangle contained by the means." It is a consequence of Proposition 14 that states that "equiangular parallelograms, in which the sides about the equal angles are reciprocally proportional, are equal." "Let GB be to BF as DB to BE; I say that the parallelogram AB is equal to the parallelogram BC (Fig. 16.3). For since, as DB is to BE, so is GB to BF, while, as DB is to BE, so is the parallelogram AB to the parallelogram FE. (VI, 1) and, as GB is to BF, so is the parallelogram BG to the parallelogram FE. (VI, 1) therefore also, as AB is to FE, so is BC to FE; (V, 11) therefore the parallelogram AB is equal to the parallelogram BC." [8] This is a consequence of geometrical propositions, mainly the

[7] Peirce, C. S., 'Prolegomena for an Apology to Pragmatism', 1906, *The New Elements of Mathematics*, vol. 4, pp. 315–316.

[8] Euclid, *Elements*, translated by Heath, vol. 2, second edition, Dover, pp. 216–217.

first proposition of Book VI. By this proposition, DB is to BE as is the parallelogram AB to the parallellogram FE.

Peirce (1978) explains that deduction consists of contructing a diagram. He writes, "Every act of deductive reasoning, even simple syllogism, implies an element of observation. Indeed, deduction consists in constructing an icon or a diagram so that the relations between parts of this icon present a complete analogy with parts of the object of reasoning, so that experimentation on this image in imagination and observation of the result occur in such a way that we can discover relations not noticed and hidden in the parts."[9] Indeed, in Euclid's (1956) proof, there is an analogy between the deductive reasoning "as DB is to BE, so is parallelogram AB to parallelogram FE" and the observation of the geometrical icons AB and FE. There is also an analogy between the deductive reasoning "as GB is to BF, so is parallelogram BG to parallelogram FE" and the observation of the geometrical icons BC and FE. The conclusion arises from the relations between different parts of the geometrical icon we discovered: "as DB is to BE, so is GB to BF, as AB is to FE, so is BC to FE; therefore the parallelogram AB is equal to the parallelogram BC."

16.3 Four Proportional Numbers in Euclid's Book VII

In Book VII, Euclid (1956) considers proportional numbers A, B, C, D, in which A is to B as C is to D. Proposition 19 states that "if four numbers be proportional, the number produced from the first and fourth will be equal to the number produced from the second and third." "Let A, B, C, D be four numbers (Fig. 16.4) in proportion, so that, as A is to B, so is C to D; and let A multiplied by D make E, and let B multiplied by C make F; I say that E is equal to F. For let A multiplied by C make G. Since, then A multiplied by C makes G, and multiplied by D makes E; the

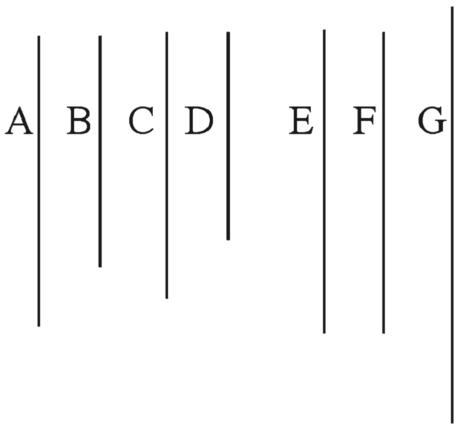


Fig. 16.4 Proportional numbers

[9]Peirce, C. S., 'On the algebra of logic: a contribution to the philosophy of notation', 1885, quotation in Peirce, *Écrits sur le signe*, Éditions du Seuil, Paris, p.146.

number A by multiplying the two numbers C, D make G, E. Therefore, as C is to D, so is G to E (VII, 17). But as C is to D, so is A to B; therefore also, as A is to B, so is G to E. Again, since A multiplied by C has made G, but further, B multiplied by C has also made F, the two numbers A, B by multiplying a certain number C has made G, F. Therefore, as A is to B, so is G to F (VII, 18). But further, as A is to B, so also is G to E; therefore also, as G is to E, so is G to F. Therefore, G has to each of the numbers E, F the same ratio; therefore E is equal to F (V, 9)."[10]

Here we have no icons, only symbols and indices. This proof is a consequence of arithmetical propositions, for instance, Proposition 17 of Book VII. By this proposition, if A multiplied by C gives G, A multiplied by D gives E, then as C is to D, so G is to E.

16.4 Euclid: Icons, Indices and Symbols for Straight Lines and Numbers

In Euclid's Books, signs for straight lines and for numbers are different. For a straight line, we have icon, index and symbol (Fig. 16.5).

For a number, we have a symbol and an index (Fig. 16.6).

A rectangle built from two straight lines AB and BC is given by an icon and a symbol (Fig. 16.7).

Fig. 16.5

Fig. 16.6

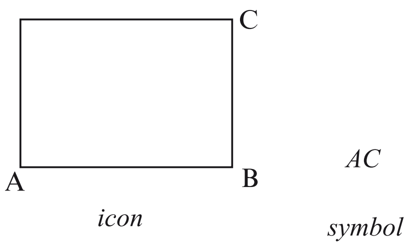


Fig. 16.7

[10] Euclid, *Elements*, translated by Heath, vol. 2, second edition, Dover, pp. 318–319.

But we have no icon and no symbol for the product of two numbers A and B (Fig. 16.8).

16.5 Arnauld: Multiplication of Magnitudes and Numbers

Peirce (1978) writes that "the reasoning of mathematicians principally rests on resemblances which are hinge-pins of the doors of their science."[11] In his *New elements of geometry*, Arnauld gives only one proposition for straight lines and numbers, because he establishes a resemblance between a rectangle and multiplication. He explains in Book I: "I suppose that multiplication can be applied to all magnitudes, and not only to numbers. Because, for example, we multiply length by width, when having a piece of ground of 4 *perches* for length and 3 for width, we say that this piece of ground has an area of 12 *perches*." [12](Fig. 16.9).

He gives as definition: "A plane magnitude is, for instance, the number 12, when we consider it is created from multiplication of 3 by 4"[13] (Fig. 16.10).

According to this resemblance, the same indices will be introduced for magnitudes and numbers. Arnauld writes, "I suppose that we are accustomed to conceive things by writing letters without seeking what they mean, because we use them only to conclude that *b* is *b,* that *c* is *c*, [...]."[14] There is one symbol for multiplication, "one

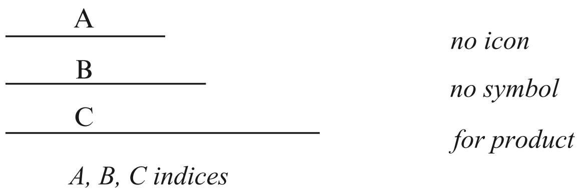


Fig. 16.8

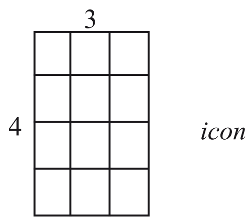


Fig. 16.9

[11] Peirce, C. S., 'The art of reasoning', 1895, quotation in Peirce, *Écrits sur le signe*, Éditions du Seuil, Paris, p. 151.

[12] Arnauld, A., *Nouveaux éléments de géométrie*, Savreux, Paris, 1667, p. 3.

[13] Arnauld, op. cit., p. 4.

[14] Arnauld, idem.

magnitude written with one character like *b* or *c* is called a linear magnitude. When we put them together as *bc,* it does not mean that they are added but that one is multiplied by the other, it is what we call the *product*."[15]

16.6 Four Proportional Magnitudes: Algebraic Icons and Algebraic Diagrams in Arnauld

Proofs of Arnauld use algebraic icons. He gives as definition, "when the ratio of an antecedent to its consequent is equal to the ratio of another antecedent to another consequent, this equality of ratios is called geometrical proportion, and the four terms proportionals. We say that as *b* is to *c,* so is *f* to *g* and we write *b* . *c* :: *f* . *g*."[16] So,

$$b\ .\ c :: f\ .\ g$$

is an algebraic icon. As Peirce (1978) notes: "every algebraic symbol is an icon because it shows, by means of algebraic signs, the relations of quantities in question."[17] For instance, proof of the second theorem uses icons, "when two magnitudes are multiplied by a same magnitude, they have the same ratio after being multiplied that they had before being multiplied."[18]

Arnauld examines two cases. In the first case, *b* and *c* have a common measure *x*:

$$b\ .\ c :: fb\ .\ fc$$

$$10x\ .\ 9x :: 10fx\ .\ 9fx$$

In the second case, *b* and *c* are not commensurable:

$$b\ .\ c :: fb\ .\ fc$$

$$10x\ .\ 9x + r :: 10fx\ .\ 9fx + fr$$

These icons allow manipulation of magnitudes and the conclusion comes from these manipulations. The importance of manipulation in algebra is emphasized by Peirce (1978): "For algebra, the idea of this art is that it presents a formula that we can manipulate and that by observation of the effects of this manipulation we discover properties which would be impossible to discern otherwise."[19]

[15] Arnauld, op. cit., p. 6.

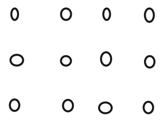
[16] Arnauld, op. cit., p. 26.

[17] Peirce, C. S., 'The short Logic', 1893, quotation in Peirce, *Écrits sur le signe*, Éditions du Seuil, Paris, p. 153.

[18] Arnauld, op. cit., p. 32.

[19] Peirce, C. S., 'On the algebra of logic: A contribution to the philosophy of notation, quotation in Peirce', *Écrits sur le signe*, Éditions du Seuil, Paris, p. 146.

Fig. 16.10 Multiplication: 3 × 4



icon

Arnauld's main proposition on proportion states that, "when four magnitudes are proportionals, the product of the extremes is equal to the products of the means."[20] It is not easy to prove, because Arnauld does not want use the complicated definition of equality of ratios that is given in Book V of Euclid's *Elements*. He introduces a way to prove it, which is a consequence of his first theorem that two magnitudes are equal when they have the same ratio to the same magnitude. Arnauld writes:

$b\ .\ c :: f\ .\ g$ by hypothesis

Therefore $bf\ .\ bg :: f\ .\ g$ by 44 sup.

$bf\ .\ cf :: b\ .\ c$ by 44 sup.

So $bg = cf$ by 43 sup.

These four lines constitute a diagram, because they render intelligible different relations between icons. Arnauld then writes, "because, by hypothesis, the ratio of $f\ .\ g$ (which is the same as the ratio of $b\,f\ .\ b\,g$) is equal to ratio of $b\ .\ c$ (which is the same as that of $b\,f\ .\ c\,f$) and consequently $b\,g$ and $c\,f$ have the same ratio with the same magnitude, $b\,f$. Consequently $b\,g$ is equal to $c\,f$."[21]

This proof is similar to the proof of Book VII of Euclid, but here we have algebraic icons and an algebraic diagram. His conclusion comes from an analogy between parts of reasoning and parts of a diagram. As Peirce (1976) writes, "the diagram not only represents the related correlates, but also, and much more definitely, represents the relations between them, as so many objects of the icon."[22] Here, the algebraic diagram uses a spatial disposition of signs.

Arnauld introduces algebraic diagrams for his proofs. For instance, in the first corollary he writes, "from this proposition, it is easy to judge all the changes we can do between four proportionals terms without them ceasing to be proportional."[23] Then, he gives this diagram Fig. 16.11.

Peirce (1976) notes associations between diagrams and icons. "A diagram is essentially an icon, in the form of an icon of intelligible relations. It is true that what must be is not to be found by simple inspection. But when we say that deductive reasoning is necessary, we do not mean, of course, that it is infallible. But precisely what we do mean is that the conclusion follows from the form of the relations set

[20] Arnauld, op. cit., p. 39.

[21] Arnauld, op. cit., p. 40.

[22] Peirce, C. S., 'Prolegomena for an Apology to Pragmatism', 1906, *The New Elements of Mathematics*, vol. 4, p. 316.

[23] Arnauld, op. cit., p. 42.

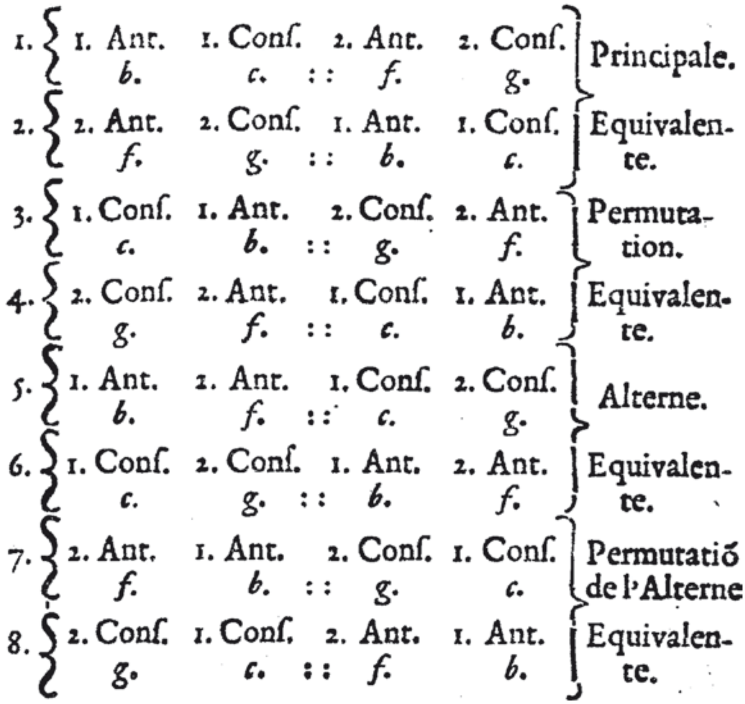


Fig. 16.11 Algebraic diagram for Arnauld's proof

forth in the premise."[24] This is the case in Arnauld, where the conclusion follows from a form of relations. Here, this is not a logical deduction of propositions, but a relational deduction of elements. This is typical of cartesian deduction[25] Logical deduction is a way to convince by discourse, but relational deduction may enlighten using the insight of a diagram.

