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# Anxiety and Abstraction in Nineteenth-Century Mathematics

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## Argument

The first part of this paper surveys the current literature in the history of nineteenth-century mathematics in order to show that the question “Did the increasing abstraction of mathematics lead to a sense of anxiety?” is a new and valid question. I argue that the mathematics of the nineteenth century is marked by a growing appreciation of error leading to a note of anxiety, hesitant at first but persistent by 1900. This mounting disquiet about so many aspects of mathematics after 1850 is seldom discussed. The second part explores the issue of anxiety in mathematical life through an interesting account of an address made by a mathematician in 1911, Oscar Perron. The third and final part ventures some conclusions about the value of anxiety as a question for historians of mathematics to pursue.

## 1. A Brief Survey of the Current Literature

*A “standard model:” the nineteenth century as a century of progress*

Historians of mathematics like to portray the growth of mathematics in the nineteenth century as a success story. They start with the reforms of French education begun during the French Revolution. They note with pleasure such themes as the rigorization of the calculus,<sup>1</sup> the re-discovery of projective geometry,<sup>2</sup> the discovery of Non-Euclidean geometry (e.g., Gray 1989 and Gray 2000a), the emergence of group theory (Wussing [1969] 1979 1984), the development of Fourier theory from its inception to the discovery of infinite sets of different cardinalities (Hawkins 1975), complex function theory from Cauchy to Riemann and Weierstrass,<sup>3</sup> and so on in a long list. There can be no quarrel with any of this: the nineteenth century is indeed the period when many of the major discoveries that occupy modern mathematics were made. This can lead to an over-estimation of the century. Some historically minded mathematicians seem

<sup>1</sup> Grabiner 1981 is a good example, but there are many other texts.

<sup>2</sup> We still lack a good modern historical account of projective geometry, but Coolidge 1940 is informative.

<sup>3</sup> Another topic that lacks a good, full-length modern historical account, but for the moment see Bottazzini 1981, chap. 4, and Bottazzini 1999, chap. 8.

to have a feeling that mathematics starts again in 1800, a feeling that has more to do with the way knowledge of the real achievements of Euler and Lagrange has lapsed from their awareness than from any insight into the eighteenth century. It is produced by a feeling that mathematics is concept-driven, whereas the eighteenth century was more interested in “mere” calculation, and this emphasis on concepts was one of the achievements of the nineteenth century. This is not the place to rescue the eighteenth century from such a criticism — which could be done in a variety of ways — but to notice that the criticism is a way of observing that the achievements of the nineteenth century are real indeed.

These achievements may be thought of in many ways. From one perspective, they were of two overlapping kinds: some enlarged the sphere of mathematics; some performed existing tasks better than before. Multi-dimensional geometries were only created in the nineteenth century; the theory of real functions was an eighteenth-century invention, but rigorous function theory with good proofs of the existence of solutions to differential equations happened only in the nineteenth century. Group theory is a nineteenth-century branch of mathematics; the misnamed fundamental theorem of algebra finds its proof, but not its role, in the nineteenth century.

From another perspective, some of these discoveries helped bring about, and others helped deepen, areas of application of mathematics, be they traditional or new. Potential theory grew out of attempts to make better mathematics sense of Newton’s theory of gravitation, and was speedily adapted to problems in the nineteenth-century science of electricity and magnetism. Many branches of analysis have a root (if not all their roots) in scientific questions. The as-yet unwritten history of partial differential equations contains many examples, some where the mathematics came before and others where it followed the physics of the day. And here we notice a significant tension between the mathematics that physicists want, and can often provide for themselves, and the mathematics that mathematicians can accept. The insights that guide a physicist and a mathematician can be different, and they translate into different views about the nature of proof. The role of mathematics in science is a complicated one, and insufficiently discussed, but even as mathematics and physics grew into distinct disciplines there was a sense that the mathematical analysis of nature was progressing with the century.

