Science in Context **17**(1/2), 1–21 (2004). Copyright © Cambridge University Press
DOI: 10.1017/S0269889704000031 Printed in the United Kingdom

Introduction: The History of Modern Mathematics – Writing and Rewriting

Leo Corry

Tel-Aviv University

The present issue of *Science in Context* comprises a collection of articles dealing with various, specific aspects of the history of mathematics during the last third of the nineteenth century and the first half of the twentieth. Like the September issue of 2003 of this journal (Vol. 16, no. 3), which was devoted to the history of ancient mathematics, this collection originated in the aftermath of a meeting held in Tel-Aviv and Jerusalem in May 2001, under the title: "History of Mathematics in the Last 25 Years: New Departures, New Questions, New Ideas." Taken together, these two topical issues are meant as a token of appreciation for the work of Sabetai Unguru and his achievements in the history of mathematics.

In the introduction to our first collection, guest editor Reviel Netz described the task of rewriting the history of early mathematics as "a necessary – but risky – enterprise." This necessity, according to him, stems from reasons of both historiographical and philosophical import. To state it briefly, the historiographical reasons pertain to the scarcity of substantial evidence that implies the need for much interpretive additions from the historian. This being the case, shifts in the historical perspective may bring about significant changes in the historical accounts produced, thus sensibly enriching our historical understanding. The philosophical reasons pertain to the fact that a contextually sensitive account of the history of mathematics raises important questions about the nature of objectivity and rationality in science. In Netz's words: "The task before us – Unguru's historiographical descendants – is to show how a historicized mathematics need refine, but not destroy, our sense of rationality and truth." Herein lies also, implicitly, the risky element of the enterprise: historicizing rationality has often been perceived as potentially calling into question its very essence. Although historicizing rationality has been undertaken in many ways and from many perspectives, it seems that when this is done in relation with one of the quintessential embodiments of rationality, mathematics, some particularly sensitive chords are sounded, and reactions are often colored with very sharp tones.

It may sound strange to the standard reader of a journal like *Science in Context* that a guest editor feels compelled to provide, somewhat apologetically, a justification for the very need of his professional undertaking. It is true, as Netz stresses in his introduction, that the continued, and currently thriving, activity of historians of ancient mathematics is not based on the discovery and publication of new textual evidence, but rather on the continued reinterpretation of evidence that has already been thoroughly analyzed by generations of historians, philologists, and mathematicians. This alone might be taken as a major reason behind an apparent need for self-justification. On the other hand, a similar assessment may apply to the history of most other fields of ancient science, not only mathematics, and indeed to ancient history in general.

It seems to me that in his eloquent introduction, Netz was implicitly addressing a major problem that underlies the professional activity of historians of both ancient and more recent mathematics, a problem that requires some analysis and elaboration. This problem has to do with the rather exceptional relationship between the historian of mathematics and its readership, as compared with that of historians investigating the development of other scientific domains. The 2001 conference from which the present collection of articles derived was defined as an attempt to "take stock of new developments in the history of mathematics in the last quarter century," a period when "from being an appendage to the study and, primarily, teaching of mathematics, the new history of mathematics has become an independent *historical* discipline." Even if our discipline at large has indeed achieved this goal, there can be no doubt that the relative extent to which the works of historians of mathematics are read by, quoted by, and absorbed into, that of historians of science specializing in other fields (not to mention general historians) is exceptionally low. More than in any other related field, the works of historians of mathematics can be more naturally read by mathematicians than by fellow historians of science. This is not to say that such works *are indeed* widely read by mathematicians. In general they are not. Rather what I mean is that, compared to other fields in the history of science, the texts produced by historians of mathematics seem to require readers with a considerable degree of technical skill in order to be properly understood, and sometimes even in order to begin to be read at all.

As a result of this situation, the historian of mathematics often works with a feeling of professional solipsism in a continuous search for the correct academic self-definition as an individual researcher or for the discipline at large. Of course, all historians, and particularly all historians of science, must ask themselves to what audience their texts are directed, and then write them accordingly; but I dare to assert (without proof, other than inductive generalization) that in general, historians of mathematics feel more strongly pressed to answer this question than their colleagues in neighboring disciplines, and that matters related to this question have been a major issue underlying the historiographical discourse of the discipline (more often than not in a tacit way) over the last twenty-five years. Should one write for an audience of historically-minded and historically-informed mathematicians or for an audience of mathematically-informed historians of science? Or perhaps more sharply: how can one make the history of mathematics relevant, appealing, and accessible to mathematicians? To historians of science? To philosophers of science and, in particular, of mathematics? To philosophers in general? To sociologists of science? Practitioners in all these disciplines may surely have their own kinds of answers to these questions, and such answers no doubt vary across disciplines, and possibly even within any given discipline. But from the point of view of those engaged in investigating the history of mathematics as their main field of scholarship, there seems to be a general feeling that fellow researchers in related fields could learn much from the work of historians of mathematics, if they only took the necessary effort to read it carefully. Thus, historians of mathematics have often followed a variety of paths in order to have their works reach audiences wider than their own more narrowly defined specialty. An insightful way to gain an overview of research in the field during the last twenty-five years is to look at different ways in which different historians of mathematics have chosen to build bridges to neighboring disciplines – not at the declarative level, that is, but rather through the way they actually do research and publish it.

Before elaborating on these ideas by giving concrete examples, however, it is important to stress that even within the discipline itself there are important differences among historians dealing with different periods, and questions arise pertaining to professional dialogue across different periods no less sharply than across neighboring disciplines. In fact, one major meta-issue underlying all the discussions in the Tel-Aviv/Jerusalem 2001 symposium was the ability of the attending historians of ancient and modern mathematics to mutually interact, and, in spite of the ambiance of collegiality and good will, the outcome, though intriguing, was not always straightforward. Incidentally, for purely contingent reasons, this particular meeting had no representatives of additional, important sub-specialties such as seventeenth-century mathematics or Islamic mathematics, which would have no doubt intensified the contrast and affinities across historical periods even more poignantly. It is obvious that some basic historiographical concerns that may affect research in any one of these periods may be irrelevant for others. Thus for instance, Reviel Netz speaks about the scarcity of sources characteristic of the history of ancient mathematics. The situation for modern mathematics is the opposite, at least in what concerns published material, and this becomes a sharper problem the more one advances into the twentieth century: How should one choose from the almost unlimited amount of sources available to us? There is also the issue of the relationship between the major milestones and breakthroughs as opposed to "normal science," so to speak. Netz writes:

> Our knowledge of early mathematics is bottom-light, top-heavy: we have rich and important single monuments in the works of Archimedes, Apollonius, and Euclid in the west (or *The Nine Chapters* in the east), but relatively little knowledge of the wider context within which those monuments took shape. Just what would an ordinary piece of early mathematics look like? What was the process leading to the major monuments? On this, relatively little is known. We are accustomed, as historians, to look for origins and for lines of development. But with early mathematics, we have available to us mostly the (remarkable) snapshots of science in its full form. (Netz 2003, 275)

In modern mathematics we also have such monuments, of course, though they are more abundant and sometimes compete with each other for glory. But as for the wider context and for coming to learn about "ordinary pieces of mathematics" and "processes leading to the major monuments" – although many times one would like to be able to gather ever new archival material – quite often the problem, rather than scarcity, is an *embarras de richesse* that needs continually to be dealt with by means of ever new editions of complete works, *Nachlässe*, notebooks, archives, letters, etc.

