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CONTENTS

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Linearity and Reflexivity in the Growth of Mathematical Knowledge

The Argument

Recent studies in the philosophy of mathematics have increasingly stressed the social and historical dimensions of mathematical practice. Although this new emphasis has fathered interesting new perspectives, it has also blurred the distinction between mathematics and other scientific fields. This distinction can be clarified by examining the special interaction of the *body* and *images* of mathematics.

Mathematics has an objective, ever-expanding hard core, the growth of which is conditioned by socially and historically determined images of mathematics. Mathematics also has reflexive capacities unlike those of any other exact science. In no other exact science can the standard methodological framework used *within* the discipline also be used to study the nature of the discipline itself.

Although it has always been present in mathematical research, reflexive thinking has become increasingly central to mathematics over the past century. Many of the images of the discipline have been dictated by the increase in reflexive thinking which has also determined a great portion of the contemporary philosophy and historiography of mathematics.

1. Introduction

Throughout history, mathematics has been seen as the paradigm of certainty and precision. Even after the Einsteinian revolution brought about deep changes in the general conception of science, mathematics mantained its privileged position as a body of unquestioned truth. The epistemological problems arising from the development of non-Euclidean geometry and the paradoxes of set theory were overcome without compromising the general agreement that mathematics represents a body of certain knowledge. In contradistinction to the philosophy of science in general, philosophy of mathematics did not consider error and uncertainty as a problem that required its attention. On the contrary, since the turn of the century the main task of the philosophy of mathematics became the justification of certainty in mathematics through the search for an adequate axiomatic foundation.

It was only in the 1960s, starting perhaps with Lakatos' work, that the first cracks of uncertainty in the wall of mathematical truth began to be philosophically recognized. The idea that every mathematical proof may be absolutely formalized, following the model of Russell and Whitehead's *Principia* (1910–1913), had long since been completely abandoned at the practical level. Today it is being abandoned at the theoretical level as well. Recent trends in the philosophy of mathematics assert that mathematical research should be studied as a human activity and not as a transcendent system of abstract ideas. When viewed as a human endeavor, mathematics throughout history has also comprised error and uncertainty.

Likewise, historical and philosophical research about mathematics has increasingly paid attention to the social dimension of the mathematical enterprise. Until the early 1970s, mathematics was generally considered to be a system of "disembodied ideas" or of ideas contained in the mind of a single "ideal" mathematician. New trends in the philosophy of mathematics underline the decisive role of communication among contemporary mathematicians, on the one hand, and of knowledge transfer from one generation of researchers to the following one, on the other. Thus these new trends reflect similar trends in the philosophy of science in general.

Lately, the term "foundationalist" has been used to identify philosophical systems that regard mathematics as a system of ideal, absolutely certain knowledge. "Foundationalism" is the search for a convenient *axiomatic characterization* of mathematical knowledge. Those who, in contradistinction to the "foundationalists," are eager to consider the sociohistorical dimension of mathematical knowledge, and stress the central role played by error and uncertainty within that dimension, have been said to uphold a "quasi-empirical" approach to the philosophy of mathematics.[1] In what follows, I shall adopt those labels.

As a result of the efforts by many authors to bridge the gap that existed until the 1970s between the philosophy of mathematics and the philosophy of science in general, the quasi-empirical trend has been highly emphasized during the past decade. This emphasis has sometimes led, in my opinion, to overstated conclusions. The willingness to take into account the sociohistorical dimension of mathematical knowledge and the role of uncertainty in its development does not necessitate blurring the distinction between mathematics and other systems of knowledge, including scientific knowledge. In what follows, I present what I take to be a more balanced picture of mathematics.

More specifically, my aim in this paper is to provide a framework for research into the history of mathematics – a framework in which sociohistorical factors are relevant

[1] I take these labels from Tymoczko 1985. Tymoczko, in turn, adopted the term "quasi-empirical" from Lakatos and Putnam. Philip Kitcher has analyzed in similar terms the diverging trends in the contemporary philosophy of mathematics. He has stressed the aprioristic conception of mathematical knowledge underlying the foundationalist trends. Kitcher uses the term "mathematical naturalism" for the "quasi-empirical" trend. See Kitcher 1988, esp. 294–98.

but in which the special character of mathematics is preserved. I make no pretense of answering all the questions that will arise from the description of this framework; instead I will simply attempt to indicate the questions that must be answered clearly in order to understand the growth of mathematical knowledge.

One further word of caution is needed. Although mathematics shares some characteristics with nonscientific fields, I shall take for granted the difference between the exact sciences and other systems of knowledge, and I shall not further describe or explain this difference. My main concern will be with those aspects in which mathematics more closely resembles other exact sciences – namely, objectivity, the high degree of agreement concerning validity of results, the extensive use of formally presented theories and arguments, and other related issues. I will therefore refer mainly to the relationship between mathematics and physics. Having said that, I proceed now to present my picture of the development of mathematics.

We may distinguish, broadly speaking, two sorts of questions concerning every scientific discipline. The first sort are questions about the subject matter of the discipline. The second sort are questions about the discipline *qua* discipline, or second-order questions. It is the aim of a discipline to answer the questions of the first sort, but usually not to answer questions of the second sort. These second-order questions concern the methodology, philosophy, history, or sociology of the discipline and are usually addressed by ancillary disciplines.

Some statements can easily be classified as either first-order or second-order. However, for some statements related to a given discipline it may be harder to establish whether they are answers to questions about the subject matter or about the discipline *qua* discipline. Newton's theory of motion clearly belongs to the first category; it is a statement about how bodies move. The claim that Copernicus' system is "simpler" than Ptolemy's clearly belongs to the second one: it is a claim about astronomy rather than a claim about the heavenly bodies. Gödel's theorems are a *deep* result within a specific branch of mathematics; however, they may also be taken to be a claim about mathematics *qua* discipline.

We can therefore tentatively consider two layers related to a scientific discipline, and they can be described schematically as the "body of knowledge" and the "images of knowledge." In the body of knowledge are included all those statements related to the subject matter of the given discipline, while the images of knowledge include all claims about knowledge itself. This division is not sharp, and to be sure, it is historically determined: we can usually classify a statement as belonging to one or the other only at a given point in time.

The body of knowledge includes theories, "facts," methods, and open problems. The images of knowledge serve as guiding principles, or selectors; they pose and resolve questions that arise from the body of knowledge, but are not part of and cannot be settled within the body of knowledge itself. For example, the images of knowledge help to resolve such questions as the following: Which of the open problems of the discipline most urgently demands attention? How should we decide between competing theories? What is to be considered a relevant experiment? What procedures, individuals, or institutions have authority to adjudicate disagreements within the discipline? What is to be taken as the legitimate methodology?

All these are second-order questions, or metaquestions; they consider diverse aspects of the discipline *qua* discipline. It is evident that the answers to these questions depend on the contents of the body of knowledge at a given stage of development of the discipline. Moreover, changes in the body of knowledge may alter these answers. But these answers are not *exclusively* determined by the body of knowledge; they may be influenced by other, external factors as well. Thus, faced with one and the same body of knowledge, two different scientists could hold different images of knowledge. In turn, the images of knowledge play a decisive role in directing research and further determining the development of the body of knowledge.

The study of the interaction between these two layers – the body and the images of knowledge – might provide a coherent explanation of the effect of sociohistorical factors on the realm of pure ideas, while avoiding dubious "strong" explanations that overemphasize the effects of these factors. Such explanations, which attribute the content of theories to factors *absolutely* external to them, lead unavoidably (and sometimes intentionally) to relativism.

The identification of two layers within mathematical knowledge may also lead to a clear differentiation of mathematics from other scientific disciplines. Further, it may clarify some issues in the philosophy of mathematics; it may, in particular, shed light on the relationship between the philosophy of mathematics and the history of mathematics.

The centrality of second-order thinking and the terms "body of knowledge" and "images of knowledge" are taken from the work of Yehuda Elkana (see esp. Elkana 1981). These concepts arose in the framework of an ambitious program aimed at an anthropologic characterization of scientific knowledge as a cultural system. According to Elkana, scientific knowledge does not grow in a linear process of progressive accumulation, its growth is explained in terms of its body and images:

> Knowledge grows dialectically by way of ongoing critical dialogue – between competing metaphysics and theories in the body of knowledge; between competing images of knowledge; between competing normative tenets. (Elkana 1982, 210)

The concept of images of knowledge plays a very central role in this scheme. According to Elkana, Western culture is characterized and distinguished from all other cultures by the special nature and status that second-order thinking assumes within it. This is not to say that second-order thinking appears in no other culture; but only in Western culture is there "a conscious attempt to apply *systematically* a corpus of thought about thinking to a body of knowledge" (Elkana 1986, 41; italics in the original).