16.7 Multiplication of Magnitudes in Lamy

In the second edition of his *Elements of geometry* in 1695, Lamy introduces a metaphor between rectangles composed of lines and the multiplication of numbers. He writes: "To multiply *a* by *b*, is to take *a* as many times that *b* has parts; and we mark it by joining this two letters *a b*."[26] Then he adds (Fig. 16.12), "When we mark two

[24] Peirce, op. cit., vol. 4, p. 531.

[25] Barbin, E., *La révolution mathématique du XVIIe siècle*, chapter VII, Ellipses, Paris.

[26] Lamy, B., *Les éléments de géométrie ou de la mesure du corps*, seconde edition, Pralard, Paris, p.124.

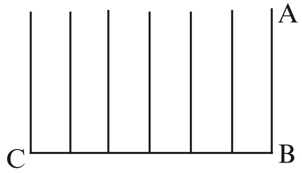


Fig. 16.12 Lamy's multiplication of magnitudes

lines by two letters, for instance, *a b* marks the multiplication of two lines AB and BC, we mean that these two lines make the rectangular shape ABC. It is evident that this shape is made by the motion of line AB moved from B to C, repeated or taken as many times as there are parts in BC."[27]

Following Peirce (1992–1998), here we say that we have a metaphor. Indeed, Peirce distinguishes three kinds of hypoicons (icons that can be any material image): an "image" has simple qualities, a "diagram" represents relations and a "metaphor" represents "a parallelism with something else."[28]

Lamy gives two symbols for multiplication of two straight lines. He writes, "To mark the multiplication of a line by a line, we have to use italics letters, and to mark each of these lines by only one letter, naming one *b* and the other *c*. The reason is that we usually denote lines by two capitals letters, as here, the line AB. Now this does not mean that A is multiplied by B, but only that A and B are the extremities of the line. The union of these two letters is not a sign of multiplication, to multiply the line AB by the line BC, we need another particular sign whose choice is arbitrary. We can multiply AB by BC putting a cross between them: AB × BC. This is the sign I use to express that AB is multiplied by BC."[29] The first symbol *a b* is used when we have indices for lines and the second symbol AB × BC is used when we have symbols for lines.

The first rule of Lamy establishes a parallel between algebraic icons and geometrical icons. First rule: when two given magnitudes each involve the sign +, their product must have the same sign +. We have to multiply $a+b$ by $f+g$. According to what we said about multiplication of simple magnitudes, we write $a\,f$, to denote the product of a by f; so making as many products as there are letters, we will have

$$a\,f + b\,f + a\,g + b\,g,$$

for the product of $a+b$ multiplied by $f+g$. Let $a+b=$AC, $a=$AB, $b=$BC. Let also $f+g=$AG, $f=$AH, $b=$HG. I suppose that ACEG is a rectangle cut by two parallels which make parallelograms ABIH, FGIH, BCDI, DEFI which are equal to ACEG, because the totality is equal to its parts (Fig. 16.13). Then it is evident that these four

[27] idem.

[28] Peirce, C. S., 'A syllabus of certain Topics of Logic', 1902, *The Essential Peirce. Selected Philosophical Writings*, vol. 2, p. 273.

[29] Lamy, op. cit., pp. 124–125.

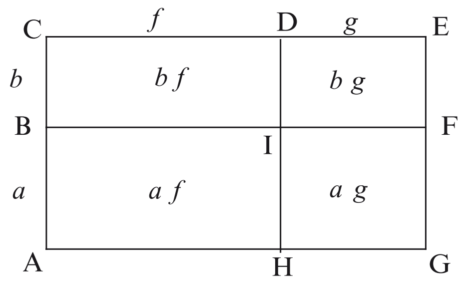


Fig. 16.13 Lamy's parallel between algebraic icons and geometrical icons

products $a\,f+b\,f+a\,g+b\,g$ are equal to the four parallelograms; they are also equal to AC × AG, which is parallelogram AC by AG, or $a+b$ by $f+g$.[30]

So, in the first place, using multiplication of algebraic icons, he obtains an algebraic icon, and in the second place, using a metaphor between rectangle and multiplication, he obtains a geometrical icon. In this way, we have two diagrammatic proofs with a parallelism between them.

16.8 Using metaphor: Book II of Euclid by Lamy

With his first rule, Lamy can obtain immediately all the propositions of Book II of Euclid by metaphor. Proposition 4 of Book II of Euclid states, "if a straight line be cut at random, the square on the whole is equal to the squares on the segments and twice the rectangle contained by segments. For let the straight line AB be cut at random at C; I say that the square on AB is equal to the squares on AC, CB and twice the rectangle contained by AC, CB."[31] So, Euclid gives a construction using a geometrical icon and a geometrical proof of the equality by a diagram.

Lamy explains that everything that Euclid teachs, he can make "visually evident." For instance, his Proposition 4 is as follows. "One straight line being cut in two parts, the square on the whole line is equal to squares of its parts, and two times the product of its parts. Let z a straight line whose a and b are parts so $z=a+b$. Multiply $a+b$ by $a+b$, the product $a\,a+2\,a\,b+b\,b$ will be the value of the square, which contains the squares $a\,a$ and $b\,b$ of the parts a and b of z and $2\,a\,b$ which is two times the product of its parts a and b."[32] Here, he obtains an algebraic proof by a diagram:

$$(a+b)\,(a+b) = a\,a + 2\,a\,b + b\,b$$

He deduces the geometrical icon of Euclid in terms of a metaphor by a parallel between a geometrical rectangle and an arithmetical multiplication.

---

[30] Lamy, op. cit., pp. 128–129.

[31] Euclid, *Elements*, translation Heath, vol. 1, p. 379.

[32] Lamy, op. cit., p. 135.

16.9 Comparison Between Arnauld and Lamy: Resemblances and Metaphors

It is interesting to compare the ways that Arnauld and Lamy use to go from icons to symbols and to see how these ways differ from a diagrammatic viewpoint.

Arnauld establishes a resemblance between two icons: an icon of a rectangle for the product of straight lines and icon for the multiplication of numbers. Because of this resemblance, he uses the same signs for lines and numbers and he has only one symbol for the product of straight lines and the product of numbers.

Lamy uses a metaphor by which a rectangle is taken as the product of two straight lines. There is a parallelism between, on the one hand, a geometrical icon and a diagram, and on the other, an algebraic icon and a diagram. So he introduces two symbols for multiplication. The first is for algebraic multiplication where lines are represented by indices: *a b*. The index *a* is a sign which indicates a line. The second one is for a geometric rectangle taken as a multiplication of lines represented by symbols: AB $\times$ BC where the symbol AB is a convention to represent a line by its endpoints.

16.10 Main Proposition in Lamy's Elements of 1695

Lamy uses the symbol of division of numbers to express a ratio of magnitudes. He writes: "Definition I. Ratio is the manner that a magnitude contains or is contained in the magnitude that we compare. To express a ratio, for instance, the ratio of *a* to *b,* we put *a* on a line, and *b* under in this manner

$$\frac{a}{b}.$$

This expression is natural because, as we saw before, it is the sign of division. Then division enables us to calculate how many times a magnitude is contained in another magnitude; so the sign of this operation expresses the value of a ratio which is called a quotient in arithmetic, which denotes the way in which one magnitude is contained in another."[33] It is important to note that this symbol represents not a ratio between two objects but a value for the operation of division.

However, Lamy uses an icon for proportion: "Definition IV. Equality of ratios is called proportion. If there is the same ratio for A to B as for C to D, we say there is the same proportion between these four magnitudes, or that there are proportionals; we can denote this with four points[34]

A B : : C D"

[33] Lamy, op. cit., pp. 139–140.

[34] Lamy, op. cit., p. 140.

This icon is similar the usual notation A : B :: C : D.

The main proposition on proportion is given in Theorem VI: “When four magnitudes are proportionals, the product of extremes is equal to product of means.” The proof is as follows. “Let A B :: C D, then I say that A × D = B × C and I will prove it. Multiplying A and B by D, we make two products or planes, and, by Proposition 52, A × D B × D:: A B. In the same manner, multiply C and D by B, then B × C B × D :: C D. Consequently the ratio between B × C and A × D is the same as the ratio with B × D, they are equal. (50).[35] “ Lamy uses Proposition 52 which establishes that if we multiply A and B by X then AX BX :: A B. To justify this assertion, Lamy explains once more that multiplication is a kind of addition. So if A is, for instance, three times B and X equals 6, “it is evident” that AX is three times BX. We see that Lamy’s proof is similar of Arnauld’s proof but without an algebraic diagram.

16.11 Main Proposition in Lamy’s Elements of 1731

In the fifth edition of his book, Lamy names the value of a ratio(in French) as its *exposant*[36]. “Definition I. The ratio of a line to a line [and so on] is the number of times that a line contains or is contained in the line compared; [etc]. We know how many times a line is contained in another one by division. So to express the ratio of a line to another, as the line *A* to line *B,* we divide *B* by *A*, writing one under the other,

$$\frac{B}{A} \text{ or } \frac{A}{B}.$$

This expression shows or expresses the ratio of *A* to *B,* [...], and is called the *exposant* of the ratio of *A* to *B*.”[37]

He gives the usual icon to represent a proportion: “Definition IV. The equality of ratios is called proportion. If there is same ratio of *A* to *B* as of *C* to *D*, we say these four magnitudes are proportionals, which we write thus:

$$A \,.\, B :: C \,.\, D\text{'}$$

But now, as he has given a name to the value of the ratio, he can give also a diagram: “We said that the ratio of A to B is expressed by $\frac{A}{B}$ so the ratio of C to D in same manner is $\frac{C}{D}$. Consequently the proportion of these four lines can also be expressed by

---

[35] Lamy, op. cit., p. 150.

[36] The French word «exposer» comes from the latin «exponere», it means to show or to exhibit. So, here Lamy introduces the word «exposant» to exhibit the ratio of two lines by an expression, which «shows» it.

[37] Lamy, *Géométrie ou de la mesure de l’étendue*, Nion, Paris, 1731, p. 153.

$$\frac{A}{B} = \frac{C}{D}.$$

So, he replaces an icon, which expresses a resemblance, by a diagram, which expresses a relation.

The statement of the main proposition on proportion is: "Proposition XV. When four magnitudes are proportionals, the product or rectangle of the extremes is equal to the product of the means." Lamy gives a new proof for this proposition: "*A . B* :: *C*. *D*. We have to prove that A × D=B × C. Let x be the *exposant* of the two equal ratios: so $A\ x=B$ and $C\ x=D$; so I can express this proportion in the form A . $A\ x$ :: C . Cx. We have to determine that $A\ Cx=A\ Cx$; which is evident."[38] The proof arises by expressing the *exposant* of a ratio by an index x.

If we compare these two proofs of Lamy, we can observe an evolution in the metaphor between ratio and division. In the second edition, Lamy explains that a ratio is the manner that a magnitude contains or is contained in another magnitude and that division enables us to know how many times a magnitude is contained in another magnitude. So the expression

$$\frac{a}{b}$$

is natural because it is the sign of division. In the fifth edition, he uses the same sign, but he also names this expression, as *exposant*. So, in this fifth edition he can replace the usual icon for proportion

A . B :: C . D

with a diagram that express the equality of two *exposants* :

$$\frac{A}{B} = \frac{C}{D}.$$

His new proof consists in representing this common *exposant* by an index, as we do when we manipulate numbers. As we said already, "arithmetization of magnitudes" does not mean that magnitudes are numbers, it means that it is now possible to perform calculations with them.

16.12 Conclusion

Calculation with magnitudes is a consequence of the arithmetization of geometry by Descartes. This calculation is accomplished by Arnauld and Lamy in two different ways. Arnauld is close to Descartes: he uses algebraic symbols to represent straight

---

[38] Lamy, op. cit., p. 163.

lines, he uses resemblances between geometrical icons for rectangles and for the multiplication of numbers he uses a similar index for straight lines and numbers. The calculation of Lamy is more radical, because, by a metaphor between rectangles and multiplication of numbers, he establishes a parallelism between magnitudes and numbers.

It is clear that with their calculation on magnitudes, Arnauld and Lamy avoid many of the difficulties of Book V of Euclid. The problem remains to know whether their reasoning can be taken as legitimate proofs by them and by their readers. The use of the cartesian word "evident" by Arnauld and, above all, by Lamy furnishes an answer. Their proofs are evident and so they are sure, because as the Italian Nardi says, "All evidence is certain, but all certainty is not evident." Descartes also declares in his *Discourse on Method*, "All we conceive very clearly and very distinctly to be true." Peircian analysis with icons and symbols shows what kind of evidence is required here, because, of course, the idea of evidence is not always the same in history.[39]

References

Arnauld, A. (1667). *Nouveaux éléments de géométrie*. Paris: Savreux.

Arnauld, A., & Nicole, P. (1662, reissued 1965). *La logique ou l'art de penser*. Paris: PUF.

Barbin, E. (1994). The meanings of mathematical proof. In J. M. Anthony (Ed.) *Eves' circles*. The Mathematical Association of America, *34*, 41–52.

Barbin, E. (2000). The historicity of the notion of what is obvious in geometry. In V. Katz (Ed.) *Using history to teach mathematics*. The Mathematical Association of America, *51*, 89–98.

Barbin, E. (2006). *La révolution mathématique du XVIIe siècle*. Paris: Ellipses.

Descartes, R. (1637). *La Géométrie*. Reprinted in facsimile with English translation by D. E. Smith, & M. L. Latham (1954) as *The Geometry of Descartes*. New York: Dover.

Euclid (translated by Heath, second edition) (1956). *Elements*, 3 volumes. New York: Dover.

Fischbein, E. (1993). The theory of figural concepts. *Educational Studies in Mathematics, 24*(2), 139–162.

Lamy, B. (1695). *Les éléments de géométrie ou de la mesure du corps*, 2nd edn. Paris: Pralard.

Lamy, B. (1731). *Géométrie ou de la mesure de l'étendue*, 5th edn. Paris: Nion.

Peirce, C. S. (1931–1958). *Collected Papers of Charles Sanders Peirce*, 8 volumes. Cambridge: Harvard University Press.

Peirce, C. S. (1976). *The new elements of mathematics, 4 volumes*. La Hague: Mouton Publishes.