A third perspective would stress the growing abstraction of mathematics in the nineteenth century, and would point to shifts in its foundations. Examples will be given below to show that it did indeed seem to many people at the time that mathematics was moving away from reality, into worlds of arbitrary dimension, for example, and into the habit of supplanting intuitive concepts (curves that touch, neighboring points, velocity) with an opaque language of mathematical analysis that bought rigor at a high cost in intelligibility. Yet others could and did reply that basing counting on the simple intuition of a set of objects to be enumerated, and building up mathematics from there (the movement for arithmetization of mathematics) was actually simpler than the problematic distinction between number and magnitude. The concept of continuous magnitude, exemplified in daily life by the things one measures (lengths, areas, volumes,

angles) had been shown to be mysterious. Why, the arithmetizers might say, was the movement to define these concepts in more rigorous ways to be disparaged, and why disparaged as abstract when it rested on fundamental activities of the human mind? Was it not more plausible that deriving mathematics from an elementary theory of sets was more direct and natural?

A fourth perspective, more critical than any of these, would be to bring to center stage the branches of mathematics often held in the wings: statistics, actuarial mathematics, engineering mathematics in its many forms.<sup>4</sup> But even as this criticism is directed against the traditionally minded historian of mathematics the prevailing trend of historical analysis is again endorsed, for all these branches too are among the successes of the nineteenth century. They too grow in size and deepen in insight. They too acquire a maturity governing their use and make connections with the intellectual world around them. The traditional historian may feel that these subjects are only semi-mathematical, and cite their continuing existence more outside the mathematics profession than in, but progress remains the governing theme.

Mathematicians in the nineteenth century knew that they were often doing mathematics that their predecessors could not have dreamed of, and that they were often doing mathematics their predecessors had not done with sufficient precision. They knew that they were tied into the new sciences, intimately if not always satisfactorily. They knew that they were, if not more intellectually honest, then certainly more intellectually shrewd than their predecessors. Historians of mathematics can sympathize with all those judgments, even as they find, like all historians, that the past was never as any one protagonist saw it (let alone as recollected in tranquility). From this near concurrence of the evident facts, the actors' judgments, and the historians' sympathies, has come what might be called the standard model of the history of nineteenth-century mathematics. This history depicts real success and real innovation, often in rich detail. While this model has been built up there have been few reversals of fortune, but the complexity of the picture has been understood over the last twenty years as never before, and without complexity there is no life. Gaps remain in the account, several topics have scarcely been taken up, much remains to be done, but the message is clear.

### *Error in Mathematics*

It is against this background of progress that I wish to develop another approach. I do not intend to diminish the achievements of the standard model, but to augment them with another note, hesitant at first but growing to a crescendo around 1900–1914. That note is the sound of anxiety. I shall approach the mathematics of the nineteenth century by seeing it as marked by a growing appreciation of error.

<sup>4</sup>A notable advocate of this point of view is Grattan-Guinness, as exemplified in the Encyclopaedia he edited, Grattan-Guinness 1993.

Mathematicians have traditionally had a low tolerance for errors, although some have been more willing to make sweeping conjectures than others, and some even to go into print with poor arguments (Wallis is a famous case in point). But during the nineteenth century the awareness of errors grew and became a source of anxiety in mathematics, for reasons it is worth analyzing.

Some significant errors were rooted in practice. The failure of standard methods to produce convincing evaluations of integrals, signaled by the existence of conflicting values, is one example (Bottazzini [1981] 1986, 137). The failure of mathematicians in the eighteenth century to come up with foundations of the calculus that were generally acceptable is a different kind of failure — one might call it a failure in the philosophy of mathematics (but that is a term whose validity will have to be discussed below) (see the discussion in Grabiner 1981). The failure of Euclidean geometry is a third kind of failure: against almost all expectations it was shown that there were two mathematically sound descriptions of physical space.