A proper understanding of the various dimensions of scientific knowledge and, in particular, of its historical development can be attained, then, only if the body of knowledge is considered through, and in conjunction with, the images of knowledge. If we look at science as a written text the essence of which we want to grasp, then we can say that the body of knowledge stands, as it were, as the text proper, whereas the images of knowledge stand as the context, in the broadest sense of the word. A complete understanding and a proper historical interpretation may be obtained only through a correct contextual reading of the text – that is to say, through a correct interpretation of the images of knowledge.

Elkana does not specifically deal with mathematics, but I hope to show in this paper that his ideas can provide an especially useful conceptual framework for a discussion of that particular discipline. Moreover, a clarification of the role of second-order thinking in the development of mathematics may, in turn, provide an appropriate framework for a closer inspection of its role in science in general; this topic, however, will not be dealt with here.

2. Reflexivity in Mathematics

What sets mathematics apart from all other exact sciences is the nature of its subject matter and the fact that mathematics becomes its own subject matter. Furthermore, these two aspects are closely interconnected. Distinctive about the subject matter of mathematics is that the question of what constitutes it is an unsettled one. Distinctive about its being its own subject of study is that mathematics is the only exact science in which statements *about* the discipline may still be inside the discipline. I shall call this capacity of mathematics to study itself mathematically the "reflexive character of mathematics." True, there are other systems of knowledge – such as philosophy, history, and art – that to some extent share this reflexive ability. However, as indicated above, the kind of knowledge attained in mathematics is from the outset different from that of those fields, and we can therefore properly speak of a singular reflexive character of mathematics.

In his exposition of the body of knowledge/images of knowledge scheme (hereafter referred to as the body/images scheme), Elkana singles out the conscious *systematic* resort to second-order thinking as the hallmark of Western culture and, in particular, of science. This resort to reflexive thinking has a historical point of departure in ancient Greece, and its origins may be found in the disciplines of rhetoric and geometry rather than in the physical sciences (see esp. Elkana 1986, 42–53). Thus the Greeks introduced geometrical proof and Elkana sees in geometrical proof an instance of second-order thinking par excellence. This last point, however, should be elaborated upon because for mathematics at least the picture is somewhat more complicated than it first appears.

The idea that mathematical knowledge is validated only through proof is indeed second-order thinking, since it is a claim about knowledge and about what kind of assertions can be considered knowledge. The proof itself, however, is part of the body of knowledge. As a matter of fact, it constitutes the body of mathematics par excellence. The novelty of Greek mathematics lies precisely in its new "image" of what is to be counted as real mathematical knowledge – namely, theorems *with* proofs. This is a knowledge-producing image, since it stimulates the discovery of new theorems (with proofs), as well as proper proofs of "known mathematical facts." Proofs are not meant to act merely as justification for the discovery of "facts"; they are themselves the "facts" to be discovered.

Thus, early in the time of the Greeks a central image of mathematics was established that has not changed until now. This image establishes that it is proofs that legitimate, or even constitute, true mathematical assertions. But how do we know what a "legitimate proof" is? The criteria for telling legitimate from nonlegitimate proofs are in themselves images of mathematics and are therefore historically determined. I shall return to this point.

The development of mathematical knowledge since the Greeks included – like the development of any other field of knowledge – a series of changing images of mathematics. But at variance with other scientific fields, mathematics provided the possibility of producing and confronting claims about the discipline within the discipline itself. Thus a considerable amount of reflexive thinking has always been part and parcel of mathematical research. The last two centuries have seen an unprecedented growth of this reflexive activity; the awareness of this growth has, in turn, produced many new images of knowledge.

The very idea that this reflexive activity can be conducted within mathematics is itself an image of knowledge. Important parts of contemporary mathematical research (e.g., everything that is usually included under "metamathematics") developed under the spell of this image; and the success attained by that kind of research has, in turn, led some people to adopt another image of knowledge – namely, that claims *about* mathematics are significant only insofar as they are formulated in strictly mathematical terms and justified by mathematical arguments. This last image is not presently part of the body of mathematics, but it might eventually become so in some unforeseen way.

The above discussion leads to a more precise differentiation between reflexive knowledge and images of knowledge, in mathematics. I shall take reflexive knowledge to be all thinking *about* mathematics that is carried out strictly *inside* mathematics – that is, all parts of the body of mathematical knowledge whose subject matter is an aspect of mathematics *qua* discipline. This includes Gödel numbering and Gödel's theorems, proof theory, postulational analysis and so on. By "images of knowledge" I intend all claims about mathematics that at a given historical point are not an integral part of the mathematical body of knowledge. This includes the philosophy and history of mathematics; it includes "comprehensive" research programs such as Klein's Erlangen program or Hilbert's list of problems of 1900 (the problems themselves are, of course, part of the body of knowledge; to claim that a certain list comprises the most important problems in mathematics to be solved in years to come is a claim *about* the discipline); it includes established beliefs about mathematics that are accepted on the basis, say, of the authority of great mathematicians; and it includes the degree of importance that is ascribed to the authority of great mathematicians in establishing the relative importance of theories and problems.

So stated, the distinction between the mathematical body of knowledge "proper" and reflexive mathematical knowledge may be rather blurred; in fact, as claimed earlier, a considerable part of mathematics deals with mathematical thinking. To ask whether there is a part of the body of mathematical knowledge that is not reflexive (in the present sense of the word) is, then, tantamount to asking whether there is some subject matter to mathematics other than mathematics itself. We can skip that question for our purposes. It is enough in this context to realize that some claims about mathematics *qua* discipline are indeed stated mathematically within the body of knowledge and some others belong more specifically to what we have called images of knowledge. It may be the case, and it has certainly been the case in history, that some images of knowledge are directly transformed into reflexive thinking and hence enter the body of knowledge of mathematics. For instance, Russell's theory of types is a reflexive theory that tackles Russell's logicist thesis, which is an image of knowledge. The requirement of confronting a philosophical claim about mathematics with a mathematical system designed to test it mathematically is itself an image of knowledge; as a matter of fact, it is a rather recent image of knowledge, one to which mathematicians and philosophers were not committed in the past. For instance, no mathematical theory was specifically developed to prove or disprove Kant's claim that the truths of geometry are synthetic a priori.

Let us return briefly to my earlier characterization of mathematics, which may now be stated more clearly. Systematic reflexive knowledge of the kind mentioned above is possible only in mathematics. Physics, the discipline, cannot be the subject matter of physical research because physics deals exclusively with certain aspects of the outside world that, up to this day, do not include scientific theories, or ideas in general, as such. While there may still be disagreement as to what constitutes the subject matter of mathematics, it is certain that some aspects of mathematics itself are part of it.

Earlier I claimed – and this is central to the present discussion – that images of knowledge and the body of knowledge cannot be neatly separated, and that they constantly interact dynamically. I now claim that the nature of the interaction between the two layers is different in mathematics from that in the other exact sciences. Only in mathematics is there an intermediate layer, reflexive knowledge; in no other instance may claims about a given discipline *qua* discipline be inspected with the same methodological tools and through the same criteria as any other claim of that discipline and, accordingly, be included in the body of knowledge or rejected from it. The reason for this peculiar interaction is that while in all other exact sciences the discipline and its subject matter are two separate entities of a completely different nature (heat and laws of heat, for example), in mathematics the nature of the discipline and the nature of its subject matter do not necessarily diverge (e.g., predicate calculus and inferential systems, categories of categories). The peculiar presence of reflexive mathematical thinking implies, as we have seen, a different interrelation of layers. The process of interaction will be explained below in greater detail.