In the not-too-distant past, the history of mathematics was mostly written by mathematicians, mostly for mathematicians, and mostly in the purest tradition of Whig history, or, as Ivor Grattan-Guinness has perceptively described it: "the royal road to me" (Grattan-Guinness 1990). This approach attained its most significant instantiation in the historical writing of the Bourbaki group and of Jean Dieudonné (when he undertook it individually) (e.g., Bourbaki 1969; Dieudonné 1978; cf. also Kline 1972). Concomitant with this mainstream trend in the writing of the history of mathematics there arose an early version of the "historians of mathematics' apology" (Grabiner 1975; May 1975) in which arguments were put forward to justify the very right to dare writing history by someone who was not himself a research mathematician (preferably a prominent one, and typically a retired one looking back at the long way traversed). More than twenty-five years ago Unguru asserted that rewriting the history of Greek mathematics would necessitate, first of all, an adequate reading and understanding of the relevant ideas *in the proper historical context* in which they arose, developed, and were spread, rather than a retranslation of them into contemporary notions and conceptions (Unguru 1975). Much less should one assess their historical importance according to their relevance to modern concerns. Evident as such principles would appear to any historian, they were at the time received with great hostility and even rage by some leading mathematicians with an interest in history (Freudenthal 1977; van der Waerden 1976; Weil 1978).

Over the years, historical work on both ancient and more recent mathematics that followed a historiographical approach akin to the one postulated by Unguru, continued to appear and to gradually create a corpus that came to justify – in the form of actual writing rather than as meta-arguments calling for action – the effectiveness of such approaches. Historians of mathematics who were not themselves practicing mathematicians felt less intensely the need to argue for their right to indulge in their kind of professional activities and practitioners of the discipline became more visible, either in history of science departments or, in some cases, even in mathematics departments. Books series and specialized journals were solidly established, such as *Historia Mathematica* and *Revue d'histoire des mathématiques*. Professional meetings dealing with the various sub-specialties in the discipline became a matter of course. Bourbakian historiography has come under sustained and effective criticism, and Whig-like history of mathematics is less and less frequent, and certainly less and less argued for.

Historians actively engaged in research of topics connected to more recent mathematical fields are, or have been, faced – like their colleagues dealing with ancient mathematics – with the need to rewrite parts of existing accounts. Unlike ancient mathematics, however, much of the very writing of the history of modern mathematics has yet to be undertaken from scratch, to begin with, and this is currently being done in many directions and from many different and illuminating perspectives. One residue of the apologetic tone that characterized some of this historical writing has remained, however, though it has become minor and it typically remains tacit, and has shifted into a new direction with fruitful consequences. The apology, or rather the concern, is related to the necessity for being more distinctly relevant (and, thus, more intensively read) by colleagues in the neighboring disciplines, mainly mathematics, philosophy of mathematics, and history of science in general. Thus, in many recent publications and workshops one can clearly notice implicit or explicit calls for a historiography of mathematics that will adequately meet this concern.[1] This pursuit is also noticeable in the articles comprising the present issue of *Science in Context*.

The history of mathematics is by now an intellectual enterprise with a long history of its own, going back to as early as the fourth century B.C., and that developed within various cultures. Still, the changes hinted at in the foregoing paragraphs have brought about an unprecedented pace of activity in the field, and much original work, over the last thirty years (Dauben and Scriba 2002). Much work continues to be written for an audience of mathematically well-informed readers or, indeed, for mathematicians themselves. In fact some mathematicians continue to write texts and to edit collections on the history of mathematics (e.g., Pier 1994; idem 2000) that basically ignore, not only the fact that historical writing may have some kind of autonomous standards to be paid attention to, but also the fact – less perspicuous to them perhaps – that a considerable corpus of serious historical writing has actually accumulated with time, and that at least part of it deserves to be read and quoted by anyone undertaking the role of the historian.

But even in collections like Pier's, we find interesting indications of varying historiographical conceptions among mathematicians approaching the histories of their own topics. Thus for instance, Gaetano Fichera opened his article in that collection on the birth of functional analysis, by refuting a claim of Dieudonné (who also has an article in the collection – Dieudonné 1994) according to which the importance usually attributed to the contributions of Vito Volterra to this branch of mathematics has been exaggerated. While arguing not only against this specific claim, but also against the more general approach underlying it – of which Dieudonné is perhaps the most prominent representative – Fichera supported a historiographical point of view that was totally opposed to the entire, very long book where his very article was published. He thus contrasted the idea of *Revisitation* with that of *History*, and wrote:

> Revisitation consists in re-examining with today's mentality, with the experience and complex of new acquisitions that the passage of time has made available to us, those themes which belong to a past historical period. This approach corresponds more to an *historical review* rather than to *history* itself. *History*, instead, is something more subtle

[1] For one recent, explicit example, see http://www.us.es/ghum717.

> and more difficult. In fact, the true historian must make the effort to shed himself of today's way of thinking and of all the experience he has acquired in the time interval between the moment in which he is working and the epoch he is examining. He must try to reconstruct the typical mentality of the period to which his historical research is directed, discarding all the superstructures which have arisen during the passage of the years. Only in this way can he give an unbiased judgment of a given historical figure and, in the case of a scientist, correctly evaluate his work. (Fichera 1994, 172; emphasis in original)

The typical reader of *Science in Context* will consider this passage to be stating the obvious, and indeed to be stating it in a rather naïve formulation, as compared to the more sophisticated ones that historians of science have been developing over the years. I have not quoted it here as a brilliant formulation of innovative historiographical principles, but rather as a piece of historical evidence. Passages that express similar concerns, when written by Unguru twenty-five years ago in a much more coherent and historiographically informed fashion, attracted furious reactions from mathematicians like van der Waerden and Weil.

It is also important to stress that, while revisitation as described here is a notion applicable in principle to any intellectual field, it has a very particular meaning in mathematics. Indeed, it may frequently happen that an active mathematician trying to explore the scope and implication of a recently developed mathematical idea or technique will do that by way of revisitation, namely, by re-examining old problems or results with the perspectives afforded by the new tools. The problem arises, of course, when this mathematically legitimate and interesting exercise is interpreted as legitimate historiography. Even in the case of Fichera's own article, for instance, or any other article written by a historian who would endorse this view, one may ask if it actually stands up to his own historiographical requirements. Like his fellow contributors in the Pier collection also Fichera makes little or no reference to secondary literature or to any but a few well-known original mathematical articles in the field whose history he is telling us. Still, once the historiographical principle has been formulated and accepted, the entire game is fundamentally different from the one implied by Dieudonné's historiography and by that implied in most other contributions to Pier's collections.