Peirce, C. S. (1992–1998). *The Essential Peirce, Selected Philosophical Writings*, 2 volumes. Bloomington and Indianapolis: Indiana University Press.

Peirce, C. S. (1978). *Écrits sur le signe*. Paris: Éditions du Seuil.

[39] Barbin, E., "The historicity of the Notion of What is Obvious in Geometry', in *Using history to teach mathematics*, Katz, V. ed., The Mathematical Association of America, Notes 51, 2000, pp. 89–98.

Chapter 17
Proof in the Wording: Two Modalities from Ancient Chinese Algorithms

Karine Chemla

17.1 Introduction

The earliest extant Chinese mathematical documents do not contain theorems, but rather algorithms, most of which – though not all – were presented in relation to problems. This holds true for writings that came down to us through two different channels. Some of these writings are known only through manuscripts excavated in the twentieth century from tombs in which, in the last centuries B.C.E, they had been buried with their owners. This is the case with the *Book of Mathematical Procedures* (算數書, *Suanshushu*), found in 1984 in a tomb sealed before *circa* 186 B.C.E.[1] Other writings were handed down through the written tradition, for example, *The Nine Chapters on Mathematical Procedures* (九章算術, *Jiuzhang suanshu*), which dates to the first century C.E. [2] Two early commentaries on *The Nine Chapters* were also handed down together with it until today. In fact, there is no ancient edition of *The Nine Chapters* that would not contain the commentary completed by Liu Hui (劉徽) in 263 or the supra-commentary on the two layers of text presented to the throne in 656 and composed by a group of scholars led by Li Chunfeng (李淳風).[3]

---

[1] Compare the critical edition with annotations in Peng Hao (彭浩 2001).

[2] Below, I shall abbreviate the title into *The Nine Chapters.* For a critical edition and a French translation of this book and its earliest commentaries, compare Chemla and Guo Shuchun 2004. Chapter B, by Guo Shuchun, discusses the opinions of several scholars regarding the time period when *The Nine Chapters* was compiled. In my introduction to chapter 6 in the same book, I argue for dating the end of the compilation to the first century C.E. (Chemla and Guo Shuchun 2004: 475–481).

[3] Below, we refer to this layer of the text as "Li Chunfeng's commentary." Two other supra-commentaries, composed during the Song dynasty, respectively in the eleventh and the thirteenth century, survived only partially. They were not handed down systematically with the collection, by that time coherent, that *The Nine Chapters* and the two earlier commentaries formed.

K. Chemla
REHSEIS, UMR 7219 SPHère, University Paris Diderot & CNRS, Paris, France

G. Hanna et al. (eds.), *Explanation and Proof in Mathematics: Philosophical and Educational Perpectives,* DOI 10.1007/978-1-4419-0576-5_17,

As a consequence, in these writings, mathematical proofs did not take the form of proofs of the truth of theorems but rather that of proofs of the correctness of algorithms. Whether the algorithms related to geometrical, algebraic or arithmetical questions, the proofs established that both the meaning of the result and the value yielded corresponded to the magnitude sought.[4] Hence, the texts give us an opportunity to think about proofs of the correctness of algorithms, a kind of proof so far seldom examined in discussions about mathematical proof.[5]

What kind of evidence do we have in these ancient Chinese writings regarding such proofs? The commentaries that Liu Hui and Li Chunfeng developed in relation to virtually every procedure of *The Nine Chapters* systematically established the correctness of the procedures. They provide ample evidence with respect to how such a proof was conducted; they have been abundantly studied in the past decades.[6] However, the two commentaries indicate another type of evidence, more complex from a methodological point of view. Recently, I have been struggling with the idea that the commentators were sometimes "reading" their proofs in the way in which the texts for the algorithms were formulated in *The Nine Chapters*.[7] In fact, many hints indicate that *The Nine Chapters* regularly pointed out reasons for which the algorithms were correct in the very way in which the text for the algorithms in the book was written. This feature reveals that the relationship between the text of an algorithm and the text of a proof of its correctness is not as simple as we spontaneously assume. This issue is in fact part of a wider problem: namely, how the text of an algorithm is handled when the question of its correctness is addressed. For lack of space, I cannot deal systematically with the wider problem here. Rather, I shall concentrate on the question of *how* the text of an algorithm can in and of itself indicate reasons for that algorithm's correctness. The question is essential to address, if we want to delineate the evidence from ancient China on the basis of which to examine the history of the ways by which the correctness of an algorithm was addressed. The evidence from ancient China provides abundant source material to ponder with a certain generality the issue raised with respect to texts. In this paper, I shall concentrate on this evidence to clarify what it means that the text of an algorithm refers to a proof of its correctness.

---

[4] I introduced this distinction in Chemla 1996. I shall come back to it below.

[5] More precisely, when such proofs were analyzed, their analysis seldom aimed at determining the specificities of proofs, whose goal is to establish the correctness of algorithms. I have suggested elsewhere that once we understand better the history of such proofs, we might be in a position to formulate hypotheses regarding the part they played in a world history of mathematical proof and, more specifically, in a history of algebraic proof. However, in my view, we have not yet reached that point.

[6] It would be impossible to mention here the many papers and books that in the last decades were devoted to the proofs contained in the commentaries. Let me simply evoke: Li Yan (李儼 1958: 40–54); Qian Baocong (錢寶琮 1964: 62–72); Wu Wenjun (吳文俊 1982), Li Jimin (李繼閔 1990); Guo Shuchun (郭書春 1992); Wu Wenjun (吳文俊), Bai Shangshu (白尚恕), Shen Kangshen (沈康身) and Li Di (李迪 1993). For a fuller bibliography, refer to Chemla and Guo Shuchun 2004. In general, the publications seldom analyze the proofs from the viewpoint that they establish the correctness of algorithms. I have attempted to identify the main operations involved in the proof of the correctness of algorithms to which these commentaries bear witness in Chap. A of Chemla and Guo Shuchun 2004: 27–39.

[7] The first synthetical article that I devoted to this issue is Chemla 1991.

With this perspective in mind, I shall begin by briefly reexamining some source material from *The Nine Chapters* and its commentaries that I have analyzed in previous publications.[8] I shall then be in a position to illuminate two main families of techniques through which the text for an algorithm can refer to reasons for its correctness. Finally, I shall rely on this analysis to examine, from the same viewpoint, source material from the *Book of Mathematical Procedures*. Although the *Book of Mathematical Procedures* also makes use of the same two distinct kinds of techniques, the second technique is used differently than in *The Nine Chapters* and its commentaries. The final part of the article focuses on this latter technique, revealing similarities and differences in how, in these various writings, texts for algorithms refer to reasons for their correctness. The features examined thus help us bring to light differences between the two books that would remain unnoticed otherwise. Both those similarities and differences give clues to address an open question, that of the historical relationship between the *Book of Mathematical Procedures*, (recorded in a manuscript found in a tomb sealed at the beginning of the second century B.C.E.), and *The Nine Chapters*, (a book probably compiled in the first century C.E. and handed down). How can the differences highlighted between the two be accounted for? Do these differences indicate that these two writings emerged from distinct social milieus, or do they attest to an evolution in practice during the centuries between their composition. My analysis thus provides data that will help tackle the problem. Before we turn to considering these questions, however, some remarks on the text of an algorithm are in order.

17.2 A Few Words on the Texts for Algorithms

The problem of how the very text through which an algorithm is given refers to a proof of its correctness raises a fundamental issue, which we need to consider simultaneously: how does – or, more precisely, how did – one write a text for an algorithm? As Chinese sources illustrate, there are two types of reality corresponding to an algorithm.[9]

On the one hand, algorithms are given by means of texts recorded in books. These texts are commonly described as "sequences of operations." Moreover, they are usually qualified as "general," since they are valid not only for the problem in relation to which they are given, but for a class of similar problems. As a result, although at first sight they do look like "sequences of operations," we must be aware that the textual appearance of the sequence sometimes hides complex structures in the list of operations.[10]

[8] See Chemla 1991, 1996.

[9] The working seminar "History of science, history of text," organized with Jacques Virbel since 2002, and especially Agathe Keller's contribution, helped me clarify this dual dimension of an algorithm. It is my pleasure to express my gratitude to the group gathered around this seminar.

[10] See below for some concrete examples.

On the other hand, there is usually, outside the book, an instrument for computing – in ancient China, it was a surface on which numbers were written down with counting rods according to a place-value decimal system. On this instrument, the algorithm corresponds to actions performed, on actual values, transforming them until the result(s) appears.[11] Below, I shall discuss this dimension of the algorithm mainly on the basis of the specific example of the surface used for computations in ancient China. I shall refer to this dimension, when seen from the point of view of the events occurring on the instrument, as the "flow of computations," thereby stressing that these actions form a sequence over time.

Usually, the text by means of which an algorithm is written down corresponds to several distinct lists of actions that can be taken on the instrument. Depending on the values to which the algorithm is applied and depending on the cases with which the practitioner is confronted, the single general text for the algorithm generates the various sequences of actions required. That the text giving an algorithm corresponds to distinct lists of actions raises the questions of how the text achieves the integration of these sequences of actions and how it corresponds to the various computational flows generated. Different textual solutions to those problems appear in various writings of the past, even if we restrict ourselves to Chinese sources. This remark reveals that the question of how the text giving an algorithm corresponds to distinct lists of actions has a less straightforward answer than may be spontaneously assumed.

The text for an algorithm can be analyzed from another angle. Usually, we do not have a one-to-one correspondence between the terms referring to operations in the text and the actions taken on the instrument. Suppose a multiplication is to be carried out. The text can either prescribe the operation by a term, which thus corresponds to a series of actions on the instrument, or embed the details of a procedure for multiplying. We shall refer to this distinction by introducing the concept of the "grain of the description": The grain can be finer or coarser, depending on whether actions on the instrument are grouped in operations at a higher level or not. We can analyze how a text for an algorithm carries out the regrouping of elementary actions by means of terms referring to operations from two perspectives. On the one hand, we can examine the way in which actions are grouped within a single operation. On the other hand, we can analyze the terms chosen to prescribe this operation. In relation to the fineness or the coarseness of the description and to how coarseness is achieved, the text for an algorithm can convey different ways of conceptualizing the various flows of computation for the function corresponding to the algorithm. We shall see below, without exhausting the variety of cases that can be

[11] I owe this element of description of an algorithm, that is, the "action," to the presentation of the project "Histoire de la calculabilité" by M. van Atten, M. Bourdeau, and J. Mosconi (Final Conference of the Program of the CNRS and MESR: "Histoire des savoirs," November 29–December 1, 2007). The proceedings of the Program can be found at http://www.cnrs.fr/prg/PIR/programmes-termines/histsavoirs/synth2003-2007Histoiredessavoirs.pdf.

documented from the Chinese sources, that several techniques were used to achieve that goal. This is precisely one aspect by means of which a text can indicate reasons of the correctness.

The use in a given text of terms referring to a single operation, for instance a multiplication, allows giving a single prescription for sequences of actions that may differ, depending on the values to be multiplied. This remark reveals a relationship between the two features of a text that we distinguished: a coarser grain in the details given by the text with respect to the sequence of actions to be executed is one means through which a single text allows handling different cases, though not the only one.

I now turn to some concrete texts for algorithms from *The Nine Chapters* and its earliest commentary. In addition to illustrating the distinctions just introduced, these texts will allow me to elucidate *how* the text for an algorithm can indicate reasons for its correctness.

17.3 Texts for Algorithms – An Insight from *The Nine Chapters*

17.3.1 The Straightforward Reference to Operations and the Question of the Meaning

The first example of a text for an algorithm is paradigmatic in two ways: On the one hand, it prescribes operations in a direct way. On the other hand, its structure allows that along the sequence of operations, step by step, sub-procedure by sub-procedure, the meanings of the consecutive results are successively brought to light. Therefore, when the end of the text is reached, the meaning of the result can be made clear and can be shown to be precisely identical to that expected. It is thereby proved that the given algorithm yields the correct result.

In such types of texts for algorithms in *The Nine Chapters*, the commentator's proof amounts to establishing the meaning of the sequence of partial results until the end result is reached.[12] The commentator thus in some sense reads a proof in the *structure* of the text.

An excerpt that illustrates these phenomena is provided by the commentator Liu Hui. In it, Liu Hui writes down a text for an algorithm and at the same time, step by step, sub-procedure by sub-procedure, he provides an interpretation for each partial result. In some sense, he has merged the text of the algorithm and that of its proof into a single text. A formulation of that kind will make it easier for us to

[12] I describe a text of that kind for an algorithm as well as Liu Hui's proof of the correctness of the algorithm in Chemla 1991.

understand this type of text for algorithms and to suggest how these algorithms could be, on the one hand, obtained and, on the other hand, proved to be correct.

Our excerpt is the initial segment of an algorithm Liu Hui presents in his commentary after the first procedure given in *The Nine Chapters* to compute the area of a circle.[13] In a passage preceding the one we shall analyze, Liu Hui had established the correctness of the algorithm stated in *The Nine Chapters*, which prescribed multiplying half of the diameter of the circle by half of its circumference to yield the area. He then exposes the fact that the ratio of 1–3 between these two data, which characterizes the values given in the statements of the problems in *The Nine Chapters* – the diameter and the circumference–,[14] differs from the one that the algorithm assumes if it is to be correct. Consequently, despite the correctness of the algorithm, the problems in *The Nine Chapters* do not provide values that guarantee the exactness of the result of the algorithm. In this context, Liu Hui sets out to compute other values. We shall examine the beginning of the text by means of which he writes down his algorithm.

First, I shall sketch out the idea of the computation, which Liu Hui bases upon the drawing he referred to in his proof of the correctness of the procedure given by *The Nine Chapters* (see Fig. 17.1).[15] Liu Hui's whole text consists of the repetition

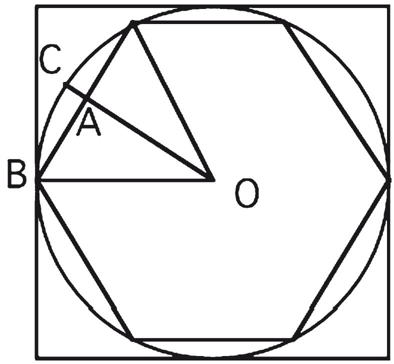


Fig. 17.1 The figure Liu Hui used to deal with the area of the circle

[13]I gave a more detailed analysis of the commentary on the area of the circle in Chemla 1996. For a critical edition and translation into French of the whole passage, see Chemla and Guo Shuchun 2004: 176–189.