There is another way in which mathematics differs from other exact sciences. Several contemporary philosophers of science, in arguing against the formerly accepted positivist view, have discussed the interaction between metaphysical conceptions and scientific theories. All of them agree that this interaction represents an important aspect of the rationality of science.[2]

Metaphysics is associated with every scientific claim about the world, because all such claims entail, along with their properly scientific content, an underlying view that is not directly testable. However, since it is not absolutely clear that mathematics deals in some way with the outside world, the necessary existence of such a metaphysical core in mathematics may be questioned. I choose not to deal with the crucial issue of the connection between mathematics and the outside world because it is beyond the scope of this paper. It is interesting, however, to point out a common image of contemporary mathematics, which sees in the modern axiomatic method an escape from this question or from any other philosophical concern.[3]

It is enough for me to point out the existence of a considerable amount of reflexive mathematical knowledge – namely a part of mathematics whose subject matter is mathematics and not the outside world – in order to assert the distinctiveness of the body of mathematical knowledge when compared to that of other exact sciences. This distinctiveness is particularly evident when the metaphysical underpinnings of different sciences are considered; the validity of the arguments justifying the claim that the sciences necessarily have a metaphysical core is debatable when it comes to mathematics. It is true that in some stages of the history of mathematics, research was carried on within a general philosophical outlook entailing a "scientific metaphysical core."[4] Likewise, we can sensibly argue that contemporary mathematical theories have metaphysical commitments. However, the fact that there is not always a clear distinction between the mathematical body of knowledge and its subject matter suggests that the arguments explaining the metaphysical commitments of physical theories – arguments that are based on the distinction between theory and its subject matter – will not apply to mathematics.

3. Linear and Nonlinear Progress in Mathematics

How science grows is a central issue in the philosophy of science of such authors as Popper, Kuhn, Lakatos, and others. Their research has shed new light on the history

[2] See for example Agassi's article "The Nature of Scientific Problems and Their Roots in Metaphysics," in Agassi 1975, 208–39; Bunge 1983, 200–207; Bunge 1985, 24–31; Elkana 1981; Lakatos 1970.

[3] This view, however, is not universally accepted and has met with harsh philosophical criticism. See for example Kreisel 1971, especially pp. 190–91. Moreover, Lakatos has shown how misleading it may be to use this contemporary belief as a guide while writing the history of mathematics. See Lakatos 1978, 2:43–60.

[4] Such was the case with Greek geometry and such was also the case with Cauchy's conception of the continuum, as has been convincingly shown by Lakatos (1978, 2:43–60).

of science and has produced concepts that help us see the growth of science as a process of constant change. This change is neither growth by sheer linear accumulation nor a process each step of which is fully determined by its predecessors. It is clear that the question of the growth of mathematical knowledge is connected with the more general one of science; but it should also be clear that the special character of mathematics suggests that the question be approached differently.

The aforementioned authors produced a deep transformation in the historiography of science, but this transformation was rather slow to include mathematics. Thus in the history of mathematics one still finds much more work written under the spell of the old image of science than in any other discipline. I propose to use the body/images scheme introduced above as a framework for describing the growth of mathematical knowledge, while taking into account the philosophical, historical, and social dimensions of mathematics – but without overlooking the distinctiveness of the discipline.

The idea that the growth of scientific knowledge is not a linear process gained substantial momentum after the emergence of the Einsteinian cosmology and the consequent rejection of Newtonian physics.[5] Mathematics, however, was never confronted with a crisis of the magnitude of that caused to physics by the rejection of Newton's theories. The so-called foundational crisis of the turn of the century, for example, did not call into question a single *theory* in the body of classical mathematics. It did pose some interesting and deep questions for a small (but important) sector of the mathematical community, and the outcome was a series of significant results that increased mathematical knowledge; but no previously proven results were rejected as a consequence of that process.

It is true that, according to some claims *about* mathematics proposed during the first decades of the present century, some previously accepted methodological presuppositions of mathematical research were to be limited (e.g., some rules of logic, according to the intuitionists). But it is significant that even some of the proponents of these limitations undertook the task of "saving" as much of the classical results of mathematics as possible, within the limits of their more restrictive methodology. The "crisis" did not introduce new results that invalidated old ones. The discovery of Russell's paradox undermined, perhaps, Frege's philosophical project, but not the lion's share of Frege's mathematical results. However, a deep change occurred at the level of the images of knowledge.

The discovery of a new theorem, proof, or concept does not warrant saying that knowledge has changed. It is the images of knowledge (which are determined socially, philosophically, by the interaction with other sciences, and so on) that determine, as in the other sciences, the way in which a new item will be integrated into the existing picture of knowledge, whether it will be considered important or be ignored. Eventual changes in the images of knowledge may later transform the status

[5] The idea itself, however, was already known in the nineteenth century. See Laudan 1973.

of existing pieces of knowledge and produce a different overall picture of mathematics. Change proceeds not only *quantitatively*, by the addition of new results or concepts. Although these additions are of course fundamental to the growth of mathematics, real change occurs only insofar as the quantitative growth is accompanied by a *qualitative* new appreciation of the body of knowledge.

Qualitative change is usually a change in the images of knowledge, and it is essentially different from quantitative change in the body of knowledge. For the latter we have quite clear criteria. Thus a theorem is added to the body of knowledge when a proof is found, and a valid proof may usually be clearly distinguished from an invalid proof.[6] What change in the images of mathematical knowledge is, and how and when such change occurs, is more difficult to define. It is precisely the task of the historian of mathematics to characterize the images of knowledge of a given period and to explain their interaction with the body of knowledge – and thus to explain the development of mathematics. To illustrate the process let us briefly consider the development of lattice theory.

At the end of the nineteenth century two alternative definitions of lattices had been formulated, by Richard Dedekind and by Ernst Schröder; but they did not attract any attention from other mathematicians. Dedekind had even found a considerable number of interesting theorems and had indicated which kind of lattices were important and should be a focus of concentration. Dedekind was far from peripheral to the mathematical community; yet despite his reputation, this part of his work was simply ignored for thirty years. In the 1930s, lattices were redefined by Garret Birkhoff and Oysten Ore, and the real development of the discipline started.[7] Without delving into the reasons for this gap of thirty years, we can learn from this example that the relevance of mathematical theories cannot be judged by their intrinsic features. Thus, among the main reasons for the rebirth of the theory, the enormous influence of van der Waerden's classic *Moderne Algebra* (1930) was central. Although lattice theory is *not* one of the algebraic theories exposed in van der Waerden's book, the strong impact produced by publication of the book encouraged research in such abstract branches of mathematics as lattice theory (Mehrtens 1979, 144–65). Thus by producing a change in the images of knowledge, *Moderne Algebra* helped to revive research in lattice theory, which in turn produced a change in the body of mathematical knowledge. Relevance of mathematical theories is open to debate, and the images of knowledge play a central role in this debate.

The original lack of interest in the theory of lattices was not produced by the rejection of any part of the body of knowledge, nor did the eventual growth of interest in it imply such a rejection. Likewise, while there may be severe disagreement between such mathematicians as Benoit Mandelbrot and those of the Bourbaki school as to the fruitfulness or the convenience of their respective approaches,[8] their results, established through proof, are still valid and mutually

[6] This is a somewhat problematic issue, which I shall discuss below.

[7] For a comprehensive history of lattice theory see Mehrtens 1979.

[8] For Mandelbrot's opinions on Bourbaki, see Albers and Alexanderson 1985, 222.

compatible. A new mathematical theory may lead to the abandonment of an older one by making it appear uninteresting or perhaps superfluous, but never wrong.[9] In contrast, Einsteinian physics and the metaphysical view that it entails are incompatible with Newton's and we must choose between them. Choices must often be made in the body of physics but never in the body of mathematics.

However, while the acceptance of new mathematical theories does not imply the rejection of old ones, that does not mean that *nothing* is ever rejected from the body of mathematics; in fact there have been serious mistakes in the history of mathematics, often made by great mathematicians, that took a long time to be detected.[10] Indeed, they deserve to be discussed in some detail.

Traditionally, mistakes and uncertainty – when considered at all – were thought of as only marginal events in the history of mathematics. In the last two decades, however, their role has been increasingly acknowledged, and they now assume a central role in the philosophy and historiography of mathematics. The first to assign mistake and uncertainty a central role in the growth of mathematics was Lakatos (in 1976). He showed the heuristic value of such mistakes, by describing the growth of mathematics as a process of systematically ferreting out mistakes in existing and new mathematical proofs and concepts. Criticism of proofs, says Lakatos, leads to new proofs and concepts, which are then themselves subjected to criticism. Lakatos' approach has facilitated a balanced evaluation of the historical dimension of mathematics; but at the same time it has encouraged the disclosure of "mistakes" even where they do not in fact exist. Interestingly enough, Lakatos himself showed, by means of an example, how such exaggeration occurs and should be avoided.

Cauchy's mistaken proof that a converging sequence of continuous functions always converges to a continuous function is one of the best-known examples of "error" in the history of mathematics. Lakatos has suggested an alternative historical interpretation that explains why Cauchy's proof should not be considered an error. However, before one takes sides in this issue, or any other, one should first define what an error is in the history of science. I shall not try to do that here; rather, taking for granted that mistakes do occur in the history of mathematics, I shall explain their role in the growth of mathematical knowledge. I shall now examine the classical example of geometry, in order to explain my views on the issue.