All these failures must be contrasted with what might be called routine failures, those occasions when systems of equations defy solution, when a mathematical description of a process does not yield useful results, when, simply, a problem is too difficult. Such occasions merely signaled that there was work to be done, that new methods might be necessary; the task was not finished. They formed, and form, the sour parts of daily mathematical life. The failures that came to animate whole aspects of nineteenth-century mathematics are of a deeper kind, because they pointed to deeper difficulties with the whole enterprise of mathematics. It is characteristic of them that they did not go away. Resolving the original difficulty typically led to others.

Cauchy showed how some questions about integrals could be tackled by thinking more carefully about the path of integration. Related questions arose in the eponymous work of Green and Stokes (see the discussion in Bottazzini [1981] 1986, 137). But the delicate questions about paths, and about the continuity of the functions involved not only remained unsolved all century, but also came to seem more elusive and more profound (see Gray 2000b). Cauchy, again, gave definitions of differentiability and integrability that survive to the present day. But his definition of continuity was less satisfactory, and later mathematicians were to realize that he was presuming more about the nature of the underlying system of real numbers than he had thought to question. Controversy about the nature of the real number system grew as the nineteenth century came to an end (see Epple 1999), even if workable (or rather, rigorous) versions of infinitesimals had to wait until the 1950s. Attempts to shift the foundations of mathematics onto set theory foundered with the recognition that naïve set theory led to irresolvable paradoxes, but axiomatic set theory has not led to unanimity yet concerning its deepest assumptions (the axiom of choice) and in French circles at the start of the twentieth century debate was particularly vigorous and wide-ranging (Borel et al., [1904] 1914).

A famous case of someone passionate about error in what should be rigorous and sound is that of Frege. One quote must suffice: “Mathematics should properly be

a paradigm of logical rigour. In reality one can perhaps find in the writings of no science more crooked expressions and consequently more crooked thoughts, than in mathematics.”<sup>5</sup> Frege is notorious for his provocative style, but it is interesting that after quoting this remark of Frege’s, A. Voss elected not to argue the charge (either for or against) but to examine the sources of error that can arise, as he put it, despite an absolutely certain method. He thereupon turned to Perron’s article, which we shall consider below.

This mounting disquiet about so many aspects of mathematics is seldom discussed as a widespread part of mathematics life after 1850, although the individual cases of it are of course discussed in essays and books devoted to the relevant particular parts of the field. The disquiet contrasts with the dominant image of the nineteenth century in mathematics, the standard model of widespread innovation and success. But in fact there is no real contradiction in the idea of continuing success co-existing with anxiety. Indeed, it has been argued by some authors that the start of the twentieth century is to be characterized culturally as an age of anxiety, despite (even because of) the immense achievements of the time and the profound changes they unleashed.<sup>6</sup> I shall argue here that once the safe havens of traditional mathematical assumptions were found to be inadequate, mathematicians began a journey that was not to end in security, but in exhaustion, and a new prudence about what mathematics is and can provide.

### *The Reliability of Mathematics*

One feature of mathematics that is most cherished is its reliability. Mathematicians, and many who use mathematics, regard its findings as among the most certain and true things we know. The claim that mathematics is a particularly clear kind of knowledge, even paradigmatic, has been taken seriously by many philosophers; few, by contrast, have sought to deny the claim or limit it severely. Mathematical physicists often deny that mathematics is at the core of their understanding, and prefer various kinds of physical insight, but they consume and produce a great deal of mathematics, in which they happily put their trust. And no mathematician would get very far in any ambitious program of research without believing that they stood on the firm ground of proven results. The confidence all these people have is based on the fact that mathematicians habitually prove their assertions. It is the nature of mathematical proof that provides the special feature of a mathematician’s work, and upon which so many others then rely. But the nature of proof, what it is for an argument to be a proof, became just

<sup>5</sup> Quoted in Voss 1914, E 26. “Die Mathematik sollte eigentlich ein Muster logischer Strenge sein. In Wirklichkeit wird man vielleicht in den Schriften keiner Wissenschaft mehr schiefe Ausdrücke und infolgedessen mehr schiefe Gedanken finden, als in der Mathematik.”