[14]These are problems 1.31 and 1.32. The pair of numbers I attach to a given problem in *The Nine Chapters* refers, first, to the chapter in which it is placed (here, Chap. 1) and, then, to the order in which the problems are arranged in this chapter (here, 31st and 32nd problems). Note that these numbers are not part of the source material.

[15]Note that the diagram is restored on the basis of the references Liu Hui makes to its structure. However, I do not attempt to produce a figure conforming to the features known to be specific of the diagrams Liu Hui used. For instance, to conform to modern usage, I name some of the points. Before the thirteenth century C.E., we have no evidence in China of such ways of marking figures.

of a sequence of operations which, from the point of view of the computations carried out, corresponds to the iteration of a procedure computing the length of the side of a regular 2*n*-gon inscribed into the circle, when knowing the length of the side of a *n*-gon and the diameter of the circle. Once he has reached the accuracy he looks for, Liu Hui derives from the side of the *n*-gon just computed the value of the circumference and hence the value of the area of the corresponding 2*n*-gon. I shall focus on the initial description of the sequence of operations, which starts from the side of the regular hexagon inscribed in the circle.

As is known from Liu Hui's previous proof in the commentary on the area of the circle, the side of a regular hexagon is equal to half of the diameter of the circle in which it is inscribed. The first half of the sequence of operations to be repeated makes use of the fact that in the right-angled triangle OAB, both the base (*AB*) and the hypotenuse (*OB*) are known: they are, respectively, half of the side of the hexagon (more generally, the *n*-gon) and half of the diameter. Applying the "Pythagorean" procedure (the main topic of Chap. 9 in *The Nine Chapters*), one obtains the height OA. Thereafter, in the right-angled triangle ABC, given that the base is the difference between the radius and OA, and that the height is half the side of the *n*-gon, on the other hand, their values are known. In the second half of the procedure, applying again the "Pythagorean" procedure, one obtains CB, which is the side of the 2*n*-gon. One can then repeat the procedure to derive the length of the side of the 4*n*-gon, and so on.

Let us concentrate on how Liu Hui formulates this sequence of operations at the beginning of the excerpt. The first sentences of the procedure read as follows:

> "Procedure consisting in cutting the 6-gon in order to make a 12-gon: One sets up the diameter of the circle, 2 *chi*. One **halves it**, which **makes 1 *chi* and gives** the side of the 6-gon that is in the circle,"
>
> (割六觚以爲十二觚術曰：置圓徑二尺，**半之爲一尺**，**即**圓裏觚之面也。; my emphases).

The goal of the procedure is announced at the beginning of the text, in its name ("Procedure consisting in cutting the 6-gon in order to make a 12-gon"); the goal – and the name – will change at each repetition of the sequence of operations, from *n*-gon and 2*n*-gon to, in the next step, 2*n*-gon and 4*n*-gon, and so on. In the initial procedure aiming to cut the hexagon into a 12-gon, the side of which is to be determined, Liu Hui initiates the computation by prescribing that a value for the diameter, 2 *chi*, be "set up" – a technical term referring to placing, on the surface for computing, a value on which the subsequent computations will be executed. As is common in Chinese mathematical writings, the whole text is formulated with respect to a given set of numerical data but it has a paradigmatic value: The numerical values mentioned hold for any other possible initial data.[16]

Note that what is "set up," right at the outset, comprises not only the initial numerical datum, but also its "meaning": it is the diameter of the circle. This feature will

[16] Evidence supporting this claim is given in Chemla 2003.

hold true for the whole text: the prescription of each operation or each sub-procedure will be followed by a similar statement of its result. The value yielded by the operation or the sub-procedure, and the interpretation of the "meaning" of this result, will both be systematically given. Let me illustrate this point again by the next operation: halving the datum set-up. As we announced at the beginning of the section, the operation is prescribed directly, by means of a term naming the operation.[17] The statement of the result can be decomposed into two parts: numerically, the operation yields 1 *chi*; and, semantically, halving the diameter will be interpreted as yielding the side of the hexagon. The dual nature of the result is essential for my argument. Thus, the text of the algorithm mentions the evolution of the values computed, while also progressively providing a geometrical interpretation of the result for each step. Therefore, finally, the "meaning" of the algorithm's result will be determined. The correctness of the procedure is established only if the meaning of the result corresponds to the magnitude sought. To designate the nature of the interpretation of the final result, Liu Hui uses a specific term: 意 (*yi* "meaning").[18] The term also refers to the successive interpretations of the meanings of the results of the preceding sequence of operations and sub-procedures composing the algorithm. Taken altogether, the "meanings" form the reasoning establishing the algorithm's correctness. In the example, the second part of the result as formulated in the text ("the side of the 6-gon"), when taken from beginning till end of the algorithm, is precisely what constitutes Liu Hui's proof of his algorithm's correctness.

I shall refer to the algorithms for which such proofs can be formulated as having a "transparent" structure. While reading the subsequent sentences of Liu Hui's text, I shall analyze the conditions required to make the sequence of interpretations possible. In the case of the previous operation of halving, formulating the second part of the result requires interpreting the result with respect to the figure. Let us observe how in the next part of the procedure, Liu Hui makes the meaning of the operations explicit:

> "One takes half of the diameter, **1 *chi*, as hypotenuse**, **half of the side**, **5 *cun***, as **base (of the right-angled triangle)**, and one **looks for the corresponding height**. [19] The square of the base, 25 *cun*, being subtracted from the square of the hypotenuse, there remains 75 *cun*. One divides this by extraction of the square root[20] [...description of the computation of an

[17] This remark is important only because there are other modes of prescribing an operation that constitute another family of cases, in which the text of an algorithm refers to the reasons for its correctness (see below).

[18] I composed a glossary of technical expressions used in *The Nine Chapters* and its early commentaries (Chemla and Guo Shuchun 2004: 895–1042). In what follows, I shall refer to it as *Glossary*. It provides evidence for the meanings and facts regarding technical terms. For *yi* ("meaning, intention") see *Glossary*: 1018–1022.

[19] The terms I translate here by "base" and "height" are in fact technical terms referring, respectively, to the shorter and the longer sides of the right angle in a right-angled triangle.

[20] I follow the structure of the Chinese term for prescribing a square root extraction and underline, as the Chinese does, the link of that operation to division.

approximation in the form of a sequence of units concluded by a decimal fraction, in the end simplified…]. Consequently, one obtains 8 *cun* 6 *fen* 6 *li* 2 *miao* 5 and three-fifths *hu* for the **height**."

(令半徑**一尺爲弦，半面五寸爲句**，爲之**求股**。以句冪二十五寸減弦冪，餘七十五寸。開方除之，(…)。故得**股**八寸六分六釐二秒五忽五分忽之二。; my emphases).

Two magnitudes, and their corresponding values, are now available: that of half of the diameter, which was computed, and that of the side of the hexagon, which was introduced as an interpretation of the result of that computation. Half of the side can thus be computed. Note that the computation of the latter value, along with its meaning, is prescribed indirectly by a mere reference to the result: "half of the side, 5 *cun*." (For other examples of indirectly prescribing operations essentially different from this one, see below). Even though the values of half of the diameter and the side of the hexagon are equal, their interpretations as segments differ, indicating the geometrical work required to formulate the interpretation of the operation of halving, as "side of the hexagon," not "half of the diameter." Moreover, the choice between these two possible interpretations (both to be used in the next step) is essential to allow the sequence of interpretations to, in the end, reach an adequate meaning for the result of the algorithm. By providing distinct geometrical interpretations of the same value, Liu Hui situates them as specific kinds of segments on the figure. Further, by granting to these segments the names of, respectively, "hypotenuse" and "base," he not only situates them with respect to each other on the diagram but also designates the right-angled triangle in which they play such parts (triangle OAB).

Chapter 9 of *The Nine Chapters* contains a problem, which, given the hypotenuse and base of a triangle, asks for the "height." The problem is followed, and solved, by a form of the "Pythagorean" procedure, the correctness of which Liu Hui discusses in that context. By using the term "looking for 求 *qiu*," in the text presently under examination, Liu Hui signals that he identifies the situation he is dealing with as similar to that of the problem in Chap. 9. He thereby justifies inserting in his algorithm, after the operations of halving, the procedure given in Chap. 9 for finding a triangle's height. This section of his algorithm reads:[21] "The square of the base, 25 *cun*, being subtracted from the square of the hypotenuse, there remains 75 *cun*. One divides this by extraction of the square root (…computation of an approximation…).. Consequently, one obtains 8 *cun* 6 *fen* 6 *li* 2 *miao* 5 and three-fifths *hu* for the **height**" (emphasis mine).

This passage raises several issues related to our topic. First, note how the various operations are prescribed. As above, the squaring of the two known sides of the triangle is indicated by the statement of the result of the operation. By contrast, the terms by which the operations are prescribed (subtracting, dividing…) are common names for them.

[21] See the term "look for 求 *qiu*," in *Glossary*: 971. The corresponding problem and procedure in Chap. 9 appear in Chemla and Guo Shuchun 2004: 704–707.

Second, in contrast to the operation of "halving" discussed above, Liu Hui here prescribes the whole sub-procedure, of which only the final result is interpreted; there is no need to interpret explicitly the meaning of the subtraction or other steps. Depending on the reasoning that is formulated in the interpretations of the successive results, either the result of an operation or that of a sub-procedure is provided; the operations of interpretation sometimes group together distinct computations into a single whole, when this is relevant for establishing the meaning of what is thereby computed.

Further, let us observe how the interpretation is achieved. The identification of a problem and the insertion of a procedure, the correctness of which was already established, allows Liu Hui to formulate the meaning of the result as "height" of the corresponding triangle and to situate it on the diagram (OA). Thus, both the problems and the procedures attached to them play parts in composing the algorithm and formulating the meaning of its sub-procedures. More generally, as the commentators bear witness, problems and their procedures play a key part in the two activities of composing, sub-procedure after sub-procedure, a desired algorithm and interpreting the sequence of results. This was probably already the case for the authors of the procedures in *The Nine Chapters*, which consists precisely of textual units composed by a problem and a procedure.

Last, note that at this stage, the two components of the result no longer have the same relation to the situation under investigation: the interpretation of the result as "height" is an exact meaning for the magnitude yielded, whereas the value is only an approximation. The two parts of the result run in parallel but no longer represent exact counterparts of each other.

In sum, the text for the algorithm as formulated by Liu Hui describes a sequence of operations (dividing, squaring, etc). For each operation, a value is yielded (exact or approximate), whereas the interpretation is provided for operations or blocks of operations.

The second part of the sequence of operations examined here can be interpreted in exactly the same terms. It reads as follows:

> "One **subtracts** this (i.e., the height) from the half-diameter, 1 *cun* 3 *fen* 3 *li* 9 *hao* 7 *miao* 4 and three-fifths *hu* remains, that one calls **small base**. Half of the polygon side then is **called once again small height**. One looks for the corresponding hypotenuse. Its square is 267949193445 *hu*, the remaining fraction being left out. **One extracts the square root, which gives a side of the 12-gon.**"
>
> (以**減**半徑，餘一寸三分三釐九毫七秒四忽五分忽之三，謂之**小句。**觚之半面**而又謂之小股。**爲之求弦。其冪二千六百七十九億四千九百一十九萬三千四百四十五忽，餘分棄 之。**開方除之，即十二觚之一面也。**; emphases mine).

Some features of this part of the text with respect to the formulation of the algorithm and the meaning of its operations were not addressed in the discussion above. To begin, Liu Hui brings out the right-angled triangle ABC by means of the same technique as above: He points out its base AC and its height AB by determining their values and indicating the part they play in the triangle. These two segments can be known on the basis of the magnitudes previously determined. The base is introduced as the meaning of an operation carried out on two segments known and placed in the diagram: half of the diameter and the height OA of the triangle OAB. As for its height, AB, introduced again as “half of the polygon side,” it played another part in the triangle OAB. Reinterpreting the same segment in another way is required to formulate the meaning of the subsequent operations. So, Liu Hui restates the meaning of the segment, distinguishing triangle ABC from OAB by qualifying each of the sides of the former as “small.”

Once the base and height of the triangle are determined, as above, by means of the term “one looks for” Liu Hui introduces the problem of finding the length of the hypotenuse. By contrast with the previous case, evoking the problem by way of its data and the desired result suffices here to indicate that the procedure – the “Pythagorean” procedure – is inserted in the algorithm composed. Indeed, even though the procedure is used for the computation of the square mentioned, it is not quoted in its entirety. Only its last two operations are listed explicitly. For the penultimate one, the approximation to be used for the numerical value it yields is given. As for the final one, note that Liu Hui makes only its meaning explicit – it is a “side of the 12-gon” – but not the value it yields. Clarifying why Liu Hui does this will allow us to understand a key characteristic of such algorithms, the structure of which I characterized above as “transparent.”

17.3.2 How Can the Structure of the Text for an Algorithm Lose its Transparency?

To answer the question just raised, I examine the subsequent section of Liu Hui’s text for his algorithm. It constitutes the beginning of the first repetition of the iterated sequence of operations:

> “Procedure consisting in cutting the 12-gon in order to make a 24-gon: Likewise, one takes the half-diameter as hypotenuse, half of the side as base and one looks for the corresponding height. **One sets up the square of the previous small hypotenuse**, and one divides this by 4, hence one obtains 66987298361 *hu,* and one leaves out the remaining parts, which gives the square of the base. This being subtracted from the square of the hypotenuse, what remains, one divides it by extraction of the square root […]
>
> (割十二觚以爲二十四觚術曰：亦令半徑爲弦，半面爲句，爲之求股。**置上小弦冪**，四而一，得六百六十九億八千七百二十九萬八千三百六十一忽，餘分棄之，即句冪也。以減弦冪，其餘，開方除之，[…]; emphasis mine).