Fichera, at any rate, is not the only mathematician aware of this historiographical dilemma and we do find indeed recent historical accounts that, while dealing with highly technical matters that by their very nature will remain addressed only to mathematicians competent in a given field, they do pay close attention to the achievements of historians over the last twenty-five years and to the standards they have set (e.g., James 1999).

A most visible and interesting example of the kind of changes experienced in the discipline over this period of time is reflected in the historical study of mathematical analysis. Since the nineteenth century, analysis has been the pillar of mainstream mathematical training and the gateway of every student into the entire discipline. To a large extent the history of that same field has also been a pillar of recent historiography and, together with geometry, the gateway to historical studies of mathematics. Thus, one way to look at the evolution of the entire discipline of history of mathematics and at the concomitant views successively associated with it, is to compare three textbooks on the history of analysis that have been published along the years. Being textbooks directed mainly at an audience of students of mathematics, they do not represent foremost examples of the cutting-edge of historical research and the most sophisticated way to approach it, but they certainly reflect some of the main, accepted trends, of the available, relevant literature, and of the general spirit reigning in at least one important sub-field of the discipline at a given point in time.

For many years Carl Boyer's classical *History of the Calculus and Its Conceptual Development*, originally published in 1939, provided an overview of the relevant developments from the Greeks to the late nineteenth century, thus constituting an adequate textbook that summarized the state of the art (Boyer 1939; idem 1959). Retrospectively seen, it contained many of the limitations that Unguru's critique addressed, though – it must be clearly stressed – much less than the typical history of Greek mathematics available at the time. A collection edited by Ivor Grattan-Guinness in 1980 brought together articles by specialists in various historical periods covering the evolutions in techniques and foundations of analysis between 1630 and 1910, and written according to highly rigorous historiographical standards (Grattan-Guinness 1980). Boyer's book, incidentally, was mentioned in this collection only in passing, whereas the relevant secondary sources contained an impressive number of works that had appeared only recently, and that had considerably helped clarify the historical significance of each of the individual topics discussed. A new collection appeared in 1999, edited by Hans Niels Jahnke, and recently translated into English (Jahnke 2003). Like the 1980 collection this one builds on separate articles written by leading specialists in the various periods and sub-disciplines discussed. Although one might argue about the relative scholarly quality of certain individual articles in the newer collection as compared to the earlier one, an overall comparison between the two essentially reflects the important achievements of the historical research conducted in each of the areas discussed over the intervening twenty years, and that is manifest in terms of historical detail, depth, and sophistication, and in the enormous amount of detailed historical research that can be found in the bibliography.

The introduction to the Jahnke collection explicitly stresses that it is aimed at a broad audience and it does not fail to adopt the typical formulation according to which the mathematical examples discussed in the various chapters "can be understood by any reader with a college background and certain openness to mathematical argumentation." Unfortunately, in this as in other cases, this would seem to be more wishful thinking than a realistic description of the audience that will actually read the book. As with many other works of this kind, one cannot but end up regretting the very limited audience that is likely to actually come to enjoy the great effort put into the production of a fine collection like this one. At any rate, this sequence of three books on the history of analysis – and the important, gradual contributions to research on which the differences among them were based – provides an outstanding illustration of historical rewriting at its best, and at the same time of the fundamental plight of the discipline. In this case, the contents and style of Jahnke's book, as well as the venue chosen for publication (a historical series published by the American Mathematical Society and the London Mathematical Society) bespeak the clear awareness that neither historians of science in general nor philosophers of mathematics will be its main readership, and the concomitant hope that, besides the relatively limited circle of fellow historians of mathematics who will be its natural readers, at least some mathematicians will also read this collection.

If the history of analysis provides the paradigmatic example of rewriting in the history of modern mathematics and helps chart important changes in the historiography, a prominent example of writing the history of a mathematical field from scratch and in depth, but still with a mainly mathematical audience in mind, is provided by Thomas Hawkins' account of the development of the theory of Lie groups (Hawkins 2000). Hawkins' book is the result of more than twenty years of focused effort, partial results of which he had published along the years. It presents an imposing example of a historian who displays full technical command of a very broad range of deep mathematical issues, combined with a fine historical sensibility that helps situate the original ideas in their proper context. Still, Hawkins seems to be aware of the definitely mathematical orientation of any potential reader of his book, and he thus constantly stresses the links between the historical issues and the kind of concepts and results that a contemporary, practicing mathematician would be interested in hearing about (perhaps even as a way to a possible "revisitation," to use the term suggested above).

A prominent figure in Hawkins' story is Hermann Weyl, one of the most original, poly-faceted and influential mathematicians of the early twentieth century. The significance of his works in fields other than Lie groups (i.e., foundations of analysis, Riemannian manifolds, or relativity theory), has also been discussed at large in recent historiography, and a leading role in such account can be attributed to Erhard Scholz (Scholz 2001). In the present issue of *Science in Context*, Scholz adds a new contribution to this important topic by discussing the evolution of Weyl's views on the problem of space. By its very nature any discussion of Weyl's work implies the need to discuss intricate technical topics in various fields of mathematics and physics, and this is also the case with Scholz's contribution here. On the other hand, Scholz focuses here on a classical, central problem of contemporary philosophical debate on the nature of space and thus opens his horizon of potential readers in the direction of technically informed philosophers.

The establishment of the general theory of relativity after 1916 as the new mainstream perspective for discussing and understanding the nature of physical space and its interrelation with geometry was followed by important theoretical developments at the borderline between physics and mathematics, of which Weyl was perhaps one of its more intriguing contributors. Equipped with masterful technical abilities and an astoundingly broad background in the various fields of physics and mathematics, Weyl was exceptional among mathematicians of his generation for his true engagement with philosophical issues. This engagement went well beyond plain philosophical knowledge for the sake of *Bildung*, and implied a true preoccupation with the implications of each philosophical approach he became involved with.

In this article Scholz addresses Weyl's 1929 adaptation of his own idea of a gauge measurement of lengths into a modified version that would allow the incorporation of recent developments in quantum physics, particularly those associated with the Dirac relativistic equation for describing the motion of an electron in an electrical field. Scholz explains how the approach followed by Weyl in this question used a modified version of the same conceptual frame which he had developed in the early 1920s in his works on the "mathematical analysis of the problem of space." From within the complex technical issues involved in these two important undertakings of Weyl, Scholz brings to light interesting analogies and interconnections between the methodological and ontological issues underlying them. He shows in detail how and why, by modifying his earlier notion of gauge with a specific, technical purpose in mind related to the Dirac field, Weyl was also modifying his previously a-prioristic conceptions of space and geometry in favor of new ones, with decidedly more empiricist leanings.