<sup>6</sup> To cite only one author: “The perception that [popular sovereignty] was now the reality, or at least the destiny, of the modern state, . . . was a powerful conditioner of middle-class anxiety, pessimism and fastidiousness” (from Burrow 2000, xi).

as unclear during the nineteenth century as any of the specific failures of important arguments to be a proof.

This anxiety about the very nature of proof coexisted with successful theorem proving for a variety of reasons and in a variety of ways. Nineteenth-century mathematicians were robust people in the main, and were quite capable of taking the common-sense position, when faced with seemingly philosophical dilemmas, of saying “I may not know what a proof is, but I know one when I see one.” They could put their trust, too, in the social aspect of proving theorems, the shared agreement that such-and-such an argument was indeed a proof. Their confidence was doubtless increased by the concomitant ability some had of deciding that certain other arguments were not, in fact, proofs. Jacobi wrote that “If Gauss says he has proved something, it seems very probable to me; if Cauchy says so, it is about as likely as not; if Dirichlet says so, it is certain. I would gladly not get involved in such delicacies.”<sup>7</sup> And Dirichlet’s wife tells us that Jacobi would spend hours with Dirichlet: “Being silent about mathematics. They never spared each other, and Dirichlet often told him the bitterest truths, but Jacobi understood this well and he made his great mind bend before Dirichlet’s great character” (quoted in Scharlau and Opolka 1985, 148).

Recognizing fallacious arguments tends to strengthen one’s confidence that the valid arguments are indeed valid. Mathematicians also developed an understanding of the subject that leads them very often to re-derive results that others have found. Poincaré was by no means the only mathematician who, on hearing of a new result, would immediately set about seeing how to prove it, and would only laboriously follow the original proof when his own intuition failed him. Lesser, but still very good mathematicians would do the same, quite possibly with higher standards of rigor. The ability to find a new, possibly different, proof also strengthens one’s confidence, not only in the original result, but also in the activity of proving. Even the up-and-down business of finding a proof, then finding a flaw in it, then a new proof, and perhaps a new flaw, often converged on a satisfactory argument; further evidence that mathematics is based on proof.

### *Real Existing Imperfection in Mathematics*

The existence of proofs of often spectacular results was nonetheless not as secure as it might seem. Numerous mathematicians accepted arguments that were shoddy or fanciful. Mathematical journals filled up with poor arguments as well as sound ones. As Jacobi’s remark indicates, confidence in proofs was likely to be confidence in idealized proofs, or those produced only rarely and by a select few mathematicians, not confidence in real existing proofs published in contemporary journals. Mathematicians had simply to accept that the standards in the field were variable. What had been called routine failures earlier included: in analysis, inadequate convergence arguments

<sup>7</sup>Jacobi to A. von Humboldt, 1846, quoted in Pieper 1980, 23.

(sometimes wholly lacking) (Lützen 1990, 456); in geometry, restrictions to the generic case (Hawkins 2000, 109); in algebra, the assumption that all roots of a polynomial are distinct (Hawkins 1977). Faced with such a mistaken argument, the better mathematicians would usually be able to check that the given argument could be repaired or supplemented, or they could see how to restrict the proof to the situation it really did cover and to employ it with greater care. Such work, often important, often interesting, was and is nonetheless routine. It is mentioned here only to establish that standards in the field were not uniformly high. Mathematicians in the nineteenth century could not and did not believe that they and their contemporaries lived up to the highest standards of proof. Daily mathematical life was error-prone. Once disquiet arose, it was not going to go away with a reassuring immersion in what one's colleagues do; such reassurance was simply not there.