The main idea of the procedure is the following: The previous computation had yielded the side of the 12-gon. Now, Liu Hui takes half of this magnitude, as before, as the base of a right-angled triangle, whereas half of the diameter is its hypotenuse. On this basis, the same procedure as before will yield this triangle’s height. The procedure requires squaring the two data, subtracting the smaller from the larger, and extracting the square root. This algorithm can, as above, be interpreted either step by step or sub-procedure by sub-procedure to determine the meanings of the partial results. However, and this is a key point, that particular algorithm is not the one best suited for computations. As a result, Liu Hui will follow two distinct lists of operations, depending on whether he determines the meaning of the result or computes its value. In other words, the algorithm formulated to follow the meaning of the sequence of results differs from the algorithm followed for the computations. The reason is simple. At the end of the previous sequence of operations, Liu Hui had obtained the value of CB by extracting the square root of the value obtained, by means of a "Pythagorean procedure," for CB^2. If we followed the operations just mentioned, we would extract a square root, divide that result by 2 and square the new result to enter it into the next "Pythagorean" procedure. Yet, in addition to the fact that the computations would be cumbersome, actually extracting the square root as Liu Hui does would increase the inaccuracy of the result. Instead of computing $[(\sqrt{(CB^2)})/2]^2$ – the sequence of operations he formulates to yield the result's meaning – Liu Hui uses another sequence of operations only for the computations; he obtains the value of 66987298361 *hu* by simply dividing CB^2 by 4. Thus, he introduces a distinction between the algorithm that shapes the meaning of the result and the algorithm that computes. The former can be represented by the formula $[(\sqrt{a})/2]^2$, whereas the latter boils down to [a/4]. This explains why only the meaning of $\sqrt{a}$, that is, $\sqrt{(CB^2)}$, not its value, needed to be determined: the operation is required for the algorithm determining the meaning of the result, not for the one that computes the value [a/4]. In fact, computing $[(\sqrt{a})/2]^2$ yields the same value as [a/4] *only* if the result of a square root extraction is always given as exact.[22] Yet the algorithm, as Liu Hui described it so far, does not give exact values for the results of root extractions. As a consequence, in terms of the "meaning" of the final result, there is no difference between the two sequences mentioned. However, as far as the values are concerned, the yielded approximations differ.

In sum, to go from the square of the hypotenuse corresponding to triangle ABC to the square of half of the side of the 12-gon, Liu Hui formulates two algorithms in parallel. The first extracts the square root, divides by 2 and then squares the value obtained; it corresponds to a text, the structure of which is transparent and the partial results of which can be interpreted directly, step by step, sub-procedure by sub-procedure. This text is obtained by combining the reasons for using the operations, and thus its structure points to the reasons why the algorithm is correct. However, the algorithm is not convenient for the computations. It makes them uselessly cumbersome and increases their inaccuracy. Liu Hui thus follows a second algorithm for computing, one that rewrites the first algorithm's sequence of computations into one algebraically equivalent operation: "dividing by 4." Its starting point and end point are the same as the first algorithm's in terms of meaning. However, although it makes computation simpler, this rewriting causes a loss in the transparency of the text. There is a tension between the text that points out, by way of its structure, reasons for correctness and the text that prescribes more convenient computations.

---

[22] Such transformations constitute parts of proofs to which I referred as "algebraic proofs in an algorithmic context." On this set of transformations and how their correctness was approached in ancient China, see Chemla 1997/1998.

The operations deleted in the latter need to be restored to retrieve a transparency similar to that of the first part of Liu Hui's text.

These simple remarks are yet fundamental: in most cases in which an algorithm's text is not structurally transparent with regard to the reasons for its correctness, one may infer that a similar rewriting occurred. That is, a list of operations carrying out a task, which was composed step by step, sub-procedure by sub-procedure, and whose structure was thus transparent, was rewritten so as to make the computations less clumsy.[23] This conclusion casts light on how the transparency of the text for an algorithm can be achieved. It also explains why, in some cases, the commentators can interpret those texts for algorithms in *The Nine Chapters* that have a transparent structure, thereby making the reasons for their correctness explicit.

Here in our first example, we have read a section of the text large enough for us to draw some conclusions. With it, we could analyze one modality – the simplest one – for writing down a text for an algorithm. Actions were prescribed in a straightforward way, by means of terms naming the operations to be executed. However, we also encountered some indirect ways of referring to actions: reference by stating the meaning of their results. Further, the text, or, more precisely, mainly the first part of the text, had a structure transparent about reasons for the algorithm's correctness. The meaning of the operations could be formulated, step by step, sub-procedure by sub-procedure, until the meaning of the result was established. In this text, Liu Hui formulated this meaning explicitly, combining the text that prescribes and the text that accounts for the correctness. The combination of the two became even more visible in the second portion, in which the two paths separated; that is, when, in order to compute a value for a magnitude, the list of operations leading to the meaning differed from that leading to a numerical value.

The part of the excerpt in which both dimensions coexist harmoniously can be considered a paradigm for such texts of algorithms in two ways. To bring these two ways to light, we shall consider separately the two components that the text combines.

To start with, texts for algorithms like the portion of the text in which operations are prescribed with transparent structure, in the technical sense I introduced above, frequently occur, not only in Chinese writings, like *The Nine Chapters* or the *Book of Mathematical Procedures*,[24] but also in other mathematical traditions. Jens

---

[23]For those algorithms in *The Nine Chapters* the text of which does not have a transparent structure, the commentators regularly argue that the reason lies precisely in such rewriting. They compose, in the way just outlined, an algorithm carrying out the task expected from the algorithm commented upon. They further bring to light the cumbersome character of the algorithm they have composed, when it comes to computations, to account for the fact that the algorithm recorded in *The Nine Chapters* differs from the one they just composed. The transformations they describe in order to transform the latter algorithm into the former, thereby proving its correctness and accounting for its shape, constitute the part of the proof to which I refer by the expression of "algebraic proofs in an algorithmic context."

[24]See for example the texts for algorithms computing the volumes of solids recorded in bamboo slips 142–145 (Peng Hao (彭浩) 2001: 101–105). They share common features with texts for algorithms in *The Nine Chapters* and the structure of which the commentators interpret as transparent (Chemla 1991). Cullen 2004: 90–99 developed this idea of mine.

Høyrup's interpretation of Mesopotamian tablets recording texts for algorithm can be reformulated by saying that it implies that these texts have a transparent structure (Høyrup 1990); thus, we have an entire corpus of tablets characterized by this feature. In addition, the texts for algorithms recorded in Al-Khwarizmi's *Book of Algebra and al-Muqabala* also share this property.[25] The portion of Liu Hui's text examined is paradigmatic for all these sources.

However, the status of the "transparent structure" for texts is different in all these sources. This remark leads us to the second component of Liu Hui's excerpt, which makes explicit the meaning of the operations throughout the sequence which constitutes the text for the algorithm, thereby "interpreting" the structure of the text. In Liu Hui's excerpt and in al-Khwarizmi's book, the proofs of the correctness of the algorithms that the authors themselves developed share this feature: the proof follows the sequence of operations, as the text for the algorithm gives it, and makes explicit the meanings, step by step, or sub-procedure by sub-procedure.[26] In this respect, the second component of Liu Hui's excerpt is paradigmatic. On the one hand, these sources all illustrate how the text for the algorithm is handled in writing down the proof of the correctness: the proof follows the text linearly, from beginning to end.[27] On the other hand, we have testimonies that the structure of the text is meaningful for the authors who wrote it down. However, the evidence regarding the *status* of the structure is more indirect in the other cases. For *The Nine Chapters*, the structure can be showed to be meaningful for commentators, since the proof they write to establish the correctness relies on the structure of the text for the algorithm. With regard to the *Book of Mathematical Procedures*, by analogy with *The Nine Chapters* and its commentaries we can assume that the structure of the text was meaningful for readers. As for the Mesopotamian cases, except for similarities with Arabic sources in the formulation of algorithms that may indicate that we are justified to read the former in relation to the evidence provided by the latter, we could be left with no evidence regarding how readers made sense of the structure

[25] See the new critical edition and French translation in Rashed 2007: 100*ff*.

[26] In the only case in al-Khwarizmi's book when the algorithm proved differs in its structure from the algorithm to be proved, we find two hints indicating that al-Khwarizmi's intention is to prove the algorithm with the structure with which its text is formulated. First, at the end of his proof, he addresses the differences between the two algorithms. Second, this is the only time when al-Khwarizmi develops a second proof, which in fact establishes the correctness of the algorithm, on the very basis of the structure of its formulation (see Rashed 2007: 108–113). Incidentally this remark shows that the structure of the text is not transparent in and of itself: It is made transparent by an interpretation.

[27] In both cases, the proof consists in making the meanings of the successive results explicit. However, the two authors carried out this operation differently. In the Liu Hui excerpt analyzed here, the meanings are made explicit in the text itself. However, al-Khwarizmi's book presents the proof as a separate text, the structure of which follows the structure of the text for the algorithm. Moreover, the *dispositifs* within which the meanings are expressed differ. Liu Hui makes use of diagrams as well as of problems and procedures attached to them. These are precisely the elements with which Liu Hui claims to have made the *yi* (意, "meaning") explicit (see *yi* in *Glossary*). Al-Khwarizmi uses only diagrams, the nature of which differs from Liu Hui's.

of the texts. However, these Mesopotamian texts have a second property that seems to also be aiming towards indicating reasons for correctness by means of the formulation of the algorithm's text. To understand this point better, I shall now turn to the second family of texts in *The Nine Chapters*: Those texts that point out the reasons for correctness in how texts for algorithms are written, but use a different technique than we have previously discussed to indicate those reasons.

17.3.3 A Necessary Digression: Aspects of Liu Hui's Practice of Proving the Correctness of Algorithms

How texts belonging to the second family refer to reasons for the correctness of the algorithm is less easy to understand than the first family. Again, the commentators' testimony will prove essential to approach these texts in a rational way. In particular, as a necessary introduction, I shall briefly discuss the practice of proving the correctness of algorithms to which the commentaries on *The Nine Chapters* bear witness. An essential passage of Liu Hui's commentary in which he establishes the correctness of the procedure that *The Nine Chapters* provided to add fractions illustrates perfectly the features of proof needed for the argument.[28] The procedure is formulated after three similar problems, of which the first asks:

> **(1.7)** "Suppose that one has 1/3 (i.e., one of three parts) and 2/5 (i.e., two of five parts). One asks how much one gets if one gathers them."
>
> (今有三分之一，五分之二，問合之得幾何。).

The procedure included by *The Nine Chapters* to solve such problems corresponds, in modern terms, to the formula $\frac{a}{b}+\frac{c}{d}=\frac{ad+cb}{bd}$. It can be used to add an arbitrarily large number of fractions. Its text reads:

> "The denominators multiply the numerators that do not correspond to them; one adds up and takes this as the dividend. The denominators being multiplied by one another make the divisor. One divides […]."
>
> (術曰：母互乘子，并以爲實。母相乘爲法。 。實如法而一[…]).

The first sentence of the procedure, which prescribes a kind of multiplication (*y hucheng x*, "multiplying the *x*'s by (each of) the *y*'s that do not correspond to them"), translates into several operations on the surface for computing. In the case when the problem deals with two fractions, the sentence corresponds to multiplying *a* by *d* and *c* by *b*. In a case of *n* fractions, the sentence groups together all the multiplications of each numerator by all the other fractions' denominators. Thus, there is no one-to-one correspondence between the terms referring to operations in

[28] I have devoted several publications to this text. I shall strictly limit myself here to what is essential to deal with the topic of this article. For greater detail, compare, for instance, Chemla 1997.

the text and the actions performed on the surface for computing. Moreover, the practitioner has to determine the relationship between the text and the actions on the basis of the problem to be solved. As the commentator will make clear, the sentence in question groups together operations that have the same "meaning."

In brief, Liu Hui approaches establishing the correctness of the procedure as follows: The expression for the fractions *m/n* involved in the outline of a problem like 1.7, "*m* of *n* parts" (n *fen zhi* m, *n* 分之*m*), gives the fractions as composed of "parts." I characterize this level as "material," as opposed to the "numerical" level, in which the stress is placed on the *pair* of numbers (numerator and denominator) defining the fraction. On the one hand, the statement of Problems like 1.7 gathers various disparate parts together to form a quantity that must be evaluated. On the other hand, the algorithm prescribes computations on numerators and denominators to form a dividend and a divisor. Establishing the correctness requires proving that the value obtained by division correctly measures the quantity formed by assembling the parts given.

In a first step, approaching the fractions as manipulated by the algorithm, Liu Hui stresses the variability of their expression: He underlines that one can multiply, or divide, both the numerator and the denominator by any given number without changing the quantity meant. In this particular context, to divide is to simplify the fraction. The opposite operation, to "complicate," which Liu Hui introduces in the context of his commentary on fractions, is needed only for the sake of the proofs. Liu Hui, then, considers the counterpart of these operations with respect to the fractions regarded as parts: Simplified fractions correspond to coarser parts, complex fractions to finer parts. The operation of "complicating" at the numerical level translates at the material level into disaggregating the parts. Again, at the material level Liu Hui stresses the invariability of the quantity, beyond possible changes in the way of composing it with parts.

Now, to prove that $\frac{a}{b}+\frac{c}{d}=\frac{ad+cb}{bd}$, Liu Hui shows that the strategy of the algorithm amounts to refining the disparate parts by "multiplication" so as to make them share the same size – in his words, "to make them communicate." This is the desired goal of the program when one considers the operation from the point of view of the fractions added, and Liu Hui has to connect this program to the operations prescribed. In order to uncover how the strategy is implemented, Liu Hui expounds the actual meaning of each step of the procedure in terms of both parts and numerators/denominators, in order to make clear how the steps combined to fulfill the program announced. When "the denominators are multiplied by one another," an operation that in the course of the proof, he names "to equalize," this computes the denominator common to the fractions involved and defines a size that the different parts can share: they can thus be added. Moreover, when "the denominators multiply the numerators that do not correspond to them" to yield *ad* and *cb,* the numerators are made homogeneous with the denominators to which they correspond; hence, the original quantities are not lost, Liu Hui says. Here too, he confers a name to this set of operations: "to homogenize." "Equalizing" the denominators and "homogenizing" the numerators, the algorithm thus yields a correct measure of the quantity formed by joining the various fractions. Thus, Liu Hui reasons, the procedure is correct.