As in most of his previous works, Scholz's head-on and exacting analysis puts great demands on the reader and establishes its dialogue with the neighboring disciplines by a straightforward invitation to delve into the intricacies of Weyl's rich mind. The kind of dialogue that Scholz's article establishes with philosophers of mathematics, and of science in general, is not an isolated example, but part of a broader trend. At the focus of this trend we find an attempt to revisit the philosophical discussions around the works and ideas of thinkers like Frege, Russell, or Hilbert, while paying closer attention to their precise historical context and their changes through time (e.g., Beaney 1996; Peckhaus 1990), rather than to an idealized, disembodied account of what they might be.

An early way in which historians of mathematics over the last twenty-five years had attempted to establish bridges with general historians and philosophers of science was based on adapted uses of concepts then in vogue, such as paradigms, revolutions, research, and programs (Gillies 1992; Lorenzo 1977). Far-reaching programs in the spirit of the sociology of knowledge were also postulated (Bloor 1976). Notoriously, in spite of the great effort spent in historiographical debates, very little came out of this in terms of valuable historical studies and scholarship[2] (a claim that is true not only concerning mathematics, but also the history of science in general). Imre

[2] Except, perhaps, for isolated examples, such as Mackenzie 1999.

Lakatos' Popperian-oriented, highly suggestive, book, *Proof and Refutations* (Lakatos 1976), presented a very concrete and, in many respects, convincing study of what a "rational reconstruction" could offer to historians of mathematics, but again, in spite of an initial enthusiasm and possibly an indirect influence, no real historiographical tradition came out of it (cf. Corry 1993).

Of the attempted bridges between history of mathematics and more general concerns pertaining to cultural history, one that deserves special attention is the one related to the question about the extent to which the "modern" aspect of "modern mathematics" has some "modern essence" in common with an underlying modernity of other aspects of contemporary culture. The most daring attempt in this direction was undertaken by Herbert Mehrtens in *Moderne-Sprache-Mathematik* (Mehrtens 1990), a book that has been received with a mixture of enthusiasm and criticism, but that has clearly indicated that, to the extent that this question can be seriously addressed by historians, there is an enormous amount of work to be done about how to do so. Different ways of discussing social and political aspects of the creation and dissemination of mathematical knowledge appear in other noteworthy books, such as Catherine Goldstein's account of the contemporary readership of Fermat (Goldstein 1995), Eric Brian's account of the participation of French mathematicians in government issues in the eighteenth century (Brian 1994), or Reinhard Siegmund-Schultze's detailed account of German mathematicians' emigration processes after the Nazi seize of power (Siegmund-Schultze 1998), to mention just a few.

It seems to me, however, that the single most visible source for the continued enrichment of the historiography of modern mathematics over the years has been the sustained addition of ever new monographic studies on the development of several, central mathematical disciplines and trends, or, to a lesser degree, on the careers of distinguished mathematicians. Many among such studies have achieved important results by following a rather straightforward kind of scholarship, not unlike that predicated by Unguru, based on a detailed examination of mathematical sources, both published and unpublished, while stressing the proper context in which the ideas were originally produced and disseminated. Well-known examples include Hans Wussing's now-classical account of the rise of the concept of group (Wussing 1969), Michael Crowe on vector spaces (Crowe 1967), Jeremy Gray on non-Euclidean geometries (Gray [1979] 1989), Erhard Scholz on the concept of manifold (Scholz 1980), Jesper Lützen on the theory of distributions and on the work of Joseph L. Liouvulle (Lützen 1982; idem 1990), Umberto Bottazzini on real and complex analysis (Bottazzinni 1986), and many others that would require more space to list in detail.

It is important to stress that many of these studies follow their own interpretive schemes that provide coherence to their accounts and help to make sense of the enormous amount of evidence available. Still, the schemes involved are in general used in an open-ended and flexible way that is not meant to assume the kind of universal applicability suggested by Kuhnian or Lakatosian perspectives. At the same time, however, these flexible schemes, where they have appeared, have also helped provide useful, general insights about the history of mathematics, with suggestive powers that manifest themselves later on, directly or indirectly in the works of other historians working in other fields. Thus, for instance, Scholz's book on symmetry and duality in the nineteenth century (Scholz 1989) discusses two interesting cases of the interrelation between mathematics and its applications: a very successful one (crystallography) and an essentially failed one (the theoretization of statics using methods of projective geometry). This discussion leads to a suggestion with general historiographical implications, that the traditional separation between pure and applied mathematics should be substituted, or at least complemented, by a different one, namely, that between autonomous and heteronomous mathematics. In my own work, I have used the idea of images of scientific knowledge as a basis for describing the rise of structural thinking in modern algebra (Corry [1996] 2003) and this approach has subsequently also been successfully adopted in other, similar investigations (Bottazzini and Dahan 2001).

A recent, successful example of the kind of straightforward, yet interpretive historiography I am referring to here appears in José Ferreirós' very detailed, and mathematically as well as philosophically informed, book on the origins and early development of the theory of sets and on the gradual adoption of the set-theoretical perspective at the turn of the twentieth century in mathematics at large (Ferreirós 1999). In the present issue of *Science in Context*, Ferreirós illuminatingly complements his own account by examining a thought-provoking and rather surprising new perspective on the intellectual background, and the underlying motives of Georg Cantor's involvement with the incipient theory of sets in the last third of the nineteenth century. By the very essence of the topic, this article interestingly opens the way for audiences, other than that of historians of mathematics properly said, to enter the field through one of its more abstract and abstruse theories.

Based on an analysis of somewhat neglected sources, and a general account of the intellectual atmosphere within which Cantor received his academic education in Germany, Ferreirós draws a portrait of Cantor as a characteristic figure of late Romanticism whose research program was led by the search of "an inside view" of nature based on an organic connection between science, philosophy, and theology. Ferreirós stresses, and convincingly documents, the remarkable fact that – in contrast to mathematicians who quickly adopted Cantor's ideas on infinity and usefully applied them to solve foundational problems in the central domains of mathematical analysis such as the theory of analytic functions – Cantor himself never seriously pursued that direction. On the other hand, by looking at Cantor's speculative texts on science and metaphysics, which have usually been considered as oddities related to the mathematician's notorious mental insanity, Ferreirós is able to come forward with elaborate explanations for the general orientation of Cantor's work, and make sense of an overall picture that the traditional, mainly intra-mathematical, perspective on his work has left partially obscure.

Also Jeremy Gray takes a fresh view on a topic widely discussed in the existing historiography of nineteenth-century mathematics, and brings it to a territory that directly concerns broader audiences, rather than just historians of mathematics. This period in the history of mathematics has always been analyzed from the perspective of relentless progress that may be characterized in terms of unprecedented growth in the activity of individuals, centers of research, mathematical institutions and journals, in terms of ground-breaking achievements, and in terms of penetrating overall changes underwent by the discipline. Without downplaying the centrality of such characterizations, Gray suggests that parallel to them a completely different, and usually overlooked, aspect of the development of the discipline was a growing sense of anxiety on the side of mathematicians, rooted in the gradual recognition of the pervasiveness of error, failure, and uncertainty in mathematics.