Geometry came a long way from Euclid's *Elements* to Hilbert's *Grundlagen der Geometrie* (1899), and of course the latter was not the last stop on the journey. It is common knowledge that our understanding of geometry is quite different from that of the ancient Greeks. But wherein does that difference lie? First of all, some specific "shortcomings" of the *Elements* have been pinpointed throughout history. A classic example is the discovery of Euclid's implicit use of Pasch's axiom. In 1882 Pasch

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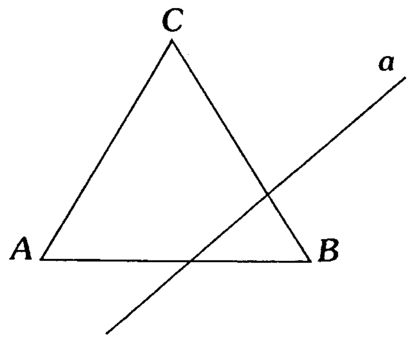
[9] Crowe points out (1988, 263) that "massive areas of mathematics have, for all practical purposes, been abandoned." Therefore, he claims, the assertion that mathematics is cumulative is one of the widely held misconceptions about mathematics and its history. From my analysis above, it follows that Crowe's premise does not yield his conclusion.

[10] An interesting list of such mistakes appears in Tymoczko 1985, 18–20, 171–75.

published his own new exposition of geometry, in which he demanded that all premises of geometry be stated explicitly "even if they are trifling," taking what later became Pasch's axiom as one such premise.[11] Stated intuitively, the axiom establishes that if a line a enters the interior of a triangle ABC through one of its sides (say CB) it also leaves it through another (say AB).[12]



The axiom may indeed seem superfluous, but it is so only when we deal with actual diagrams. Nevertheless, from a strictly logical point of view, this assumption is independent of the other axioms and must be stated explicitly. Euclid was using the axiom implicitly,[13] and Pasch stressed the necessity of stating it explicitly to ensure the truly axiomatic status of his geometry. Later, in 1936 – after several editions of Hilbert's *Grundlagen* had already been published including Pasch's axiom – van der Waerden proposed a more general axiom, from which Pasch's own axiom could be derived as well as another of the axioms included in Hilbert's system, thus further reducing the number of axioms.[14]

These are two instances of "local" errors. Once they have been detected and corrected, they can be taken into account by the system through minimal changes in the latter; the discovery of such errors does not lead to a rejection of the whole system of theorems and deductions. Improvements such as those introduced by Pasch and later by van der Waerden are certainly local improvements.

As noted earlier, there could be some disagreement at this point about the very definition of such cases as errors. However, what interests us now is just what kind of changes are necessary for the body of knowledge to absorb the improved claim. Thus

[11] For a detailed exposition of Pasch's views on geometry, see Torreti 1978, 210–18.

[12] The formal version of the axiom is quoted in Hilbert 1971, 5, axiom II, 4. Pasch's axiomatic system originally appeared in Pasch 1882.

[13] See for example Euclid, *Elements,* Prop. I, 10; in the bisection of a segment, Euclid presupposes that the line bisecting an angle of a triangle will also intersect the opposite side. This is obviously true in a diagram of a triangle; but as noted above, in a more logically rigorous conception of geometry it has to be explicitly stated.

[14] Cf. Hilbert 1971, Supplement 1, p. 200. Bernays, who wrote the supplement, cites van der Waerden 1934–36 as the reference.

our question here is not whether Euclid should (or could) have paid attention to his "careless" use of unwarranted presuppositions, rather, it is this: after Pasch realized the need for the explicit statement of his axiom, what changes did its introduction necessitate in the body of geometrical knowledge? Or, to take the above example from the history of calculus, the question here is not whether Cauchy's "incautious" use of the concept of "convergence" (instead of "uniform convergence") in his proof was a mistake; rather, it is this: after Seidel defined uniform convergence, what kind of changes did the introduction of this new concept, and of the proofs based on it, necessitate in the body of knowledge? My answer is that it necessitated only "local" changes and that, moreover, this is the only kind of change that could have occurred when trying to overcome this kind of errors in mathematics.

Discoveries leading to local improvements such as those described above do not occur exclusively in mathematics. They appear in what Kuhn calls the "normal" stage of any science. The paradigmatic example is the improvement of Ptolemy's astronomy by means of successive additional epicycles. Eventually, however, a new improvement essentially different from the addition of epicycles was suggested – namely, a different theory, capable of surmounting the difficulties confronted by Ptolemy's theory but *incompatible* with it. It is precisely improvements of this sort – those that replace old theories with more powerful, *mathematically incompatible* theories – that are foreign to the history of mathematics.

In addition to local improvements resulting from the correction of local errors, geometry has also changed through generalization. The introduction of non-Euclidean geometry has rendered Euclidean geometry a particular case of more general "geometries." The addition of an impressive corpus of specific results of non-Euclidean geometry has not, however, invalidated the old results of geometry in any possible way – it has merely changed their conceptual setting. In other words, while the logical inferences constituting proofs have remained untouched, the meaning of certain proofs has changed significantly and its scope has been considerably widened.[15] Only the old idea, that Euclidean geometry conveyed a true description of physical space, has proved to be not necessarily true. The latter, however, is a physical claim that belongs to the images of mathematical knowledge, but not to the body of mathematical knowledge.

I have described two different kinds of changes undergone by the body of knowledge of geometry. Such changes also produced a revolution in the images of geometrical knowledge and simultaneously triggered interesting and fruitful processes of reflexive thinking in mathematics. The theorems derived from the Euclidean axioms remained valid inferences (with the pertinent local changes) but their status and significance for us have totally changed during the process, and of course this new outlook spread to many other mathematical fields. This broad transformation in the body of knowledge raised problems in the reflexive realm of mathematics. For instance, mathematicians were compelled to attempt to understand the nature of

[15] One of the most serious limitations of Lakatos' methodology of "proofs and refutations" is its inability to account for this kind of change. I shall consider this point below.

axiomatic systems, to understand the nature of mathematical inference, and to study the interconnection between geometry and other axiomatically defined systems in mathematics.

So much for the role of error in the growth of mathematical knowledge. We can now go on to discuss the relation between images of mathematics and the role of proof in mathematics. Usually there exists a high degree of agreement among mathematicians about what constitutes an acceptable proof. The criteria according to which this decision is made belong to the images of mathematics and, although they may seem universal, are subject to criticism, debate, and change. These criteria have never been explicitly formulated. Rather, they appear as a tacit code shared by practitioners of a given branch of mathematics. As such, they are the outcome both of a historical process of reciprocal interaction with the body of mathematics and of external influences. It is here that the nonlinearity of mathematical growth may be most clearly identified, for the criteria of proof have not progressed unfailingly from naiveté to sophistication, from concreteness to abstraction, from looseness to rigor.

By the turn of the century, such mathematical works as Russell and Whitehead's *Principia,* Hilbert's *Grundlagen der Geometrie,* and Frege's *Grundlagen der Arithmetik* induced the adoption of strict and restrictive standards for proof. In the decades that followed it was assumed that all mathematical proofs should, and in principle could, conform to those standards. Over the past years, however, proofs that do not, and cannot even in principle, fit those restrictive standards of rigor have been increasingly used in mathematics. Although proofs of this kind cannot today be considered mainstream, they are being increasingly accepted and used by the mathematical community. The need to reevaluate the existing images of mathematics thus becomes increasingly acute. Many recent works in the philosophy of mathematics reflect this reevaluation, and they may be characterized as an attempt to articulate more realistic images of mathematics.

Some of the new kinds of proofs to which I refer above are computer-assisted proofs (e.g., relating to the four-color problem); "very long proofs" (e.g., relating to the simple-groups classification theorem); and proofs whose truth is established by claiming that their probability to be so is "extremely high" (e.g. see Rabin 1976 on the distribution of prime numbers).[16]

The existence of proofs of this type lends further credence to the growing acknowledgment of the ubiquitous presence of uncertainty in mathematics. Those who want to accept such proofs as legitimate (and ignoring them is becoming more and more difficult) find it hard to go on accepting the definition of proof provided by the formalistic conception of mathematics – namely, that proof is a chain of unquestioned deductive inferences within a formal axiomatic system, produced according to inference rules prescribed in advance. It may also become increasingly harder to claim that the formalistic conception of proof is the ideal to which mathematicians

[16] Discussions of the philosophical problems raised by computer-assisted proofs appear in Tymoczko 1979 and Detlefsen and Luker 1980. On "very long proofs," see Kitcher 1983, 40ff. On proofs based on probabilities, see Kolata 1976. A general discussion of all these issues appears also in Davis 1972.

aspire, although they sometimes fall short of it. The new images of mathematics are trying to shift the borderline between proof and nonproof so as to include such proofs as those mentioned above. Amid all these changes, however, one image of mathematics has not changed and does not seem likely to change – namely the idea that mathematical knowledge is legitimated through proof. The need for proof as such, then, has been preserved, while the criteria of what counts as proof have changed. All these changes occur at the level of the images of mathematics, while simultaneously new proofs and theorems are continually being integrated into the body of mathematics.