Liu Hui's new terms referring to the necessary operations do so in the same way as the term "multiplication of the x's by each of the y's that do not correspond to them" did: "Equalizing" corresponds to the action of multiplying, as many times as necessary, two or more denominators by one another, depending on the number of fractions dealt with. Moreover, "homogenizing" comprises in a single term all the multiplications needed to compute numerators homogeneous with the newly formed denominator. The key point for us here is to observe *how* the terms introduced in the proof refer to the actions to be carried out. "Equalizing" and "homogenizing" do not prescribe these multiplications directly. Instead, they refer to the actions to be taken by way of the "meaning" that the operations have in their context of use (in the sense of the word "meaning" introduced in II.2, above). In other words, the operations are prescribed by means of terms designating the intention that commands their use: one multiplies denominators so as to yield an "equal" denominator and thereby determine an "equal" size for the "parts" of the fractions involved. The same principle holds true for "homogenizing." The terms "equalizing" and "homogenizing" thus each designate groups of multiplications that achieve one and the same goal. In addition, Liu Hui introduces the operation "making communicate" as a step of the proof, capturing an overarching meaning in the main part of the procedure: It brings into "communication" parts that were disparate, allowing them to be added. However, the term corresponds to no specific step in the procedure, being in fact decomposed into and specified by the operations of "equalizing" and "homogenizing." The name of the overall strategy discloses the key goal of using the latter two operations: "equalizing" and "homogenizing" conjoin in making the parts share the same size and hence enabling them to "communicate."

Liu Hui perceives the operations "equalizing" and "homogenizing" as an alternative way of writing a text for an algorithm corresponding to the same set of actions on the surface for computing. This observation derives from the fact that in some contexts, he actually uses them, as later mathematicians like Zhu Shijie would also do, to prescribe how to add up fractions. However, the two ways of writing down a text for the same course of actions do not seem equivalent in his eyes, judging by the final remarks he makes regarding the operations introduced, for instance: "[...] If so, the procedure of homogenizing and equalizing is essential. [...] Multiply to disaggregate them, simplify to assemble them, homogenize and equalize to make them communicate, how could those not be the key-points of computations/mathematics?"

I have argued elsewhere that these remarks can be interpreted as underlining that the terms "equalizing" and "homogenizing" have a second meaning, both in this context and in the other contexts in which they occur conjointly in the commentaries. For instance, in addition to its meaning in relation to fractions (equalizing denominators at the numerical level as well as equalizing the size of the parts at the material level), the term "equalizing" takes on a formal meaning. In each of the contexts in which Liu Hui discloses the pattern of equalizing and homogenizing, the terms highlight that the algorithm under consideration formally proceeds through making some quantities equal and making other quantities that are linked to them by a linear relation homogeneous of them.[29] The expression of this second meaning is one key reason for which the two texts corresponding to the same actions are not equivalent.

To conclude, in establishing the correctness of *The Nine Chapters*' procedure to add up fractions, Liu Hui pursues two goals simultaneously. On the one hand, he makes the "meaning" of the operations clear with respect to fractions: Their parts are disaggregated in concordant ways. On the other hand, he does so in such a way as to bring to light a "pattern," a "form," in how the material operations are carried out: They equalize and homogenize. This form discloses similarities between apparently unrelated algorithms. This description of Liu Hui's way of proving the correctness of the algorithm for adding fractions also accounts for his practice in other contexts in which equalizing and homogenizing occur. Although in each context they may have different concrete meanings, the fact that Liu Hui manifests the same pattern of proceeding in various contexts brings to light a formal strategy common to otherwise distinct algorithms.

In addition, our reading of the proof Liu Hui developed in this piece of commentary shows how he produced a new text that prescribed an algorithm by stating the meaning of its operations: that is, the reason for using them. *The Nine Chapters* contains texts for algorithms precisely of this type. I shall now examine one of them, once again relying on Liu Hui's commentary on it.

17.3.4 Texts for Algorithms Covering Various Cases and Referring to Operations by Way of their Meaning

I shall illustrate the second family of texts with the example of the algorithm given in *The Nine Chapters* to divide quantities combining integers and fractions.[30] The text is placed after two problems, which read:

> **(1.17)** "Suppose one has 7 persons sharing 8 units of cash, 1/3 of a unit of cash. One asks how much a person gets."
>
> (今有七人，分八錢三分錢之一。問人得幾何).

---

[29] To give but one example, Chap. 8 in *The Nine Chapters* is devoted to solving systems of linear equations. The algorithm provided for this is the so-called "Gauss elimination method." In his account for the correctness of this procedure, Liu Hui brings to light that it "equalizes" the coefficients of the unknown that is eliminated, whereas it "homogenizes" the other coefficients in the equations between which one eliminates. At a material level, the operations of equalizing and homogenizing have a meaning that differs from those occurring in relation to fractions. However, at a formal level, the algorithms share the same strategy.

[30] I argued for an interpretation of this text in Chemla 1992. In a forthcoming paper, I examine how the text covers the various cases in greater detail. This paper will be published in the volume edited by J. Virbel and myself, as the outcome of the seminar "History of science, history of text." Here, I rely on my 1992 publication without repeating its argument, my main focus being to analyze the text of the algorithm from the perspective of how it refers to reasons for correctness.

(1.18) "Suppose again one has 3 persons and 1/3 of a person sharing 6 units of cash, 1/3 and 3/4 of a unit of cash. One asks how much a person gets."

(又有三人三分人之一，分六錢三分錢之一、四分錢之三。問人得幾何).

The problems are followed by a text for a procedure, however, at first sight, the meaning of this text is obscure for a present-day reader. I translated it in such a way as to keep the flavor of the original text, as follows:

"One takes the quantity of persons as divisor, the quantity of cash as dividend and one divides the dividend by the divisor. If there is one type of part, one **makes them communicate**. [here, Liu Hui inserts a commentary on the algorithm] If there are several types of parts, one **equalizes** them and hence **makes them communicate**."

(以人數爲法，錢數爲實，實如法而一。有分者**通之**； 重有分**者同而通之。**; emphases mine).

In the Chinese text, as in the English translation, the terms I marked in bold prescribe operations indirectly, in contrast with the straightforward way of referring to operations in the previous examples of texts for algorithms in *The Nine Chapters*. Since we are not members of the scholarly culture for whom these indirect prescriptions made sense, we are not in a position to understand them and translate them into action, let alone analyze them. However, we are able to perceive that this mode of prescribing operations does relate to the type of proof described in the previous section. Fortunately, we can rely on Liu Hui – the most ancient reader available to us to observe – to determine for us, through his eyes, the actions corresponding to the text. I shall examine his interpretation, before analyzing his view of how these indirect speech acts – or, in this case, "indirect scribal acts" – are carried out.

Liu Hui interprets the text as dealing with several cases. The first and most fundamental case corresponds to no actual problem in *The Nine Chapters*: it is the case in which the two data are integers. The algorithm then boils down to its first part, directly prescribing a division.

The case in which the data contains only one type of fraction occurring in the dividend and/or the divisor, partly illustrated by problem 1.17, is dealt with by a sequence of actions that can be represented, in modern terms, by the following formulas:[31]

$$(a+\frac{b}{c})/d = (ac+b)/dc$$

$$(a+\frac{b}{c})/(d+\frac{e}{c}) = (ac+b)/(dc+e)$$

These computations, as Liu Hui explains, translate into action the prescription "one makes them communicate." This operation, which constitutes the second section of the text, transforms $(a+\frac{b}{c})$ and d (or $(d+\frac{e}{c})$) into, respectively, $(ac+b)$

[31] In fact, the general case meant here corresponds to the second formula, the first corresponding to e equal to 0.

and dc (or $(dc+e)$), that is, into a problem in which we recognize the fundamental case.

The data characteristic of the third and final case – where two (or more) different fractions are involved, as illustrated by problem 1.18 – are transformed, by the operation of "equalizing," into what can be represented as follows:

$$(a+\frac{b}{c})/(d+\frac{e}{f})=(a+\frac{bf}{cf})/(d+\frac{ec}{cf})$$

Clearly, the operation of "equalizing" transforms the problem back to the second case. This interpretation fits with the fact that the next operation prescribed in this segment of the text is to "make them communicate," which returns them to the fundamental case. In brief, the text for the algorithm presents the various sets of actions to execute a division, sorting them out into three cases of increasing complexity. The actions necessary for solving problems falling under the last case embed those required for the second case. Both sequences embed the operations solving the fundamental case, which constitute in a sense the root of the text.[32]

Liu Hui's commentary here contains two layers. In one, he translates the indirect prescriptions into terms that prescribe the operations straightforwardly. In the second, exactly in the same way as for the addition of fractions, he elucidates that the terms "equalizing" and "making communicate," used this time in the text itself, indicate the "meaning" of the actions to be performed; in other words, the reasons why these actions conjoin into a correct algorithm. This testimony proves that Liu Hui interprets the indirect speech acts as prescribing the computations by stating the reasons why they should be carried out. Thus, in Liu Hui's view, the text for the algorithm recorded in *The Nine Chapters* refers to reasons for its correctness.

The text just examined achieves that property in a way that contrasts sharply with that I described above in Sect. 17.3.1. In the earlier example, the text presented the algorithm in the form of a sequence of operations, the structure of which was transparent; that is, the "meaning" of which could be formulated step by step, or sub-procedure by sub-procedure. Liu Hui, when meeting such texts, makes explicit the "meanings" thereby indicated. The second type of text, illustrated by the last example, designates the reasons for correctness by means of the terms chosen to prescribe the operations: These operations are prescribed indirectly by the reasons for using them. Again, Liu Hui develops proofs that make these meanings explicit. The feature of indirectness characterizes texts that belong to the second family, whereas transparency captures the essence of the

[32] My forthcoming article points out that such types of text, organizing cases in exactly the same way, recur in Chinese sources from the second century B.C.E. till at least the seventh century. The next section of this article will show another example of this phenomenon. The way in which the practitioner used the text to derive lists of actions requires clarification. It illustrates how, behind what appears to be a list of operations, complex structures may be hidden. However, I cannot dwell on this issue here.

first family. I indicated above that Høyrup's analysis of Mesopotamian texts implied that they belonged to the first family. However, things seem to be subtler in this case: Høyrup not only shows that the structure of the text allows us – and probably also the practitioners – to interpret the meaning of the operations geometrically in a progressive way, but also suggests that the terms used to prescribe the operations simultaneously indicate the geometrical operation to be carried out to account for the whole procedure's correctness. In other terms, we may cautiously assume, given that we have no testimony of how ancient readers interpreted these texts, that the Mesopotamian texts in Høyrup's analysis belong simultaneously to both families. They use both of the two main techniques illustrated here in order to indicate, by way of the text of the algorithm itself, reasons for its correctness. Thus, by making use of the distinction introduced here, the historian can disclose various ways in which practitioners used different possibilities for writing texts for algorithms.

However, going one step further in this analysis will yield further source material for historians. In fact, different Chinese sources bear witness to two distinct ways of realizing texts from the second family identified. More precisely, the way in which the property characterizing the second family of texts is implemented in *The Nine Chapters* is specific. *The way* in which the terms state the reasons is coherent with Liu Hui's commentary on the addition of fractions: the terms indicate the reasons, while disclosing simultaneously a "form" in the computations. This last feature is essential for the new distinction just formulated, because *the way* in which the reasons are indicated by the terms chosen to prescribe the operations differs in other contexts from that in *The Nine Chapters* (e.g., as I shall show in the next section, in the *Book of Mathematical Procedures*).

In order to prepare the description of this contrast, I shall examine in greater detail how Liu Hui interprets, in the text for the division analyzed above, the term "one makes them communicate." This implies returning to the second case, which deals with divisions such as: $(a+\frac{b}{c})/(d+\frac{e}{c})$. As we saw, Liu Hui translates the prescription in question into the two sequences of actions that lead to computing, respectively, $(ac+b)$ and $(dc+e)$. But how does Liu Hui understand that these actions are prescribed by the term "one makes them communicate"?

Lui Hui relates the use of the term to two main facts. First, computing ac and dc consists in carrying out a multiplication that, on a material level, disaggregates the integers a and d, "making" the integers "communicate" with the numerator. One can thus add them, which yields $(ac+b)$ and $(dc+e)$. The operation is prescribed by neither the term, "one multiplies," nor by the term that would capture the reason at a material level, "one disaggregates." Rather, the operation is prescribed by the reason expressed in a way that highlights a general "form" in the computations. At that level, the use of "making communicate" echoes how other algorithms, like that to add up fractions, proceed, even though the specific operations meant are different. Moreover, the use of "making communicate" falls under the rhetorical category of the synecdoche: as Liu Hui understands it, designating the reasons for carrying out the multiplications also prescribes the ensuing additions ($ac+b$ and $dc+e$).

However, the use of "making communicate" also captures another feature in the procedure – this is the second fact that Liu Hui associates with it. This second feature corresponds to no specific action but is essential for the computations to be correct. These computations bear on what will eventually be a dividend and a divisor. The data are thereby "brought into relation" by the fact of eventually being terms of a division. As a consequence of "being in relation," they "are made to communicate," a second layer of meaning that the term here conveys in Liu Hui's eyes. This implies, Liu Hui stresses, that their values must be modified simultaneously – multiplied or divided by the same number – in order for the result of the division not to be changed. In fact, Liu Hui approaches this property of quantities being brought into relation in the most general way possible, indicating that these phenomena are general and that sets of quantities sharing such properties fall under the rubric of the general concept of *lü*, which he introduces on that occasion.[33] Observing the computations carried out from this perspective, one notices that the algorithm proceeded in such a way that it transformed the would-be dividend and divisor *simultaneously* and *in the same way*, multiplying both by c. The fact that the quantities in question "are made to communicate," by being made terms of a division warrants the correctness of the set of multiplications with respect to the outcome of the final division. In the end, this property warrants that the second case can be reduced to the first one. Hence, this aspect of "making communicate," which Liu Hui brings to light, corresponds to no action but discloses another reason, linked to the "communication" between values, that accounts for the algorithm's correctness.