In his analysis of this kind of anxiety, Gray refers back to well-known, mainstream developments and episodes of nineteenth-century mathematics: research on the foundations of analysis as well in certain specific problems of the theory of integration, non-Euclidean geometry, the discovery of the paradoxes of set theory. He also hints at the varying conceptions about the reliance on proof as the basis for the certainty of mathematical truth. Finally, he also points out plain mistakes that passed unnoticed for long periods of time. Gray refers to many examples that are far from being unknown, but in many cases he also calls our attention to lesser known examples that help to clarify his main tenets. And yet, his main point is that while historians have often found ways to retrospectively downplay the centrality of such difficulties, presenting them as marginal and as incapable of affecting a putative, generalized feeling that mathematics was just moving from one great success to the next, Gray presents weighty and consistent evidence to show that contemporary mathematicians recognized the presence of these difficulties in their day-to-day activity, continually discussed their status, and indeed expressed their distinct feeling of anxiety and disquiet because of them. Moreover, Gray stresses, this feeling reached a peak toward the end of the century, precisely as the new achievements in research on the foundations of analysis became widely known and accepted, and decades of research into the foundations of geometry were leading to acceptable views on the nature of the non-Euclidean geometries that had emerged early in the century, creating confusion and doubt where previously certainty and apparent clarity had reigned.

Interpretive schemes used as the basis for the analysis of individual episodes in the history of mathematics are often enhanced by the addition of conceptual tools taken from neighboring disciplines, on the basis of which the complex historical processes investigated can be further clarified. The example of "images of science" was already mentioned above. Among the most frequently used conceptual tools that historians of mathematics use inspired by current practice in the history of science are those that address the internal dynamics of a mathematical community and the way that this dynamics shapes the local, as opposed to universal, aspects of the mathematics that this community produces. A known example is the notion of a "school" whose use as an analytical category in the history of science has been widespread (Geison 1981).[3] In fact, the notion of a mathematical school has been the object of separate discussions and attempts to elucidate its meaning and possible use as an analytical tool with potentially broad applications by historians of mathematics. An interesting case in point can be found in a recent article by Karen Parshall. After referring to various examples where the term is used in various, loosely defined yet tacitly understood, senses, Parshall suggests that a more analytically elaborate sense of it should be sought so that it might be used to investigate new historical contexts. Such an elaborate definition would keep an eye on what the history of science has already done in this regard:

> Historians of science also use the word school, but, for them, a school is almost exclusively something associated with the laboratory sciences, so mathematics falls outside their purview. Still, historians of science – largely unlike writers on the history of mathematics – have at least tried to provide a definition of school as an analytical construct for evaluating and understanding the past. A consideration of some of their definitions sheds light on how these definitions might be adapted to the mathematical context. (Parshall 2004, 10)

A brief analysis of how historians of science have used the term in various contexts leads Parshall to suggest some features that in her opinion characterize mathematical schools. Prominent among these features is the existence of a charismatic leader, who should be a distinguished researcher to begin with. Further, this leader should be involved in the intensive pursuit of a certain idea or general approach that his or her students should also then engage as part of their identification with the school, and one of these students might eventually succeed the master and take over the role of leader. As any other definition of this kind, Parshall's is open to debate and modification, and in particular it raises the question to what extent the Chicago school of algebra, at the focus of her own enquiry, fits well, or perhaps too well, the scheme that she proposes.

Be that as it may, Parshall's proposal is representative of a more general trend noticeable in recent historiography that stresses the importance of studying the ways in which mathematical knowledge is produced and propagated, and the way that charismatic individuals, institutions, and related mechanisms, are variously responsible for these processes. Some historians believe that the notion of school puts excessive stress on one way of looking at the possible role of the leader, and thus inadequately translates from other contexts a notion that fails to describe the variety of fashions in which personal interactions play a central role in the history of mathematics. Among the alternative, related notions that are suggested for a more adequate account, there appear "mathematical traditions," "mathematical culture," or "networks" (Rowe 2003).

[3] On Eastern-European approaches to the evolutionary patterns of schools in the natural and social sciences as well as in mathematics, see Mikulinskiy and Jaroševskiy 1977 and 1979, published in parallel in Russian and German.

A recent, very illuminating study that can be associated with such a historiographical perspective appears in Andrew Warwick's account of the rise of mathematical physics at Cambridge University between 1760 and 1940. Like many of the historians contributing to the present issue of *Science in Context*, as well as others who express similar concerns, Warwick explicitly mentions his intention to create bridges with the existing discourse of general historians of science, and more specifically to undertake the task of writing a "social history of mathematical physics," a task that a fellow historian reportedly dubbed "a contradiction in terms." Expressing some of the same concerns raised so far in this introduction, Warwick writes:

> A good deal has been written about the rise of big, mainly experimental science in the twentieth century, but mathematical theory got big, at least in relative terms, a century earlier. It did so by successfully colonizing undergraduate studies in the expanding universities. My interest is focused not so much on why that colonization occurred as on its consequences for the emergence of mathematical physics as a discipline. That said, the first half of this study is as much a contribution to institutional and educational history as it is to the history of science. . . . I am also concerned with the way novel theories are made teachable. . . . On the one hand, training provides a mechanism by which the esoteric culture of mathematical physics is preserved and replicated at new sites; on the other, the peculiarities of each site produce distinct and local research cultures. Attempts to exchange knowledge across these boundaries are especially interesting as they highlight tacit forms of expertise not carried in published works. (Warwick 2003, x–xi)

Also in the present collection we find several contributions that deal with local, as opposed to universal, aspects of mathematical knowledge, and with the ways that the internal dynamics of a given mathematical community is influential in shaping this knowledge. Thus, in an explicit attempt to open a disciplinary dialogue with historians and philosophers of science in general, and using concepts taken from recent historiography of *experimental* science, Moritz Epple's article focuses on a typical, *abstract* (some would say abstruse) sub-specialty of twentieth-century mathematics, knot theory, and discusses a very significant breakthrough achieved in apparent simultaneity in the 1920s in two leading centers of distinguished research, Vienna and Princeton.