In view of the acceptance of new criteria for proof and the increasing awareness of the role of uncertainty in mathematics, many authors have remarked on the social dimension of mathematics and, in particular, of proofs. Philosophers and methodologists of mathematics who favor a quasi-empirical approach tend to consider proof, above all, as a process of communication among researchers trying to convince themselves of the truth of certain assertions. Once again Lakatos – to whom a proposed proof was no more than an invitation to fellow mathematicians to find the mistake in the argument – was one of the pioneers of this conception of proof.

A thought-provoking description of the social dimension of the acceptance or rejection of proofs appears in Davis and Hersh 1986. In their account, a published mathematical proof

> is nothing but a testimony that the author has convinced himself and his friends that certain "results" are true, and it presents part of the evidence on which this conviction is based. (p. 66)

Davis and Hersh reject an assumption that has long been unquestioned[17] and assert that a complete formalization of mathematical arguments is not only impracticable but also unilluminating and absolutely unnecessary. The belief in the correctness of a given proof is, therefore, the result of social agreement (among practitioners of the relevant mathematical field) and no more than that. This agreement is not definitive, and it is subject to criticism. Thus absolute certainty cannot be attained in mathematics.

> We do not have absolute certainty in mathematics; we may have virtual certainty, just as in other areas of life. Mathematicians disagree, make mistakes and correct them, are uncertain whether a proof is correct or not. (Hersh [1979] 1985, 20)

Therefore, conclude Davis and Hersh:

> Our confidence in the correctness of our mathematical results is not absolute, nor is it fundamentally different in kind from our confidence in our judgment of the physical reality or ordinary daily life. (1986, 73)

Although Davis and Hersh's description is accurate, their conclusion does not necessarily follow from it. A quasi-empirical approach to mathematics need not lead to an equation between mathematics and other kinds of knowledge.

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17 Such as that expressed in Bourbaki 1968, 8.

Davis and Hersh seem to ignore the special character of the body of mathematical knowledge, as do many other authors who stress the social dimension of mathematical activity. The fact that the criteria for acceptance and rejection of proof are in part socially determined and that the identification and (local) removal of error are in part historically determined does not change the fact that a stable body of mathematical knowledge has been progressively created. A considerable number of theorems and results have been absorbed into this body of knowledge, and it seems likely that no future theorem will force us to delete organic parts of it.

Mathematics grows differently than the empirical sciences, and Davis and Hersh overlook this difference when they equate mathematical knowledge and other forms of knowledge. I think that if pushed to the wall most quasi-empiricists who defend ideas similar to those of Davis and Hersh will not deny that the body of mathematical knowledge is, in the long run, cumulative. When mistakes are discovered in proofs, the theorems themselves are usually not refuted. In the empirical sciences, in contrast, mistakes in arguments or experimental data may often lead to the refutation of theories, or at least to questions about their truth. More generally, the central difference between mathematics and the empirical sciences is that new mathematical theories do not force us to reject old ones. As Hilary Putnam wrote in a different context:

> [The] chief characteristic of empirical science is that for each theory there are usually alternatives in the field, or at least alternatives struggling to be born. As long as the major parts of classical logic and number theory and analysis have no alternatives in the field – alternatives which require a change in the axioms and which affect the simplicity of total science, including empirical science, *so that a choice has to be made* – the situation will be what it always has been. (Putnam 1967, 51; italics added)

We are faced, therefore, with two apparently contradictory features of mathematical knowledge. On the one hand, its development has an active sociohistorical dimension, the existence of which suggests a certain degree of uncertainty. On the other hand, the body of knowledge is peculiarly stable. It has continually grown (and has been amended *locally*) as a result of thousands of years of research. Its truth status has never really been threatened by alternatives; on the contrary, it has been reinforced by the successful application of mathematics to empirical sciences. This apparent contradiction reveals a tension inherent in the quasi-empirical approach: between a tendency to *relativism* (which results from the sociohistorical character of the *creative process* of mathematics) and the *objective status* of the *end product*. This tension is not explicitly mentioned by quasi-empiricist authors, and it is certainly not resolved in their writings. It may be resolved, however, by accepting that mathematical knowledge comprises both images of knowledge and the body of knowledge. Thus the sociohistorical dimension plays a central role in determining the images of mathematics, and only through them does it affect the content of the body of mathematics. The body of knowledge is not directly affected by the action of external factors, so its objectivity and stability are preserved from relativism.

To summarize, mathematical knowledge comprises two interacting layers: a hard core which accumulates over time, and shifting images of knowledge. Some components of the hard core are furnished by the images of knowledge, as the latter enter the hard core and stay there as reflexive thinking. It may sometimes be hard to distinguish between the pure body of knowledge and pure images of knowledge. These constitute the extremes of a continuum, and what we have called reflexive thinking (as distinct from pure images of knowledge) stands somewhere in between. Reflexive thinking is clearly part of the body of mathematical knowledge, because it is produced as is any other piece of mathematical knowledge and is justified by proof. On the other hand, it is produced by concentrating on purely second-order problems, and hence it is related to the images of knowledge. Mathematical knowledge includes *all* the layers, and their separation is done for analytical purposes only. They are in a state of "ongoing dialectical debate," which we must try to understand if we want to understand the historical process of the growth of mathematics. We cannot really separate hard-core mathematical knowledge from the way it is looked upon mathematically, just as we cannot separate form from content (in mathematics or in any other field): they are conditioned by each other. This analysis can often be formulated only in hindsight.

4. Alternative Views

As mentioned earlier, several authors in the last two decades have presented their own conception of the growth of mathematical knowledge. Some of them represent attempts at translating approaches used by historians of empirical science. It is usually hoped that such conceptions will close the gap existing between those two related fields. It may be illuminating to discuss some of those views and to assess their value by comparing them with the body/images scheme proposed here.

It is interesting to note, first of all, that in many cases alternative conceptions of the history and philosophy of mathematics may be subsumed under the body/images scheme. Consider, for example, the "ten laws concerning mathematical change" suggested in Crowe 1975, 1976. Rather than being universally valid laws, they seem to describe certain specific historical instances of the interaction between the two layers of mathematical knowledge. Crowe even mentions the existence of various layers in mathematics, although he does not explain how they interact. Thus one of his laws states:

> The "knowledge" possessed by mathematicians at any point in time is multi-layered. A "metaphysics" of mathematics, frequently invisible to the mathematician yet expressed in his writings and teaching in ways more subtle than simple declarative sentences, has existed and can be uncovered in historical research or becomes apparent in mathematical controversy. (Crowe 1976, 469–70)

By replacing the term "metaphysics" with "images," one can see this law as a particular instance of the scheme I have presented. Like Crowe's, the work of Lorenzo (1977) can also be subsumed under the paradigm I have suggested. Lorenzo speaks of an "ideology" of mathematical practice, but unfortunately he does not develop his ideas systematically and leaves many questions unanswered.

As noted above, one of the first systematic presentations of the new approach to the philosophy and history of mathematics was the work of Lakatos. In his *Proofs and Refutations* (1976), Lakatos used a classic historical example to demonstrate that mathematics grows according to a scheme of "conjectured proof-refutation-improved proof" parallel to that proposed by Popper (1963) for empirical science. Lakatos' work pioneered the very idea that it is possible to attack the positivist view of science in its last and most impregnable fortress, mathematics. He was the first to attribute to error an active role in the development of mathematics instead of an accidental, marginal one. Lakatos' work has also been the target of sustained criticism, on which I shall not comment here.[18] I would like, however, to mention some aspects of that criticism which are related to the present discussion.

Lakatos' approach fails to distinguish mathematics from empirical science. He cannot account for the cumulative character of the body of mathematical knowledge or for its objectivity. Nor can he explain any change that is not local. Interestingly enough, Lakatos overlooks the distinction that Popper drew between empirical and mathematical knowledge. Only in mathematics, Popper claimed, can a theory be considered definitely true; wrong proofs that are not discovered in the long run are the exception rather than the rule (Popper 1963, 197).