In this context, notice also some topics that were not pursued but might have been. After Darboux's study of uniform convergence in the 1870s, which included his account of a continuous, nowhere differentiable function (Darboux 1875), French mathematicians waited more than a decade to respond. From lack of interest, or a distrust of one's abilities in such a novel, and unintuitive, an area? Another example is the curious lack of interest in differential geometry in dimensions greater than 2, despite the pioneering work of Riemann. Some work was done, much of it by Bianchi, but less than one might imagine before Einstein's theory of general relativity.

There are also significant results that people refused to accept. French textbooks discussed continuous functions as if they were differentiable (Laugwitz 2000). No less a mathematician than Bertrand endorsed defenses of the parallel postulate well after Non-Euclidean geometry was established.<sup>8</sup> He was speedily criticized for this by Darboux and others, but Cayley was another who failed to grasp the full import of Lobachevskii's work (Cayley 1865; see also Richards 1979 and 1988).

### *Historians' Ways with Imperfections*

It can be argued that historians of mathematics have insufficiently appreciated the question of inadequate proofs in mathematics. We have been concerned to show how each generation advanced, and comment when they sort out a mistake made by their predecessors. We are less comfortable when inadequate arguments pass into the mathematical record, and hurry quickly past these blots on the collective copybook as if blinded by the successes of the standard model.

Some examples will sharpen the point. Morris Kline: "Cauchy here made some additional missteps with respect to rigor. . . . He overlooked the need for uniform convergence. . . . Cauchy ultimately recognized the need for uniform convergence. . . but even he at that time did not see the error in his use of the term-by-term integration of series" (Kline 1972, 964–5).

<sup>8</sup> See the introductory essay in Gray and Walter 1977.



Grattan-Guinness: “Naturally, Cauchy did not resolve all questions of rigour; he did not often distinguish multiple from repeated integrals, he used infinitesimals fairly freely, . . . and so on” (Grattan-Guinness 1990, 695).

Lützen: “In spite of its simplicity, this proof presents the most profound mistake in Sturm and Liouville’s theory of second-order linear differential equations. I do not refer to the curious neglect of the problem of convergence, solved the following year, nor to the term-by-term integration, but to the last step. . . . In fact, one has to use a totally different approach to prove this theorem” (Lützen 1990, 456).

Bottazzini, writing about Riemann’s work on complex analysis: “This global view would be taken up with the necessary rigor by Weyl (1913)” (Bottazzini [1981] 1986, 239).

Gray: “At this point Poincaré assumed . . . which is false” (Gray 2000a, 207).

Hawkins: “Of course, generic reasoning would be unobjectionable if the conditions of the validity of a proposition were made precise, but this was never done, and the result was completely unsatisfactory” (Hawkins 2000, 109).

It should be said straight away that from certain standpoints there is nothing wrong with these quotations, and others it would be easy to find. It is good practice (if optimistic) for historians to imagine that their readers will consult the original sources and read them carefully. These readers will see the mistakes, and sometimes all the historians are doing is assuring them that they have seen them too, so that no misunderstanding can arise on that score. The historians signal that they are trustworthy guides. This is particularly necessary when another question is under discussion and to discuss the quality of the mathematics would be to digress. For example, when priority questions are at stake, and the more important argument is that, as it might be, Liouville was the first to show something, although his proof was flawed.

Another legitimate use for such an approach might be that these remarks are made as part of a discussion about the historical response. Yes, Cauchy said this, and Abel criticized him for it. Yes, Riemann did this and Weyl took up the issue. The historian confirms that there was an error in order to help the reader understand how the error was dealt with. It is important to follow carefully how mathematicians of the past responded to each other’s work, and many remarks are made of this kind to direct the readers’ attention.