The history of experimental science has consistently stressed over the last decade the local, as opposed to universal, components of scientific knowledge, and has thoroughly analyzed the difficulties implied by the attempt to reconstruct experiments at different settings, based only on the textual testimony provided by scientists who originally performed them. Local traditions that involve much tacit know-how, both theoretical and technical, which are fundamental to setting-up, realizing, and analyzing the outcome of those experiments, thus appear as crucial and as extremely difficult to translate from one given environment to the other. This point of view brings to the fore precisely the kind of questions that Netz associates with the "risky" dimension of the history of mathematics, since the stress on local knowledge and irreproducibility of results runs contrary to the standard scientific ethos of universality and possibility of constant replication. Historians of experimental science have addressed this issue in different ways, drawing different conclusions from their historical research (Galison 1987; Rheinberger 1997). Obviously, whatever conclusions one may draw for experimental science concerning these kinds of questions, a possible examination of local (as opposed to universal) underpinning of mathematical knowledge and practice seems always to be distinctly intriguing. If this kind of historical research is applied to one of the typically highly abstract theories that constitute the core of twentieth-century "pure mathematics," then even more so. This is precisely what makes Epple's article alluring, as he attempts to address head-on both the historiographical and philosophical issues that arise from such an analysis.

One basic question to be asked in this attempt concerns the mathematical equivalents of "experiments" and "experimental systems," notions that play central roles when used by historians of science. In this regard it is instructive to point out a debate recently held, not among historians and philosophers, but among practicing mathematicians, following a proposal by the retiring president of the American Mathematical Society for introducing structural changes in the way that current mathematical research is undertaken, supported, and published (Jaffe and Quinn 1993, 1994; cf. Corry 1997, 284–291). The proposal that led to heated discussions among mathematicians included the possibility that the sacrosanct criteria of proof as a basis for publication should be bypassed in certain cases in order to allow for publication of fruitful conjectures (that would take the place of "mathematical experiments"), still in need of proof (the mathematical equivalent of "theory"). Epple's analogy with the experimental sciences is different from this one, since, rather than looking at – and trying to give legitimization to – certain kinds of mathematical practices that can be dubbed "experimental" and that differ from the mainstream, rigorous type, it attempts to look at a very strict and classical manifestation of the latter and tries to explain it in terms similar to those fruitfully used in describing the accepted cognitive practices of experimental science. Epple relies on the idea of an *epistemic configuration* as a way to profit in the historiography of mathematics from conceptual schemes such as elaborated by Rheinberger for the experimental sciences.

Epple's article, then, aims not only to present an interesting account of one important episode in the history of early twentieth-century mathematics, but also to gauge the usefulness of a specific kind of a given conceptual scheme as an analytical tool that the historian of mathematics might use for understanding other situations as well. At the focus of his account there appear two important, competing centers of mathematical activity and excellence, which Epple consistently avoids referring to as "schools," but rather describes mainly as two different "mathematical traditions" or "mathematical cultures."

"Mathematical culture" is also the central analytical category used in David Rowe's article in this collection, dealing with one of the most outstanding centers of excellence in early twentieth-century mathematics, Göttingen. By looking at one distinctive aspect of this "mathematical culture," namely its "oral dimension," Rowe aims to provide the basis for a coherent and comprehensive account of this unique phenomenon in the history of mathematics. However, analyzing the peculiarity of the Göttingen mathematical culture in terms of its oral dimension is a task that intrinsically raises an apparent methodological contradiction for the historian, as it is to be expected that much of the components of an oral culture are difficult to identify through written evidence. Moreover, oral knowledge is by nature unstable, changing, and much less systematic than written knowledge, and therefore, attributing such a central role to it directs the focus of attention to relatively less investigated aspects of mathematical knowledge at large, and to the nature of the rationality that underlies it. The more traditional historiography of mathematics, Rowe stresses, tends in general to overlook such aspects and to concentrate not only on "written," but more specifically "published," expressions of mathematical knowledge.

This viewpoint raises interesting historiographical challenges that Rowe meets in various ways, as he stresses that many members of the community were fully aware of the need to translate the intellectual richness of the Göttingen mathematical oral culture into material media to make it accessible to others. A prominent instance of the various ways that Rowe brings to bear in his analysis is the publication in 1924 of the famous Hilbert-Courant textbook, Courant and Hilbert, *Methoden der mathematischen Physik*, written by Courant alone and based on ideas that at the time were well known in Göttingen, mainly through the courses and seminars of Hilbert after 1900, and that Courant felt the need to canonize as a standard text.

Rowe needs to explain not only the functioning of a mathematical culture with such a strong oral component, but, indeed, also its very possibility. Rowe associates the latter to the growth of research activity at the turn of the twentieth century both in the number of persons involved and in the ever increasing, intrinsic difficulty of research that called for collaborative effort that was previously relatively rare. One among many ways used by Rowe to illustrate the changing modes of producing and communicating mathematics is by comparing two pairs of prominent and enormously influential mathematicians active at Göttingen in the era considered in his article and in previous times. Thus for instance, he calls attention to the differences between Gauss and Hilbert. Whereas the former had practically no students, the latter had more than sixty-eight doctoral students overall and directly influenced dozens through personal intercourse. On the other hand, whereas Gauss' complete works were collected in twelve volumes, Hilbert's went hardly into three, thus emphasizing that his enormous influence in so many fields of mathematics could hardly be explained in terms of his published work. Gauss' main work on number theory, *Disquisitiones Arithmeticae*, is described by Rowe as "much admired, but little read," and he contrasts this with the way in which Hilbert's personal influence – much more than his celebrated, yet hard to master, *Zahlbericht* – was responsible for both the writing of elementary textbooks and advanced dissertations on the topic. Rowe presents his analysis of the essentially oral, mathematical culture of Göttingen relying not only on Gauss and Hilbert but also on similar observations about Riemann-Klein and their successors.

In particular, he describes at length how Einstein's visit to Göttingen in 1915, which signified a major turning point in the history of the reception of the General Theory of Relativity, was essentially embodied in acts of oral and non-published scientific interchanges.

Important mathematical schools and mathematical communities have often exported their ethos, their mathematical approach, and their concepts throughout history. They have even exported their people, be they established leaders or rising stars. The dissemination of mathematical knowledge ensuing from such a process of export, especially from "centers" to "peripheries," has been a focus of research for historians of mathematics in recent years, very much as it has been for historians of science in general (Parshall and Rowe 1994; Pyenson 1895, 1993; Siegmund-Shultze 1998). In the present issue of *Science in Context*, Shaul Katz's article describes the story of the transfer of German mathematical knowledge and ideals, as well as some leading figures who played a fundamental role in the creation and early development of the Einstein Institute of Mathematics, established at the Hebrew University in Jerusalem in 1927. Focusing on the roles of its first leaders, Edmund Landau, Abraham Halevy Fraenkel, and Michael Fekete, Katz describes the quest for the establishment of a research tradition of the highest level in pure mathematics in the framework of what was then a truly peripheral institution at the beginning of its existence in the rather provincial environment of Jerusalem at the time of the British Mandate. Besides its importance to the history of mathematics, this story is intertwined with several other, separate historiographical threads, such as the history of Zionism, and in particular the Hebrew University, and also the question of knowledge transfer from center to the peripheries.