In his later work Lakatos developed his own approach to the history and philosophy of science in general, an approach he termed "Methodology of Research Programs" (MRP) (see Lakatos 1978, vol. 1). The possibility of applying this approach to mathematics was explored in Hallett 1979. Hallett's primary task was to characterize universally a "progressive" research program in mathematics as opposed to a "stagnated" one. He claimed that there exists a wide consensus among mathematicians regarding the relevance of different mathematical theories, and he tried to define systematically the basis of this consensus. He concluded that the consensus was based on the demand that a mathematical theory resolve important problems it was not originally conceived to solve. He constructed a concise formulation of this criterion, which allegedly enables a clear-cut universal identification of progressive theories (1979, 10).

Hallett takes Lakatos' methodologies of "proofs and refutations" and MRP for granted; valid criticism of Lakatos can therefore be applied to Hallet's claims as well. Certainly Hallet's criterion is important, but one wonders whether it is indeed as universal as Hallett claims. Moreover, I think that the very idea that a criterion of this sort can independently determine the relative importance of theories is wrong. Determining the relative relevance of theories is a central component of the images

[18] Such criticism may be found, for instance, in Dauben 1988, 179–83; Feferman 1978; Hacking 1979; and Holton 1978, 105–7.

of knowledge, but it is not the only, or even the most important, determinant of the growth of the body of knowledge. Many factors, and not only a clear-cut answer to a succinctly formulated question, determine the relative relevance accorded to different mathematical theories. These may include personal, sociological, and philosophical factors, and not only purely rational ones.

A further attempt to apply theories of the growth of science to mathematics is related to Kuhn. The problems arising in connection with the application of Kuhn's theory to science in general,[19] arise with equal or greater force when it is applied to mathematics. Clearly, if truth in the body of mathematical knowledge is established by proof, it may not be said to be determined by a conventionally established paradigm. Some of Kuhn's concepts might be useful, though to a limited extent, in studying the images of mathematics (paradigm, crisis, disciplinary matrix, etc.).

The possibility of applying Kuhn's ideas to mathematics was explored in Mehrtens 1976. Of the concepts that Kuhn developed, Mehrtens believes that only the concept of "anomalies" is pertinent to mathematics. It is noteworthy, however, that what Mehrtens termed anomalies in the history of mathematics (e.g., Kummer's wrong belief that the methods and theorems of the theory of natural numbers can be extended to algebraic integers, and his attempt to prove Fermat's theorem by wrongly assuming that those numbers may be uniquely decomposed into prime factors) are instances in which accepted images of knowledge lagged far behind the development of the body of mathematics. In each of the cases mentioned by Mehrtens, when new, more realistic images were adopted by the mathematical community, the anomalies disappeared.

A more fully articulated attempt to use Kuhn's ideas to describe mathematical progress is that of Philip Kitcher.[20] Like Kuhn, Kitcher believes that observational statements of science are produced *within* a theoretical conceptual framework (which he calls *scientific practice* instead of paradigm) and not independent of it. At variance with Kuhn, however, Kitcher denies that theoretical change takes place in a discontinuous fashion – namely, through the revolutionary change of paradigms. He attempts to apply a similar thesis to mathematics, while arguing that mathematical and scientific change are similar. Kitcher's picture is similar in some important aspects to the one I have presented here.

In order to develop his ideas, Kitcher first examines the three main arguments on which the alleged difference of scientific and mathematical knowledge is based and later rejects them. The arguments presented are the following: (1) there are no true debates in mathematics; (2) there is a corpus of mathematical truths that has long remained unquestioned; and (3) the only mathematical claims that have been abandoned in the history of mathematics are those that were unjustifiably held from the outset. Let us see how Kitcher deals with these arguments.

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[19] A thorough discussion of Kuhn's theory appears in Lakatos and Musgrave 1970.

[20] These views are presented in detail in Kitcher 1983. Kitcher 1988 contains a more succinct presentation of them, including some reformulations of earlier ideas.

Kitcher refutes the first argument by presenting historical examples of mathematical debates such as the Kronecker-Cantor debate and the Leibnitz-Newton debate. The second argument is rejected by asserting that in fact both mathematics and the empirical sciences contain truths which have long been unquestioned. Just as Euclid's theorems in mathematics have been accepted since the time of the Greeks, so too has the claim that a feather floats in water. Regarding the third, Kitcher examined the history of mathematics and analyzed some epistemic situations where mathematicians were justified in holding beliefs that were revealed to be false only as a result of later work.

The body/images scheme uncovers some problems in Kitcher's arguments. The highly significant debates invoked to refute the first point were debates about the images of mathematics and not debates between two reciprocally exclusive mathematical theories. Kitcher's argument regarding the second point is unconvincing since the comparison on which it is based is unsound. Euclid's theorems are part of a *system* of knowledge, while the assertion that a feather floats is a claim about an isolated fact. Aristotle's explanation for the feather's buoyancy, on the other hand, is no longer accepted, while Euclid's proofs are. Regarding the third point, as remarked earlier, the question is not whether people were justified in holding some claim but how the improved claim has been reincorporated into the whole system. This reincorporation is different in mathematics than in empirical science, since in the former it never implies the rejection of whole theories. In fact Kitcher himself realizes that such a difference exists, and he states that "we do not seem to find in mathematics . . . the analogs of the discarded theories of past science" (1983, 158). Thus Kitcher's own view of mathematical change concedes that "mathematics is cumulative in a way that natural science is not"; but his "concession to the thesis that mathematics is cumulative should not be taken to invalidate the project of describing mathematical methodology" (p. 161). Let us briefly consider his description.

Kitcher proposes to see mathematics as a series of historically changing "mathematical practices," which the historian of mathematics must discern and whose evolution he must explain. A "mathematical practice" is a quintuple $<L,M,Q,R,S>$: a language, L; a set of metamathematical conceptions, M; a set of accepted questions, Q; a set of accepted inference procedures, R; and a set of accepted statements, S. This scheme allows a common description of mathematical and scientific change, while at the same time accounting for the special quasi-cumulative character of mathematics. Thus while most scientific change is produced as a response to new observations, most mathematical change is produced as a response to tensions among the various components of the practice at a given point in time.

Note, however, that among the five components of a given mathematical practice L, Q, and R belong to the images of mathematics; S constitutes the body of knowledge, and M represents the body of reflexive knowledge, which is exclusive to mathematics. Thus while it is clear that these are indeed important points to be considered while analyzing mathematical change, one may wonder whether L, Q, and R are the unique parameters through which the images of knowledge should be examined.[21] Kitcher shows their relevance through a detailed case study; but the question remains, in my view, open to debate. In addition, the peculiar reflexive character of the interaction of the different components of the practice does not come to the fore in Kitcher's account.

Quasi-empirical views of mathematics developed from an increasing dissatisfaction with the contribution of foundations research to the philosophy of mathematics. Quasi-empirical views demand that we analyze "what mathematicians really do" in their work, and not an idealized picture of mathematical knowledge. I have thus far presented quasi-empirical views that stress the sociohistorical character of mathematical activity. There have also been mathematicians who proposed to bring about a revival in the philosophy of mathematics by looking at "what mathematicians really do," but laying the stress on their *end product*. Such is the case of Saunders MacLane 1981, 1987a, or of Solomon Feferman 1984, 1985.

Feferman's views are interestingly relevant to the present discussion. In his search for a new, more fruitful direction for the philosophy of mathematics, Feferman rejected both quasi-empirical views such as those discussed above and the attitude of mathematicians who avoid philosophical concern by hiding behind a formalist-Platonist shelter that cannot in the long run be sustained. His attempt to foster a philosophy of mathematics based on an analysis of "what mathematicians really do," however, does attribute a leading role to logic in explaining the peculiarities of mathematics. On the other hand, the fact that the "great logical systems" (such as Russell's, Quine's, or Rosser's) have failed to provide a sound basis for all of mathematics, leads Feferman to the conclusion that the search for such a system should be abandoned. The role of logic in the production and justification of mathematical knowledge should be stressed; but at the same time it should be explained in terms other than those proposed thus far in the foundationalists' attempts.