Where such comments betray a shallowness in the historian’s approach is when they are made with a modern mathematical sensibility and judge the work of the past anachronistically. Such occasions may be recognized by the presence of a number of diagnostic features. The specific mathematical error is corrected by a specific, and subsequent, mathematical technique. No significant historical point about the error is made, it is simply left until the appropriate correction is given (perhaps by the historian directly).<sup>9</sup> Above all, the tone of voice. It is easy to imagine the scribble in the margin

<sup>9</sup> For good examples of how to discuss such matters, however, see Giusti 1984 and Lützen 1999.

in red ink, as if to a student: “Uniform convergence needed, put right” perhaps, or “Argument only works for distinct eigenvalues” or “Argument only works for simple singular points.” On such occasions, one feels that the great sweep of progress required by the standard model must be seen to sweep out of the way the little pebbles of imperfection, and if need be the historians will put the mistakes right themselves.

How might it be done differently? I have already noted that one commonly encountered way is to immerse the noting of error in a reconstructed contemporary debate about the caliber of the proofs. When this is done well the effect is a better balance between the results (often taken to be timelessly correct) and the methods (often treated as mere human creations). If we take the view that acceptable mathematics, indeed, that which is mathematics, is what has been proved, that it is the proof which warrants recognition of a hitherto unknown eternal truth, then proofs — and their absences — are as much the business of the historian of mathematics as results. The judgmental tone is unnecessary, and may even obstruct our understanding of what is going on.

What is less usually provided is reflection on what all this imperfect proving might mean. There are explicit discussions in the literature: Judy Grabiner’s “Is mathematical truth time-dependent?” (Grabiner 1974) and Imre Lakatos’ *Proofs and Refutations* (Lakatos 1976), for example. The former is actually a discussion of why a major change came about, as the author sees it, from symbolic manipulation to a style of mathematics that puts rigor first.<sup>10</sup> But the latter is aimed at philosophers of mathematics, for all its historical focus on the history of polyhedra, and its Appendix 1 (on Cauchy’s above-mentioned mistake) has been rendered obsolete by the accounts of Guisti and Lützen mentioned above.

The diverse nature of real existing proofs would be an interesting subject for the historian of mathematics. Much effort has been spent in the last twenty years to include accounts of methods in the history of nineteenth-century mathematics, and it is clear that the culture of proving things in mathematics is no more one of steady accumulation than is the history of results. It is true that mathematicians know more and more about some things, and it is also true that they know about more and more things, and in this way knowledge accumulates. But it is also true that they know about different things. What a function is, what a curve is, what geometry is, are all topics that mathematicians in different centuries would answer differently. The present answers are highly abstract, and their very abstraction is sometimes criticized for being unintuitive to the point of unintelligibility or for being arcane, and sometimes supported for helping to make mathematics widely and unexpectedly useful. With this altered ontology came a new epistemology, and the present state of affairs should not be seen as having come about simply from a lofty sense of mission, but in fair part from a painful state of anxiety.

<sup>10</sup> Grabiner’s theme is not the truth of mathematics, and her conclusion is a marked retreat from the idea raised in her title: truth may be eternal but our knowledge of it is not.

## 2. Anxiety in mathematical life

### *Criticism of mathematical arguments was widespread*

It has already been said that the historical literature often deals with specific failures of mathematicians. The poor reception of Bolyai and Lobachevskii's Non-Euclidean geometry is a case in point, and the continued production of purported "proofs" of the parallel postulate is another. The controversies surrounding the discovery and use of uniform convergence is another, and one that has been alluded to several times already. The controversy about the use of infinite sets is a fourth. The point at issue in the second part of this paper is whether these were isolated cases, or whether they were part of a larger picture.

One could begin by asking how many cases were there, and over what period of time? The history of Non-Euclidean geometry begins with attempts to defend the parallel postulate, taken as undoubtedly true but in need of a proof (the view of all Greek and Arab investigators) (see Gray 1989 and 2000a for the later history). Historians remain divided as to whether Gauss knew as much as Bolyai and Lobachevskii, but there is no dispute that whereas they were happy to publish, he declined. Much of what we know about Gauss' ideas in Non-Euclidean geometry are culled from his posthumously published writings. His stated view on one famous occasion was that he feared the cry of the Boetians,<sup>11</sup> and it has been suggested that he never had the clear view of the essence of the new geometry that he wished to have before going into print (Dombrowski 1979, 121–123). The inference is strong, however, that something about the new geometry prevented him from pulling his ideas together and investigating them properly as undoubtedly he could have done. The new geometry was disquieting to him, as it was to those whose strong instincts were to reject it once it had been discovered.