Drawing on a wealth of original archival material, Katz describes in the first part of the article the background for the creation of an institute especially devoted to pure mathematics as part of the plan for establishing the Hebrew University. Landau was among the few truly outstanding Jewish scientists who not only expressed support for the new university, but took a bold step and actually moved to Jerusalem to undertake the directorship of the institute in 1927. Although Landau stayed in Jerusalem for only a little more than a year, his influence became decisive for the further development of mathematics at the Hebrew University. Abraham Fraenkel, together with Michael Fekete, undertook to continue this unlikely enterprise which ended up as nothing less than a great scientific success at the highest international level. In the second part, Katz explains the uneasy dialogue between the leaders and the university authorities, who attempted to introduce applied mathematics in a much stronger dose than Fraenkel considered appropriate for the kind of mathematical institute he had in mind. Katz illuminates the ideological, personal, and institutional struggles that took place behind the scenes. The fact that the Berlin-type ideal of pure mathematics finally prevailed at the Hebrew University was the outcome of a series of incidental circumstances no less than it was the product of a well-planned and fully articulated view.

In the story told by Katz, then, the development of a school promoting the purest ideal of mathematical research is told against the background of the inheritance of time-honored scientific ideals transplanted from center to periphery, of a national ideology in search of a role for scientific excellence, of the determined leadership of two Jewish-German mathematicians who mostly shared the same view about the discipline but differed in their personalities, and of the institutional circumstances that effected the creation of the Hebrew University. Also Amy Dahan's article in this collection is an account of the creation and development of a mathematical school far away from the traditional centers of European science, against the background of a historically unique interaction between social, political, personal, technological, and more purely scientific factors. This story, rather unknown to Western readers, involves a group of distinguished Soviet mathematicians who carried out important research activities in the USSR, specifically in Gorky between 1930 and 1950, and mainly, though not only, under the charismatic leadership of Alexandre Andronov. The juxtaposition of Katz's and Dahan's articles shows the flexibility of the ideas of a mathematical school or a mathematical culture with their suggestive powers, but at the same time the diversity of meanings that they may convey.

The members of Andronov's school worked mainly in fields now grouped under the term "non-linear" science, concentrating on problems pertaining to engineering and physical domains, such as self-oscillations, control theory, and radiophysics. The current interest on non-linear science and the study of "chaotic" phenomena is one main reason for interest in the Andronov school these days. Another important reason is the recent impetus devoted to research in the more general field of the history of Soviet science (Kojevnikov 2002). Looking at the overview of this collection, Dahan's article is of particular interest not only because it touches upon completely different geographical and periodical contexts, but also because it focuses on strictly applied, rather than pure mathematics.

One interesting aspect of Dahan's account, when seen from the perspective of the entire collection, is the stress laid upon the relatively high degree of autonomy that the researchers and their ideas continued to enjoy in the Andronov school, in spite of the very pressing political and ideological situation. Thus, a tradition of research in non-linear phenomena stood at the basis of the school's activities over twenty-five years, and it seems that the peculiar geographical setting and the specific kind of social interrelation among the main figures involved – Maria Grekhova and her husband, the physicist Viktor Gaponov, also played a key role – created a scientific microcosm of sorts in which purely scientific concerns continued to be a major, autonomous motivation. More importantly, Dahan suggestively insists that while the general context of the interaction between state and science during the Cold War, in the Soviet Union on the one hand and in the United States on the other hand, presents many similarities, it is mainly in their content or, rather, in the specificities of the scientific culture, seen from the point of view of their aims, methodologies, and ideals, that these two worlds differ. For her broad account of these kinds of topics, Dahan's article not only presents a story of intrinsic interest, but suggests a provocative point of departure for an interesting dialogue with neighboring disciplines such as the history of technology, interaction between state and science, and more specifically in the Soviet context.

The articles collected in this issue, then, can be seen as original representatives of the highest standards of research practice currently followed in the discipline both for writing and for rewriting the history of modern mathematics, and particularly for attempting to establish dialogues and bridges with neighboring scholarly disciplines. Of course, these articles hardly provide a comprehensive overview of the current state of the art in the discipline, not only in what concerns existing approaches. The relatively strong emphasis given here to pure as opposed to applied issues, to mathematics produced in and derived from German-speaking countries rather than other national and geographical contexts, and to the specific period considered here rather than to earlier or later ones, by no means implies that these are the boundaries actually covered, or that should be covered, by current research in the discipline. As a whole, however, I believe that the collection is in itself a noteworthy contribution and a further stimulus, both in spirit and in content, to the continued and fruitful activity of this thriving field of historical research. I do believe, moreover, that mathematicians, philosophers of mathematics and science, and historians of science in general, who may want to cross the bridges built by historians of mathematics and explore the intellectual vistas that this discipline opens to practitioners of any of those disciplines, will find the present collection to be an illuminating point from which to take the first steps in that direction.

Acknowledgments

I am indebted to Alexandre Métraux, Moritz Epple, and David Rowe for useful comments on an earlier version of this text.

References

Beaney, M. 1996. *Frege: Making Sense.* London: Duckworth.

Bloor, D. 1976. *Knowledge and Social Imagery*. London: Routledge.

Bottazzini, U. 1986. *The "Higher Calculus:" A History of Real and Complex Analysis from Euler to Weierstrass*. New York: Springer.

Bottazzini, U. and A. Dahan-Dalmedico, eds. 2001. *Changing Images in Mathematics: From the French Revolution to the New Millennium*. London and New York: Routledge.

Bourbaki, N. 1969. *Eléments d'histoire des mathématiques*. Paris: Hermann.

Boyer, C. B. 1939. *The Concepts of the Calculus: A Critical and Historical Discussion of the Derivative and the Integral*. New York: Columbia University Press.

Boyer, C. B. 1959. *The History of the Calculus and its Conceptual Development*. New York: Dover.

Brian, E. 1994. *La Mesure de l'État. Administrateurs et géomètres au XVIII^e siècle*. Paris: Éditions Albin Michel.

Corry, L. 1993. "Kuhnian Issues, Scientific Revolutions and the History of Mathematics." *Studies in History and Philosophy of Science* 24:95–117.

Corry, L. [1996] 2003. *Modern Algebra and the Rise of Mathematical Structures* (2nd revised edition). Basel and Boston: Birkhäuser.

Corry, L. 1997. "The Origins of Eternal Truth in Modern Mathematics: Hilbert to Bourbaki and Beyond." *Science in Context* 10:253–295.

Crowe, M. 1967. *A History of Vector Analysis: The Evolution of the Idea of a Vectorial System*. Notre Dame: Notre Dame University Press.

Dauben, J. W. and C. J. Scriba, eds. 2002. *Writing the History of Mathematics: Its Historical Development*. Basel and Boston: Birkhäuser.

Dieudonné, J., ed. 1978. *Abrégé d'histoire des mathématiques, 1700–1900* (2 vols.). Paris: Hermann.

Dieudonné, J. 1994. "Une brève histoire de la topologie." In Pier 1994, 35–156.