Feferman's own proposal consists in providing "logical foundations at work" for mathematics. In order to do this, he exhaustively examined the various "clarification activities" that have been undertaken within different branches of mathematics. He classified those activities under ten tentative headings – a classification that should provide the basis for an explanation of mathematics' singular character and growth. Among Feferman's categories we find, for instance, "Conceptual Clarification" – namely, the definition of frequently used informal concepts in terms of better-understood, basic ones. Further headings are "Dealing with Problematic Concepts

[21] In fact, there is good reason to think that they are not. For example, a similar view of science appears in Bunge 1983, 200–207, and Bunge 1985, 24. However, parallel to Kitcher's five components of the scientific practice, Bunge mentions ten components of a "scientific field of knowledge" $<C,S,D,G,F,E,Q,P,A,O,M>$: a community of researchers, C; a society, S, which supports or at least tolerates the activities of the members of C; a domain of discourse, D; a general philosophical conception, G; a formal background, F; a specific background, E; a set of problems, P; a background of accumulated knowledge, A; a set of objectives, O; and an accepted methodology, M. Should we prefer the ten-component scheme over the five-component one, or the other way round? The body/images division of knowledge is in any case simpler, clearer, and more soundly justified.

and Principles," which led in the past to clear formulations of concepts (e.g., imaginary and complex numbers) and principles (e.g., the axiom of choice), and "Axiomatization" (of mature theories), etc. Of special interest is the category that according to Feferman has been insufficiently appreciated: "Reflective Expansion of Concepts and Principles." Examples of this kind of foundational activity are found in the passage to $\mathbb{R}^n$ after plane and space geometry have been understood in terms of $\mathbb{R}^2$ and $\mathbb{R}^3$, or in the definition of spaces of operators after differentiation and integration have been understood as such. What, according to Feferman, characterizes this mathematical way of creation?

> Basically, this is a form of *generalization,* but of the following particular character: at a certain point one reflects on what has led one to accept and work with certain concepts, and sees that a much more general concept is *implicit* in accepting that. (1985, 245; italics in the original)

Feferman includes some additional examples from different branches of mathematics, in particular from different branches of logic, and explains:

> From a logical point of view, our interest here is in whether we can make theoretical sense of *describing all the concepts and principles that one ought to accept if one has accepted given concepts and principles or,* put more succinctly, *describing all the concepts and principles implicit in given ones.* (Ibid., 246; italics in the original)

Here, I think Feferman has subsumed two essentially different things under the same heading. The awareness that it is possible to generalize from $\mathbb{R}^2$ to $\mathbb{R}^n$, and especially that it is worth doing so, is a good example of an image of mathematics. Generalization of this sort has often been a successful strategy. However, it has also on occasion led mathematical research up a blind alley. In contrast, the logical inquiry to which Feferman refers is part of the body of mathematical knowledge within a certain branch, the aim of which is to elucidate in an abstract and general fashion the logical basis and the limitations of such a passage whenever it is successfully done. While the two aspects are closely related, the first, as a part of the images of knowledge, may only direct the mathematical research of the second, which belongs to the body of knowledge or, to be more precise, to the body of the reflexive knowledge of mathematics.

Feferman exposed his ideas as a program to be developed in the future, rather than as a definitive picture of mathematics. As described in his articles, it is neither clear how these activities account for the growth of mathematical knowledge in general nor why they are so peculiar to mathematics. All this is more clearly understood, I think, when Feferman's account is seen in the light of the body/images separation of mathematics, and when it is remembered that reflexive thinking is possible only in mathematics. What Feferman has achieved is, in my view, no less (but also no more) than an exhaustive and authoritative survey of present reflexive knowledge in mathematics and its role in the ongoing debate on the images and body of mathematical knowledge. In fact the "foundational ways" that Feferman expounds in his articles include many ideas from mathematical branches other than logic. While this is probably not what Feferman intended, it turns out that the special role logic plays in the explanation of the singular character of mathematical knowledge is only a part of the role of reflexive thinking in general in the evolution of mathematics.

Saunders MacLane, too, urged that a new direction be developed in the philosophy of mathematics. The main problem he saw in existing works in the field is that recent advances in all branches of mathematics are never taken into account when the nature of mathematical knowledge is discussed. As extreme examples of this tendency, he mentions Kant and Wittgenstein, whose philosophies of mathematics barely examine the basic facts of arithmetics and geometry. In contrast, MacLane's own picture is based on an exhaustive presentation of the achievements of contemporary and past mathematics.[22]

MacLane seeks to describe the genesis of mathematical concepts, to define the subject matter of mathematics, to account for the peculiarities of mathematical knowledge, and to explain what makes a particular mathematical field or pursuit more interesting and relevant than another. He answers all these questions by examining exclusively the body of mathematics, with no reference to external factors.

The deep and delicate tissue of interconnections among diverse mathematical theories was one of the central issues in MacLane's own mathematical research. These interconnections appear as a central concern of his philosophical inquiry as well. Unfortunately, MacLane remains at the level of tentative formulation, and he does not clearly articulate a philosophical picture of mathematics. Rather he poses a score of questions that he considers central to elucidating the nature of mathematical knowledge and answers each of them by presenting long lists and complicated tables in which the interconnection of ideas behind mathematical activity comes to the fore. Thus, for example, he claims that his presentation of mathematics also suggests preferred directions for future research. Regarding this issue MacLane writes:

> Mathematics, in our description, rests on ideas and problems arising from human experience and scientific phenomena and consists in many successive and interconnected steps in formalizing and generalizing these inputs. Thus mathematical research can be directed in a wide variety of overlapping ways:
> (a) Extracting ideas and problems from the scientific environment;
> (b) Formulating ideas;
> (c) Solving externally posed problems;
> (d) Establishing new connections between mathematical concepts;
> (e) Rigorous formulations of concepts;
> (f) Further development of concepts (e.g., new theorems);
> (g) Solving (or partially solving) internal mathematical problems;
> (h) Formulating new conjectures and problems;

[22] MacLane outlined his ideas on various occasions – e.g. MacLane 1981, 1987b. Those ideas are fully developed in MacLane 1987a.

(i) Understanding aspects of all the above. (1987a, 450)

MacLane's list "does put special emphasis upon extracting and formulating ideas and on understanding their import," in contrast to the view that stresses only the importance of new theorems and problem solving.

I think that MacLane's specifically philosophical claims about mathematics are in need of much further refinement. On the other hand, his detailed description of many branches of mathematics may be used as sound evidence for the picture I described in the earlier sections of this paper. In particular, MacLane takes pains to underline all through his book (and the long quotation above is a good example) the central role of reflexive thinking in mathematics.

5. Images of Knowledge in Twentieth-Century Mathematics

Reflexive thinking in mathematics reached a peak in the present century. The modern axiomatic method enabled a new kind of reflexivity in mathematics that does not exist in any other exact science. Many examples may be mentioned of the results achieved in reflexive fields of mathematical research. For instance the logicist school's efforts, which led – through the study of given axiomatic systems – to the articulation of specific propositions (e.g., the axiom of reducibility or the axiom of infinity) essential to determining whether mathematics can be reduced to logic.[23] Similarly, through the analysis of a set of postulates Sheffer introduced the "stroke symbol," which reduced the number of basic operations of Boolean algebras to one, thus basing the entire theory on a single undefined term and circumventing the supposition of the existence of a zero element, a unity element, or a complement for each element of the algebra (Sheffer 1913; Wilder 1952, chap. 9). Finally, Gödel's theorems, through a rigorous deductive argument formulated within an axiomatic system, set definite limits to reasoning within axiomatic systems.

The success of this reflexive research may, to a great extent, account for foundationalism's domination of the philosophy of mathematics since the turn of the century. Most of the debates during the so-called foundational crisis were conducted not as philosophical debate but rather as *mathematical* research. This research consisted in the study of mathematical axiomatic systems designed to justify and develop the tenets of each of the "philosophical schools" involved in the debate (i.e., the intuitionist school, the logicist school, and the formalist school). This is not to suggest that there were no philosophical issues at stake. There were, and they were very important ones. However, these debates occurred not on the philosophical level but on the mathematical level.[24]

Philosophical debates about mathematics came to be understood exclusively in terms of foundational research – *mathematical* research in branches that happen to

[23] A good exposition of this point may be found in Wilder 1952, chap. 9.

[24] Errett Bishop (1975) argued this point while presenting his views on the controversy between intuitionists and formalists. See esp. pp. 509 and 515.

deal with problems related to "foundations." Moreover, the striking mathematical insights attained through reflexive research produced a new central image of mathematics – namely, that the only meaningful assertions about mathematics are gained through technical mathematical research. To quote Errett Bishop:

> People tell me in so many words that when I was proving theorems, I was doing something original and worthwhile; but when I started to think about philosophical questions, I could not possibly be doing something deep. This prejudice, that all good work must be technical in the mathematical sense, has made economists, sociologists, etc., feel inferior, as if they should mathematicize, very often to the detriment of the real *meaning* of their work. (1975, 515; italics in the original)

The success of reflexive thinking further influenced the images of knowledge by reinforcing the traditionally well-established authority of the great mathematical masters to decide all-embracing questions about mathematics. The issue of authority in mathematics presents a paradox.