To recapitulate, the term "making communicate," as Liu Hui comments on it, designates a set of elementary actions and properties (the main property being that the data that become "dividend" and divisor" "are made to communicate"). The term refers to a cluster of operations and properties in relation to the fact that they receive the same "meaning" and hence are shown to be correct as a whole. In other words, the cluster has a "meaning" and the procedure refers to it by way of this "meaning." This analysis shows how a term in the text of an algorithm can both prescribe a set of actions and correlatively convey a conceptualization of the transformations carried out. The grain of the initial description here was particularly coarse and, in relation to that, loaded with meanings that Liu Hui unpacks. Comparing Liu Hui's uses of "making communicate" in this context and in his proof of the correctness of the algorithm to add fractions enables an even finer interpretation: even though formally in each context the actions meant by the term allow the data to enter jointly into certain common operations, the actual computations required to do so differ in each context.

The use of these types of terms and operations characterizes *The Nine Chapters* and its commentaries. This fact emerges from a comparison with the texts for algorithms in the *Book of Mathematical Procedures*, to which I now turn.

[33] *Lü* qualifies quantities that are defined only relatively to each other – see below. This concept was discussed in Li Jimin (李繼閔 1982) and in Guo Shuchun (郭書春 1984). See also *Glossary*, 956–959.

17.4 Relationships Between Texts for Algorithms and Reasons in the *Book of Mathematical Procedures*

The *Book of Mathematical Procedures* also contains texts for algorithms of the first family, essentially similar to those included in *The Nine Chapters* some two centuries later. However, I shall focus on its texts that make use of techniques specific of the second family, concentrating, in particular, on how they are formulated.

I shall examine closely the text for an algorithm that executes an operation called in the *Book of Mathematical Procedures* "*lü*-ing with the *dan*." The *dan* (石) designates a unit of measure.[34] If we rely on the occurrences of the expression "*lü*-ing with the *dan*" in the book, we see that the operation computes the price for 1 *dan* of something, given the price for another quantity of the same thing. The character *lü* used here is the same as the one Liu Hui later used in his commentary on *The Nine Chapters'* algorithm for division above. Although, Liu Hui mostly used the term *lü* as a noun, the *Book of Mathematical Procedures* and the related sections in *The Nine Chapters* itself used it mostly as a verb. I have shown elsewhere (Chemla 2006) that, when recording exactly the same procedures to carry out operations having names of the kind "*lü*-ing with the *dan*," *The Nine Chapters* renamed two quantities involved in the *Book of Mathematical Procedures*' computations with the character *lü*. This fact seems to indicate a historical connection between these algorithms and the emergence of the concept of *lü*. In addition, it suggests that the interpretation of *lü* in the *Book of Mathematical Procedures* should, at least as a first hypothesis, rely on this later development. Hence, I here interpret *lü* as referring to the fact that the algorithm will choose "1 *dan*" as making a set of *lü* with the quantity of something given in the statement of the problem to be solved, in the sense outlined in the preceding section. I shall however, at least for the moment, leave *lü* untranslated.

I shall first examine a problem for which the operation is executed and the algorithm described in a straightforward way before turning to the text provided for its more general statement. The problem recorded in bamboo slip 76 reads as follows:

[34] I am grateful to Professor Ma Biao, who has established that the reading of the character 石, when it designates a unit of measure for capacities, should be *dan*, and not *shi* as occurs in most Western sinological literature. I refer the reader to his forthcoming article on the topic. When the *Book of Mathematical Procedures* was composed, this character designated both the highest unit of capacity and the highest unit of weight used. In both cases, it read *dan*. There are reasons to believe that both units of measures are meant in the title of this operation and that they paradigmatically refer to the highest unit in a given series of units. The critical edition of the part of the *Book of Mathematical Procedures* that I analyze here can be found in Peng Hao (彭浩 2001: 73–75). Note that the manuscript found in a tomb was written on bamboo slips, which were discovered unbound. In such cases, the operations of the critical edition include suggesting an order of the bamboo slips. The order for the slips to which I refer is the one suggested by Professor Peng Hao. Below, we shall refer to two series of units. For the units of weight, the relationships between them are given in slip 47, as follows: 24 *zhu* for 1 *liang*, 384 *zhu* for 1 *jin,* (…), 46080 *zhu* for 1 *dan*. We can deduce the relationships between the units of capacity used in the *Book of Mathematical Procedures* from its text. They are, respectively, 10 *sheng* for 1 *dou*, 100 *sheng* for 1 *dan.* These values correspond to what contemporary sources attest to.

"Trading salt Suppose one has 1 *dan* 4 *dou* 5 *sheng* 1/3 *sheng* salt and that when trading it, one obtains 150 cash. If one wants that the *dan* "*lü*'s" it (the quantity of salt bought), how much cash does this make? One says: 103 cash 9[2]/43[6] cash." (賈鹽 今有鹽一石四斗五升少半升，賈取錢百五十，欲石a（率）之，為錢幾何」。曰：百三錢四百卌(三十)[六]分錢九十[三]。/76/).

In other words, for a given amount of cash, one trades an amount of salt, which is expressed with several units of capacity and a fraction. The question is: how much cash corresponds to a given unit of capacity, here the *dan*? The idea put into play in the algorithms for solving this category of problems, whether in *The Nine Chapters* or in the *Book of Mathematical Procedures*, is to apply a rule of three. In modern terms, the algorithm can be represented by the formula:

$$\frac{\textit{cash multiplied by } 1\, \textit{unit}(\textit{dan})}{\textit{quantity bought}}$$

According to the way in which the rule of three was handled in ancient China, the divisor and one term of the product that makes the dividend are considered as *lü*. The algorithm first transforms, simultaneously and in the same way, the unit (1 *dan*) and the quantity bought – that is the two "*lü*'s," the first in the dividend and the second in the divisor–, so as to turn them into integers. Only then are the operations – multiplication and division – executed. The end point of these transformations can be represented by the following formula:

$$\frac{\textit{cash multiplied by } 1\, \textit{unit}(\textit{dan}) \textit{ expressed in the same unit as the divisor}}{\textit{quantity bought expressed with respect to a unit in which the quantity becomes an integer}}$$

As for the sequence of transformations, it amounts to the following operations:

$$\frac{\textit{cash multiplied by } 1\, \textit{unit}\, (\textit{dan})}{\textit{quantity bought}} = \frac{\textit{cash multiplied by } 1\, \textit{unit}\, u_1}{q_1 u_1 + q_2 u_2 + \frac{m}{n} u_2}$$

$$= \frac{\textit{cash multiplied by } n.1\, \textit{unit}\, u_1}{nq_1 u_1 + nq_2 u_2 + m u_2}$$

and if $u_1 = k_1 u_2$

$$= \frac{\textit{cash multiplied by } n.k_1 u_2}{nq_1 k_1 u_2 + nq_2 u_2 + m u_2}$$

This sequence of transformations is described in the text of the algorithm associated with this particular problem as follows:

"Procedure: One triples the quantity of salt, which is taken as divisor. One **also** triples the quantity of *sheng* of 1 *dan*, and with the cash, one multiplies it, which is taken as dividend."

(术(術)/76/曰：三鹽之數以為法，**亦**三一石之升數，以錢乘之為實。/77/; emphasis mine).

The procedure stated is specific to the stated problem, using its data. It refers to operations straightforwardly and as a sequence of prescriptions to be followed. However, it has a "shape": the way the transformation of 1 *dan* is expressed underlines, with the use of the word "also," that it is parallel to the transformation undergone by "the quantity of salt." This "also" would be useless if the text was a pure sequence of prescriptions. One might suggest that this way of emphasizing a structure in a sequence of operations points to the operations' meaning – where the meanings can be made explicit step by step – which would make the text a part of the first family.

However, much more interesting for us, is the text provided in the same book for the general algorithm, which Peng Hao chose to place right before the specific problem and procedure just mentioned. This general text does not seem to be associated with any specific problem. I shall translate it to give a flavor of its formulation. Again, its interpretation requires that the reader be trained in the scholarly culture in which the text was composed. I shall then offer an interpretation for it within the framework of the example of the previous problem. The text reads:

> "*lü*-ing with the *dan* Procedure for *lü*-ing with the *dan*: One takes what is exchanged as divisor. One multiplies, by the cash obtained, the quantity of 1 *dan*, which is taken as dividend. Those for which, in their lower (rows), there is a half, one doubles them; (those for which there is) a third, one triples them. Those for which there are *dou* and *sheng*, *jin, liang* and *zhu*, one **also breaks up all** their upper (rows), one makes the (rows) below join them, (yielding a result) which is taken as divisor. What the cash was multiplying is **also broken up like this**."
>
> (石a（率） 石a(率)之術曰：以所買＝（賣）為法，以得錢乘一石數以為實。其下有半者倍之，少半者三之，有斗、升、斤、兩、朱（銖）者**亦皆//破**其上，令下從之為法。錢所乘**亦破如此。** / 7 4 - 7 5 / ; my emphases)

The interpretation of the text that I suggest relies, not only on the problem quoted above, but also on hypotheses regarding the use of the surface of computing to which the *Book of Mathematical Procedures* refers (see Figs. 17.2–17.5 below).[35] Step by step:

1. "One takes what is exchanged as divisor. One multiplies, by the cash obtained, the quantity of 1 *dan*, which is taken as dividend."

   (以所買＝（賣）為法，以得錢乘一石數以為實。).

The terms of dividend and divisor refer to, respectively, the middle and the lower rows of the surface. When the division is executed, the quotient is progressively placed in the higher row. In the case of the procedure analyzed, what is placed in the middle row is the setup of a multiplication. Each row can become the space in which an operation can be set up. Here the multiplicand and multiplier are placed in sub-rows of the middle row, according to the usual setup of a multiplication: the multiplier is in the higher sub-row, the multiplicand in the lower one. However, although the

---

[35]To support my reconstruction of the use of the surface for computing, see my description in Chemla and Guo Shuchun 2004. Simply, I use Arabic figures in place of the configurations of counting rods with which in ancient China figures were written down on the surface. Moreover, for a more detailed discussion of the interpretation provided, see Chemla 2006.

terms of the operations are set up, neither the division nor the multiplication seem to be executed at this point, since several terms will undergo transformations before the main operations are carried out (see below). Exactly the same thing occurred in the sequence of transformations of formulas above: it presented multiplications and divisions, and modified their terms before they were executed. This phenomenon also appears in the text of the algorithm for division examined above.

Last, the quantity placed in the position for the divisor comprises several units and a fraction. In my interpretation, the lower unit associated with an integer is placed in the middle sub-row of the lower position, whereas the larger units are placed in the sub-rows above it, and the fractions horizontally (numerator on the left, denominator on the right) in the sub-rows under it. The initial configuration thus resembles Fig. 17.2.

2. "Those for which, in their lower (rows), there is a half, one **doubles** them; those for which there is a third, one **triples** them."

   (其下有半者倍之，少半者三之; my emphases)

The text now turns to examining cases in which the quantity exchanged includes fractions. Later, it prescribes what to do in cases where the quantity contains more than one unit from a series. In other words, the text encompasses several types of cases and gives sequences of actions to be followed depending on the particular case encountered.

In case there are fractions, one has to multiply the quantity in the divisor position (i.e., each of the rows constituting it), by the denominators of these fractions. This operation is prescribed in a new indirect way; that is, by a simple enumeration of two paradigmatic cases and the specific action that they require. A similar kind of prescription will be chosen in the next sentences. If there is no fraction, the practitioner skips this sentence when deriving actions from the text. However, the sentence must, in any case, be read. For our example, the sentence prescribes actions that lead to the configuration in Fig. 17.3.

Quotient	Below — not indicated	any longer
Dividend	1 *dan* multiplied by 150 cash	
Divisor	1 *dan* 4 *dou* 5 *sheng* 1 3 *sheng*	 Upper Middle Lower

Fig. 17.2 The first step in the use of the surface of computation

Dividend	1 *dan* multiplied by 150 cash	
Divisor	3 *dan* 12 *dou* 15 *sheng* 1 (3) *sheng*	 Upper Middle Lower

Fig. 17.3 The second step in the use of the surface of computation

The next step contains the key phenomenon of interest here:

3. "Those for which there are *dou* and *sheng*, *jin*, *liang* and *zhu*, one **also breaks up all** their upper (rows)."

(有斗、升、斤、兩、朱（銖）者**亦皆//破**其上; my emphasis)

As above, the general possibility that there be more than one unit in the quantity exchanged is expressed by an enumeration of two specific cases. Each of these cases is itself formulated as an enumeration: The quantity would have either two units from the series of units of capacity or three units from that of weight, both enumerations listing units smaller than the *dan,* which both series have as their largest unit.

The main feature of interest here is the prescription with the expression "one also breaks up…." That the text underlines "also" implies that the operation meant is a multiplication, as in Sentence 2 above. This explains my assumption that, even if there is no fraction and Sentence 2 is irrelevant with respect to the actions carried out, the practitioner using the text must read Sentence 2 for the "also" in Sentence 3 to make sense. As in the procedure for the specific problem on bamboo slip 76 examined above, the "also" would be of no use if the text were merely prescriptive.

What needs to be multiplied is made clear: the operation is to be executed on "all the upper (rows)" (皆…其上) in the quantity placed in the position of the divisor, that is, "all the rows" above the middle one, in which the smaller unit is placed. This leads to the configuration in Fig. 17.4.

But the essential issue is how the multiplications are designated. The term "break up" indicates the actions indirectly. This indirect speech act designates the multiplications by the intention for using them: to break up all the higher units so as to convert them into the smaller unit appearing on the surface. The text thus simultaneously uses different ways of prescribing operations. Two remarks are interesting at this point.