Epple, M. 1999. *Die Entstehung der Knotentheorie: Kontexte und Konstruktionen einer modernen mathematischen Theorie*. Wiesbaden: Vieweg.

Ferreirós, J. 1999. *Labyrinth of Thought: A History of Set Theory and Its Role in Modern Mathematics*. Basel/Boston: Birkhäuser.

Fichera, Gaetano. 1994. "Vito Volterra and the Birth of Functional Analysis." In Pier 1994, 171–184.

Freudenthal, H. 1977. "What is algebra and what has been its history?" *Archive for History of Exact Sciences* 16:189–200.

Galison, P. 1987. *How Experiments End*. Chicago: University of Chicago Press.

Geison, G. 1981. "Scientific Change, Emerging Specialties, and Research Schools." *History of Science* 19:20–40.

Gillies, D., ed. 1992. *Revolutions in Mathematics.* Oxford: Clarendon Press.

Goldstein, C. 1995. *Un théorème de Fermat et ses lecteurs*. St. Denis: Presses Universitaires de Vincennes.

Grabiner, J. 1975. "The Mathematician, the Historian, and the History of Mathematics." *Historia Mathematica* 2:439–447.

Grattan-Guinness, I., ed. 1980. *From the Calculus to Set Theory, 1630–1910*. London: Duckworth.

Grattan-Guinness, I. 1990. "Does the History of Science Treat of the History of Science? The Case of Mathematics." *History of Science* 28:149–173.

Gray, J. J. [1979] 1989. *Ideas of Space: Euclidean, non Euclidean, and Relativistic* (2nd edition). Oxford: Oxford University Press.

Hawkins, T. 2000. *Emergence of the Theory of Lie Groups: An Essay in the History of Mathematics: 1869–1926*. New York: Springer.

IHPMM (*International History and Philosophy of Mathematics Meeting*), Sevilla, 17–19 September 2003. Department of Philosophy and Logic, University of Seville. http://www.us.es/ghum717/

Jaffe, A. and F. Quinn. 1993. "Theoretical Mathematics: Towards a Cultural Synthesis of Mathematics and Theoretical Physics." *Bulletin of the AMS* 29:1–13.

Jaffe, A. and F. Quinn. 1994. "Responses to M. Atiyah et al." *Bulletin of the AMS* 30:208–211.

Jahnke, H. N., ed. 2003. A *History of Analysis*. Providence, RI: American Mathematical Society.

James, I. M., ed. 1999. *History of Topology*. Amsterdam: Elsevier.

Kline, M. 1972. *Mathematical Thought from Ancient to Modern Times*. Oxford: Oxford University Press.

Kojevnikov, A. 2002. "A New History of Russian Science." *Science in Context* 15:177–182.

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

Lorenzo, J. 1977. *La Matemática y el Problema de su Historia*. Madrid: Tecnos.

Lützen, J. 1982. *The Prehistory of the Theory of Distributions*. New York: Springer.

Lützen, J. 1990. *Joseph Liouville,*1809–1882*: Master of Pure and Applied Mathematics*. New York: Springer.

Mackenzie, D. 1999. "Slaying the Kraken: The Sociohistory of a Mathematical Proof." *Social Studies of Science* 29:7–60.

May, K. O. 1975. "What is good history and who should do it?" *Historia Mathematica* 2:449–455.

Mehrtens, H. 1990. *Moderne – Sprache – Mathematik: Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme*. Frankfurt/Main: Suhrkamp.

Mikulinskiy, Semën R. and Michail G. Yaroshevskiy. 1977. *Shkoly v nauke*. Moscow: Izdatel'stvo "Nauka."

Mikulinskij [Mikulinskiy], Semën R. and Michail G. Jaroševskij [Yaroshevkiy]. 1979. *Wissenschaftliche Schulen*. Berlin: Akademie Verlag.

Netz, R. 2003. "The History of Early Mathematics – Ways of Re-Writing." *Science in Context* 16(3):275–286.

Parshall, K. H. 2004 (forthcoming). "Defining a Mathematical Research School: the Case of Algebra at the University of Chicago, 1892–1945." *Historia Mathematica* 31.

Parshall, K. H. and D. E. Rowe. 1994. *The Emergence of the American Mathematical Research Community, 1876–1900*. Providence, RI: American Mathematical Society.

Peckhaus, V. 1990. *Hilbertprogramm und Kritische Philosophie. Des Göttinger Modell interdisziplinärer Zusammenarbeit zwischen Mathematik und Philosophie*. Göttingen: Vandenhoeck and Ruprecht.

Pier, Jean-Paul, ed. 1994. *Development of Mathematics, 1900–1950*. Basel-Boston-Berlin: Birkhäuser.

Pier, Jean-Paul, ed. 2000. *The Development of Mathematics, 1951–2000*. Basel-Boston-Berlin: Birkhäuser.

Pyenson, L. 1985. *Cultural Imperialism and Exact Sciences: German Expansion Overseas, 1900–1930*. New York: P. Lang.

Pyenson, L. 1993. *Civilizing Mission: Exact Sciences and French Overseas Expansion, 1830–1940*. Baltimore: Johns Hopkins University Press.

Rheinberger, H.-J. 1997. *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*. Stanford: Stanford University Press.

Rowe, D. E. 2003. "Mathematical Schools, Communities, and Networks." In *Cambridge History of Science, vol. 5, Modern Physical and Mathematical Sciences*, edited by Mary Jo Nye, 113–132. Cambridge: Cambridge University Press.

Scholz, E. 1980. *Geschichte des Mannigfaltigkeitsbegriffs von Riemann bis Poincaré*. Basel: Birkhäuser.

Scholz, E. 1989. *Symmetrie, Gruppe, Dualitat: zur Beziehung zwischen theoretischer Mathematik und Anwendungen in Kristallographie und Baustatik des 19. Jahrhunderts*. Basel: Birkhäuser.

Scholz, E., ed. 2001. *Hermann Weyl's "Raum-Zeit-Materie" and a General Introduction to His Scientific Work*. Basel, Birkhäuser.

Siegmund-Schultze, R. 1998. *Mathematiker auf der Flucht vor Hitler*. Wiesbaden/Braunschweig: Vieweg.

Unguru, S. 1975. "On the need to rewrite the history of Greek mathematics." *Archive for History of Exact Sciences* 15:67–114.

van der Waerden, B. L. 1976. "Defense of a 'Shocking' Point of View." *Archive for History of Exact Sciences* 15:199–210.

Warwick, A. 2003. *Masters of Theory: Cambridge and the Rise of Mathematical Physics*. Chicago and London: University of Chicago Press.

Weil, A. 1978, "Who betrayed Euclid?" *Archive for History of Exact Sciences* 19:91–93.

Wussing, H. 1969. *Die Genesis des abstrakten Gruppenbegriffes. Ein Beitrag zur Entstehungsgeschichte der abstrakten Gruppentheorie*. Berlin: VEB Deutscher Verlag der Wissenschaften.