On the one hand, more than in any other exact science or scholarly field, an isolated result in mathematics may be attained and corroborated without resorting to the authority of individuals or of written sources. Results may often be obtained through ingenuity and inspiration, and a thorough knowledge of sources and of the writings of the great masters is by no means a necessary condition for innovation (as it is in the humanities, for example). Proof remains the only validating procedure, and its standards are established through shared images of knowledge.

In contrast, in fields where new results may invalidate previous ones, the authority of scientists may be jeopardized as research develops in new directions. Consequently, these scientists may have good reasons (scientific, institutional, or political reasons) to refuse to accept new results. This kind of conflict of interests, cannot arise within the body of knowledge of mathematics. There are no polemics, in principle, within the body of knowledge of mathematics. Proof, based on its accepted standards, has the last (and only) word. Authority of any kind is explicitly proscribed as a criterion for acceptance of any claim within the body of knowledge.

On the other hand, it is a plain fact that the images of knowledge are constantly being debated, and these debates must be settled. Since there are no accepted criteria for settling debates about the images of mathematics, more often than not these debates are settled by relying on the authority of mathematical masters.

A good illustration is furnished by a "leading figure," the Bourbaki group, whose conceptions have to a large extent dominated the images of contemporary mathematical knowledge. Bourbaki insisted that the axiomatic approach adopted in their books exempted them from philosophical debate; and he claimed that the group's attitude toward foundational problems (problems that, to Bourbaki, subsume all philosophical questions) "can be best described as total indifference." Confronting criticism directed at the overall conception of Bourbaki's work, Jean Dieudonné, one of the most outspoken members of the group, replied:

> No one can understand or criticize the choices made by Bourbaki unless he has a solid and extended background in many mathematical theories, both classical and more recent.

If, however, somebody decides anyway to put forward some kind of criticism, then

> [Bourbaki] has no means to prevent such a misguided behavior, for it would imply engaging in polemics, and as I told you that is something he has steadfastly refused to do. (Dieudonné 1982, 623)

These two apparently contradictory facets of the role of authority in mathematics are, in fact, closely connected. The lack of polemics within the body of knowledge is taken to be also the desired state for the images of knowledge; but since proof cannot always be used here, as in the body of knowledge, authority often becomes decisive in debates. The authority of the masters cannot be questioned, since they know the details better than anybody else. They are the best equipped to speak about the body of knowledge, so they are thought to be the best equipped to dictate the images of knowledge. In some special cases it is convenient to support those images through the ultimate judgment: that of mathematical proof. However, this is not always possible. Therefore, as a consequence of the success of reflexive thinking and its associated images of knowledge underrating the value of mathematical claims other than those supported by proof, authority often determines which images of knowledge will be accepted.

There is some justification for the tendency to arbitrate debates pertaining to the images of knowledge through the criteria of mathematical authority, particularly because specialization has brought about such a diversity of flourishing fields in mathematics as to make an overall appreciation of mathematics extremely difficult for the layman, and even for most mathematicians. But problems arise when there is no room left for any other kind of considerations and authority becomes the *only* criterion for the passing of judgment.

6. Images of Knowledge and the History of Mathematics

The historiography of mathematics has been greatly affected by the dominant images of knowledge in contemporary mathematics – in particular, by the idea that all meaningful debate about the body of mathematics should be done only through mathematical argumentation and, in cases where this is not possible, by relying on the undisputed authority of the masters. These images of knowledge suggest that the history of mathematics should be written only by mathematicians (or if written by somebody else then judged only by mathematicians) and that historical research about mathematics is meaningful only insofar as it throws new light on present mathematical research through a strictly mathematical analysis of past mathematics.[25] The general acceptance of this view accounts to a large extent for the dominance of the positivistic approach in the historiography of mathematics. This may be illustrated by the following quotation from an enthusiastic review of Bourbaki 1960 – a classic among history books on contemporary mathematics:

> This fascinating volume assembles the historical notes from Bourbaki's various [books]. . . . These elements of history are just . . . former mathematics as it seems now to Bourbaki, and not as it seemed to its practitioners then. In the terminology of historiography, it is "Whig history". But for the mathematician, the various chapters are full of interesting insights. (MacLane 1986)

This is, to be sure, neither the most emphatic defense of Bourbaki's historiographical approach nor the harshest criticism of the alternative one. I quote it in this context, however, because it expresses the heart of the matter. Mathematicians will certainly endorse Bourbaki's historiographical approach because of the interesting *mathematical* insights it affords, even when it is done at the expense of a faithful historical interpretation. As a matter of fact, after mentioning some of the interesting insights, the reviewer is compelled to note that "on the way to these insights, there may be some minor annoyances." His list of "annoyances" includes some very typical instances of Whig history. For instance:

> The Gibbs-Wilson vector analysis (1900), which has dominated notation in Anglo-Saxon Physics for 85 years, is dismissed as a vulgarization of the ideas of Hamilton and Grassman – despite the fact that Gibbs understood the tensor product of vector spaces well before Bourbaki. (MacLane 1986)

Preference is granted to the interesting insights – which may be natural when mathematical research is the main interest. This preference, however, is often taken as historically right as well, and any historiographical research not willing to accept the "minor annoyances" is disparaged as uninteresting, if not downright nonsense.

One example of the tension between mathematicians and historians of mathematics[26] is the debate about the so-called "geometrical algebra" of the ancient Greeks. Traditional views about the issue are based mainly on the approach described above; the geometry of the Greeks is understood as a series of algebraic problems whose real algebraic identity they concealed for some unexplained reason in geometric language. In order to understand the Greeks we must, according to this view, use

[25] This position is articulated in Weil 1980. See also Askey 1988. For a different opinion, see Grabiner 1975 and May 1975, each of which could have been entitled "A Historian of Mathematics' Apology." Their main point is that there is room for the independent historical research of mathematics, which is at least as important as research carried out by mathematicians themselves. This issue is similarly viewed in Lorenzo 1977, 33.

[26] In a discussion similar to the present one, Grabiner (1975, 444) adds the following footnote: "By 'historian' and 'mathematician' I do not mean a classification according to the field of a person's Ph.D., but according to his point of view. . . . In general mathematicians and historians have, while writing the history of mathematics, in fact taken the different approaches I describe, though there is no a priori reason they would necessarily have to do so." I adopt Grabiner's formulation but would add that I hope in the present article I have explained satisfactorily the (a posteriori) reasons for this state of affairs.

mathematical language and ideas that developed much later, namely those of symbolic algebra as developed since Viète and Descartes.

This historiographical point of view has been thoroughly criticized by Sabetai Unguru (1975);[27] but the historical evidence he advanced in support of his claim that algebraic thinking was alien to the Greeks was simply ignored as irrelevant. Strong reactions to Unguru's criticism were advanced by three leading mathematicians who have also written on history of mathematics – see Freudenthal 1976, van der Waerden 1976, and Weil 1978. The disagreement between Unguru and the three mathematicians may be characterized in the terms used above. The mathematicians' arguments were mathematical; the mathematical equivalence between specific points of Greek geometry and the symbolic manipulation of algebraic formulae was taken as the key to a proper historical understanding. Unguru, on the other hand, introduced extra-mathematical considerations into his argument in order to conduct the debate on historical rather than purely mathematical grounds. His outlook met with bitter criticism, and he was accused of using irrelevant arguments to arrive at nonsensical conclusions.[28] The force of the arguments against Unguru resides solely in the authority of the mathematicians involved in the debate.

In this paper I have presented a picture of mathematical knowledge that also suggests a program for historical research. According to this view, the aim of the historian of mathematics is to detect and describe changes in the images of mathematics in a given context, and explain the growth of the body of knowledge and the interaction of the two layers. The best test of this suggested program will be its use in detailed case studies in the history of mathematics. It is in such research that the value of this approach will be determined.

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[27] See also Unguru & Rowe 1981, 1982, for further arguments based on mathematical grounds.

[28] It is pertinent to add that in Unguru and Rowe 1981, 1982, interesting *mathematical* arguments were added to the historical ones, thus reinforcing the strength of the latter. These purely mathematical arguments, however, were not mentioned, let alone refuted, by any of the critics of Unguru 1975.

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