The original publication of Bolyai was hard to find and hard to understand. Lobachevskii's versions were easier to find, but not always logically compelling. They rank, indeed, among the more striking examples of a mathematician venturing into print with highly novel ideas and less than rigorous arguments in their support. Once Beltrami, in 1868, had published arguments that were rigorous, mathematicians swung behind them. But the young Klein still felt it prudent to refer to the *sogennante* or so-called Non-Euclidean geometry in 1871, and as we have seen both Bertrand and Cayley held out for a while against the validity of a new geometry. Bertrand fell for a mistake that anyone familiar with Beltrami's work should have spotted in five minutes, which suggests a lingering preference for the parallel postulate. Cayley held out more against the validity of Non-Euclidean geometry as the geometry of space, perhaps because, as Richards has suggested, he feared the implications for contemporary morality if the a priori truth of mathematics was ever satisfactorily impugned.

<sup>11</sup> Gauss in a letter to Bessel, 27 January 1829 (see Gauss 1900, 200).

The discovery of Non-Euclidean geometry could not be regarded, even by its supporters, as a simple success. What the existence of the new geometry established was that the old, Euclidean geometry, was not necessarily true, and that the claim of mathematics to be true had either to be restricted to arithmetic, or reformulated in some way. For those mathematicians who took no interest in the philosophical, or even pedagogic, implications of their subject, this implication could be ignored, but for all others, and many in the wider intellectual community, it could not. Torretti has surveyed the responses of some of the more mathematically inclined philosophers (Torretti 1984), and it is plain that they strove mightily to overcome what they saw as a huge problem, exacerbated by whatever position they took up in the ongoing struggle to be the best sort of neo-Kantian.<sup>12</sup> Philosophers are not mathematicians, but the question of the truth of mathematics is a profound one, and one can at least note that no mathematician had an answer to it either. The discovery of Non-Euclidean geometry is a classic case of a discovery that raised more questions than it answered and more problems than it solved.

Projective geometry is a subject that was revived from a moribund state in 1800 to become one of the hegemonic subjects in 1900. But it cannot be regarded as an unproblematic success story. If Poncelet is to be credited with its rebirth, his use of imaginary and ideal points and his principle of continuity caused much disquiet. Cauchy attacked it, and so confident was Poncelet of the need for a reformulation of geometry that he reprinted Cauchy's attack in the preface to his *Traité*, but he seems to have found little support, and moved away from the subject and into the theory of machines in the 1830s. The principle of continuity thereafter exerted little appeal, but the use of imaginary points remained, and burdened the research and pedagogic literature until it contributed to the demise of the synthetic branch of the subject in the twentieth century (Samuel 1988, v). To quote Coolidge: "It is hard to escape the conclusion that the field of synthetic projective geometry, except perhaps in the matter of studying the postulates, is pretty much worked out" (Coolidge [1940] 1963, 104).

Poncelet's approach was heavily synthetic, in the sense of being a general form of reasoning about figures without much in the way of calculation. He deliberately challenged the algebraic way of thinking about geometry that had been growing in importance since Descartes, and the history of projective geometry in the nineteenth century is often and usefully seen as a contest between the synthetic and algebraic ways of proceeding. Among the algebraists were Möbius and Plücker, and Plücker felt himself to be in competition with the much more synthetic approach of Steiner. So much so that at one point he abandoned the subject for several years and took up experimental chemistry with much distinction, and only returned to geometry after Steiner had died.