First, the term "break up" evokes the term "disaggregating" that Liu Hui repeatedly uses in his commentary on fractions from *The Nine Chapters*. There is a continuity between the terms by means of which the *Book of Mathematical Procedures* refers to multiplications in this context and the reasons as formulated by Liu Hui in a similar

Dividend	1 *dan* multiplied by 150 cash	
Divisor	300 120 15 1	Upper Middle Lower

Fig. 17.4 The third step in the use of the surface of computation

context. This connection supports my interpretation that in the present case the operation of multiplication is prescribed by way of the reason to make use of it.

Second the "also" in Sentence 3 makes the meanings circulate both ways. It not only supports the interpretation of the prescription "to break up" as referring to a multiplication but also retrospectively transmits the meaning "breaking up" to the multiplications prescribed by Sentence 2. Here too, such a meaning is continuous with how Liu Hui would use it in his commentary on *The Nine Chapters.* Most important, however, "break up" refers to multiplication by stating its "material" meaning, *not* by capturing its meaning in any formal way. This constitutes the key difference between *The Nine Chapters* and the *Book of Mathematical Procedures*: When prescribing operations by stating the reasons for using them, the former book uses reasons formulated so as to capture a general form in the computations, whereas the latter uses reasons formulated at a material level.

Sentence 4 simply prescribes adding up all the rows in the divisor position, which by this point have all been converted into the same unit. It reads:

4. "One makes the (rows) below join them, (yielding a result) which is taken as divisor."[36]

   (令下從之為法。).

The fifth and final sentence again presents the phenomenon in which we are interested in a way that allows further conclusions:

5. "What the cash was multiplying is **also broken up like this.**"

   (錢所乘**亦破如此**; my emphasis).

I shall discuss the interpretation of this sentence piece by piece. "What the cash was multiplying" designates the "1 *dan*" by the operation involving it in Sentence

[36] Note that the same term "divisor" designates different values at different points in the flow of computations. This is one of the many examples of the use of the "assignment of variables" in ancient Chinese texts of algorithms.

1. However, this operation, by means of which the value "1 *dan*" is indicated, was not executed then, since one of its terms is now to be modified.[37]

Further, for the second time in this text an "also" occurs. Here too, it indicates that two parallel procedures are used in the sequence of actions. However, what is designated here, as well as how it is designated, is different. Now the procedure reused is the one that modified the quantity in the divisor, and it is signified as "like this." So the list of actions meant by this "also" depends on the case to which the procedure is applied. The prescription simply indicates that the procedure to be applied to 1 *dan* is the same one needed to apply to the quantity in the divisor, depending on its fractions and list of units. In our example, the procedure involves multiplying by 3 and transforming into *sheng*. It yields the configuration in Fig. 17.5.

Note *how* this procedure is designated again by the verb "break up": Understanding this text demands that the transformation linked to the presence of fractions, upstream, be understood as "breaking up." Only in such a case can the appropriate series of actions be understood as "breaking up in the same way," again, a quite coarse-grained description. Moreover, the series of actions is indicated by the reasons that make the operations necessary; that is, by the intention of the set of actions. But in prescribing actions a second time with the same term, the author of the text is confident that the reader will know how to translate the same reason into different actions; that is, the different actions will be determined by when, in the flow of computations, the reason must be fulfilled.

Finally, as in the previous case and in contrast to *The Nine Chapters* when it designates actions by their reasons, the text in the *Book of Mathematical Procedures* designates actions by their material meaning, not their formal one. Nevertheless, the text analyzed here still prescribes actions indirectly by means of the reasons for carrying them out. Consequently, the text itself also formulates reasons for the correctness of the algorithms. This text, thus, also belongs to the second family of texts that I identified.

Dividend	300 multiplied by 150 cash	
Divisor	436	

Fig. 17.5 The fourth step in the use of the surface of computation

[37] The 1 by which the amount of cash was supposed to be multiplied will now be modified. This explains why I initially suggested not executing the multiplication immediately. This recalls how the text for division is formulated in *The Nine Chapters.*

Still, this last example raises the questions of the reason for such a difference between the *Book of Mathematical Procedures* and *The Nine Chapters*, as well as its bearing on the issue of the historical connection between the two writings.

17.5 Conclusion: Writing Texts for Algorithms and Understanding

These analyses clarify how anachronistic and naïve an approach to texts of algorithms can be, especially one that holds that these texts refer to operations only by name, and boil down to a sequence of computations to be executed in the order in which the terms prescribing them occur. Such is not the case in ancient texts. In the examples I examined, the relationship between the text for an algorithm and the actions carried out on an instrument is by no means straightforward. For example, the last text examined showed the case of a multiplication that was prescribed initially but not executed until later. In addition, in the same text, the order in which the operations were to be executed was far from obvious. In the text for division, the way in which cases are covered by a single text differs from expectation. Last, in several cases elementary actions were grouped under a single term, the meaning of which was not always straightforward – sometimes, this feature related to the indirect reference by the text of an algorithm to actions by giving the reasons for carrying them out.

These observations recall the issue of proof. The detailed descriptions here disclosed two main ways in which the text for an algorithm can indicate reasons why the algorithm is correct.

First, some texts for algorithms are written in such a way that the structure of the list of operations constituting them is "transparent." In other words, the meaning, or intention, of the operations or blocks of operations can be made explicit simply by following the sequence given by the text. Consequently, at the end of the sequence of interpretations, the meaning of the final result is established, thus showing that the result is the one desired. Luckily, we have evidence that, for texts of that kind, some ancient Chinese commentators read proofs of the correctness in this way. However, such texts for algorithms are not specific to China, since texts found in several other scholarly cultures also present the same property.

Second, the text for an algorithm could prescribe the same operation in different ways: Sometimes, the speech act is carried out directly, designating the operation by a term like "multiplying"; in other cases it is carried out indirectly. I gave two examples of the latter, with the terms "making communicate" or "breaking up." In both cases, the operations were prescribed by terms indicating the intentions motivating their use – in other words, the goal, or the meaning of the result. This constitutes a fundamental similarity in *the way in which* operations were prescribed indirectly. This feature explains why such texts indicate, in their very formulation, reasons for the correctness of the algorithm described. In fact, there is evidence in our sources supporting this conclusion: reading the ancient commentators on these texts also shows how they develop their proofs of correctness by reading the arguments put forward in this feature of the formulation of the algorithm.

In both types of cases, the commentators handled the texts for the algorithms in specific ways to bring the reasons indicated to light: in the first type, they exploited the structure of the narrative; in the second, they relied on the terms used.

However, despite the fundamental similarity of their indirect prescriptions, the second type of texts analyzed also show key differences. The terms used to indirectly indicate operations in *The Nine Chapters* captured the meaning of the operation not only at a material but also at a formal level, one at which relationships between various procedures could be established. By contrast, the *Book of Mathematical Procedures*, apparently composed some two centuries earlier, indirectly prescribed operations by way of their material meaning. *If* the *Book of Mathematical Procedures* belonged to the same Chinese written tradition that produced *The Nine Chapters* and its commentaries, these texts may provide evidence of the emergence of an interest in formal properties in mathematics. I have argued elsewhere that such an interest for formal properties permeated *The Nine Chapters* and its commentaries. However, it is not perceptible in the *Book of Mathematical Procedures*.

Despite the differences in how texts for algorithms referred to reasons for correctness, I was led to an unexpected conclusion: Practitioners apparently wanted texts that had this property, to the point that we find distinct types of text realizing it. As to why, I hypothesize that the answer could be found in a result arising from psychological research. Apparently, practitioners using texts of instructions such as algorithms use them all the better when they understand what they are doing.[38] Hence, to me, the evidence of the texts above shows a constant and stable drive, among practitioners, to shape texts for algorithms that would yield understanding. The two families of text examined above show two main ways in which practitioners achieved this goal. Moreover, the difference between the *Book of Mathematical Procedures* and *The Nine Chapters* may even highlight a historical evolution in the ways in which practitioners shaped such texts. In other words, their features simply emphasize that the texts were made and used by human practitioners rather than by machines, as previous historians perhaps surreptitiously assumed.

Cavillargues

Acknowledgments It is my pleasure to express my deepest gratitude to John Holt, who had the difficult task of taming my English, and to Sarah-Jane Patterson who helped me in a crucial way to implement these changes. Without them, the paper would not be as readable as it has become. Nevertheless, I remain responsible for all remaining shortcomings. My most sincere thanks to Gila Hanna and Niels Jahnke, for their support and their patience in all circumstances!

[38] I owe this notion to Jacques Virbel, who took part in research in cognitive psychology on texts of instructions (private communication). Compare also J. Virbel, J.M. Cellier, J.L. Nespoulous (éds.), *Cognition, discours procédural, action*. Pôle Universitaire Européen de Toulouse & PRESCOT, Novembre 1997, p. 163; *Cognition, discours procédural, action*. Volume II. PRESCOT, Mai 1999, p. 308.

References

Chemla, K. (1991). Theoretical aspects of the chinese algorithmic tradition (First to Third Century). *Historia Scientiarum, 42,* 75–98; Errata in the following issue.

Chemla, K. (1992). Les fractions comme modèle formel en Chine ancienne. In: K. C. Paul Benoit, & J. Ritter (Eds.) *Histoire de fractions fractions d'histoire* (pp. 189–207, 405, 410). Basel: Birkhäuser.

Chemla, K. (1996). Relations between procedure and demonstration: Measuring the circle in the "Nine chapters on mathematical procedures" and their commentary by Liu Hiu (3rd century). In: H. N. Jahnke, N. Knoche, & M. Otte (Eds.) *History of Mathematics and Education: Ideas and Experiences* (pp. 69–112). Goettingen: Vandenhoeck & Ruprecht.

Chemla, K. (1997). What is at stake in mathematical proofs from third-century China? *Science in Context*, *10,* 227–251.

Chemla, K. (1997/1998). Fractions and irrationals between algorithm and proof in ancient China. *Studies in History of Medicine and Science. New Series*, *15,* 31–54.

Chemla, K. (2003). Generality above abstraction: the general expressed in terms of the paradigmatic in mathematics in ancient China. *Science in context, 16*, 413–458.

Chemla, K. (2006). Documenting a process of abstraction in the mathematics of ancient China. In: C. Anderl, & H. Eifring (Eds.) *Studies in Chinese Language and Culture – Festschrift in Honor of Christoph Harbsmeier on the Occasion of his 60th Birthday* (pp. 169–194). Oslo: Hermes Academic Publishing and Bookshop A/S.

Chemla, K., & Shuchun, G. (2004). *Les neuf chapitres. Le Classique mathématique de la Chine ancienne et ses commentaires*. Paris: Dunod, pp. 27–39.

Cullen, C. (2004). *The Suan shu shu* 筭數書 *'Writings on Reckoning': A translation of a Chinese mathematical collection of the second century BC, with explanatory commentary*. Needham Research Institute Working Papers Vol. 1. Cambridge: Needham Research Institute.

Guo Shuchun郭書春 (1984). 《九章算術》和劉徽注中之率概念及其應用試析 (Analysis of the concept of *lü* and its uses in *The Nine Chapters on Mathematical Procedures* and Liu Hui's commentary) (in Chinese). *Kejishi Jikan* 科技史集刊 *(Journal for the History of Science and Technology)*, *11*, 21–36.

Guo Shuchun郭書春 (1992). *Gudai shijie shuxue taidou Liu Hui* 古代世界數學泰斗劉徽 *(Liu Hui, a leading figure of ancient world mathematics)*, 1st edn. Jinan: Shandong kexue jishu chubanshe.

Høyrup, J. (1990). Algebra and naive geometry: An investigation of some basic aspects of Old Babylonian mathematical thought. *Altorientalische Forschungen*, *17,* 27–69, 262–324.

Li Jimin李繼閔 (1982). Zhongguo gudai de fenshu lilun中國古代的分數理論'. In: W. Wenjun (Ed.) *'Jiuzhang suanshu' yu Liu Hui [The Nine Chapters on Mathematical Procedures and Liu Hui]* (pp. 190–209). Beijing: Beijing Shifan Daxue Chubanshe.

Li Jimin李繼閔 (1990). *Dongfang shuxue dianji Jiuzhang suanshu ji qi Liu Hui zhu yanjiu* 東方數學典籍——《九章算術》及其劉徽注研究 *(Research on the Oriental mathematical Classic The Nine Chapters on Mathematical Procedures and on its Commentary by Liu Hui)*, 1 Vol. Xi'an: Shaanxi renmin jiaoyu chubanshe.

Li Yan 李儼 (1958). *Zhongguo shuxue dagang. Xiuding ben* 中國數學大綱 *(Outline of the history of mathematics in China. Revised edition)*, 2 Vols. Beijing: Kexue chubanshe.

Peng Hao彭浩 (2001). *Zhangjiashan hanjian «Suanshushu» zhushi* 張家山漢簡《算術書》注釋 *(Commentary on the Book of Mathematical Procedures, a writing on bamboo slips dating from the Han and discovered at Zhangjiashan)*. Beijing: Science Press (Kexue chubanshe).

Qian Baocong 錢寶琮 (1964). *Zhongguo shuxue shi* 中國數學史 *(History of mathematics in China)*. Beijing: Kexue chubanshe.

Rashed, R. (2007). *Al-Khwarizmi. Le commencement de l'algèbre. Texte établi, traduit et commenté par R. Rashed*. Sciences dans l'histoire. Paris: Librairie scientifique et technique Albert Blanchard.

Wu Wenjun 吳文俊 (Ed.) (1982). *'Jiuzhang suanshu' yu Liu Hui* 九章算術與劉徽 *[The Nine Chapters on Mathematical Procedures and Liu Hui]*. Beijing: Beijing Shifan Daxue Chubanshe.

Wu Wenjun 吳文俊, Bai Shangshu 白尚恕, Shen Kangshen 沈康身, LI Di 李迪 (Eds.) (1993). *Liu Hui yanjiu* 劉徽研究 *(Research on Liu Hui)*. Xi'an: Shaanxi renmin jiaoyu chubanshe, Jiuzhang chubanshe.

Index