Plücker's work is sometimes obscure. He was inclined to count the number of configurations of a particular kind by counting the number of solutions to a system

<sup>12</sup> See Kohnke [1986] 1991 for an account focused on the more institutional aspects of the matter.

of equations, but this sometimes led him into error (as in his study of bitangents to a quartic curve) (Plücker 1839; see Gray 2000a, 151). This mode of reasoning remained popular throughout the nineteenth century, and bedeviled the study of complicated singular points and their resolutions. This is intimately connected to the Brill-Noether Theorem, as so profound were the switches back and forth on this complex of results that Noether himself was said on one occasion to have been reduced to tears. As late as 1923 Bliss was to record, in a Presidential address to the American Mathematical Society, that the matter was not satisfactorily resolved (Bliss 1923).

But if Plücker and the algebraists who came after him left a chain of inadequate proofs behind them, the synthetic tradition proved even worse. Steiner was so obscure that Cremona called him “the celebrated sphinx” (Cremona 1868, 3). It is the synthetic tradition, with its preference for real configurations, that was most harmed by the lack of clarity between real and complex points (algebraic geometry rapidly became the study of complex algebraic geometry). A further twist in this debate was captured by Nagel many years ago, when he observed that the emphasis synthetic geometers placed on duality (between points and lines in the plane) made no intuitive sense: one intuitively grasps the idea of a line as made up of points and the point as fundamental, but rejects the idea of a line as a fundamental object (Nagel 1939). This, he suggested, was a distinction that did much to push mathematicians towards a purely logical conception of their subject. One might add that the process which starts with Pasch’s axiomatization of geometry and reaches by 1900 to Hilbert’s axiomatic geometry and the even more radical accounts of several Italian mathematicians seeks foundational clarity quite openly at the expense of intuition, which was at the very least a journey in barely twenty years that broke entirely with Pasch’s own views on the nature of geometry. We must conclude that geometry, for all its successes, did not provide even geometers with a reliable body of knowledge.

Indeed, Pasch himself wrote of the disturbing state of geometry that was one of his reasons for writing his celebrated book of 1882. In the preface he compared the new, advanced, synthetic geometry with the old, elementary geometry. “Elementary geometry,” he wrote, “cannot only be reproached for its difficulties, but also for incompletenesses and obscurities, which the ideas and proofs still retain in extended measure. The repair of this defect is an incessant struggle, in manifold ways, and if one examines the results one can come to the opinion that that the struggle is hopeless” (Pasch 1882, 2). Naturally, he went on to show how, in his opinion, the task was not hopeless after all, and indeed that his book finally put the matter to rest. The point here is that the criticism of elementary, intuitive geometry from the standpoint of late nineteenth-century criteria of rigor was always finding faults, so much so that that whole branch of introductory mathematics had come to seem riddled with flaws and perhaps beyond repair. This is a most uncomfortable position for a mathematician to be in, and it was to that anxiety that Pasch responded with his book.

The history of real analysis, and especially the foundations of real analysis, is one that historians have investigated most thoroughly, so we may be brief here. Cauchy provided

analysis with good definitions of continuity and the other basic terms of analysis, but left the nature of the real numbers unclear. After much debate, including some unexpected ideas about the number system such as those Weierstrass proposed, a consensus grew up around the different but equivalent approaches of Cantor and Dedekind.<sup>13</sup> But these approaches, in their different ways, committed the mathematician to a naïve theory of sets, and a break with the intuitive, but inadequate, concept of the number line. This theory committed the mathematician around 1900 to whole new orders of infinite sets, which might have been acceptable to all but a few (such as Kronecker, and his former student Molk) who were committed to a radically different philosophy of mathematics. However, when the naïve theory of sets succumbed to paradoxes, voices were raised suggesting that the whole idea of large uncountable sets was an illusion, one that lay beyond the human mind. Poincaré had reservations, and spoke of mathematicians responding to the challenge as a doctor to a beautiful pathological case (in Poincaré 1908).<sup>14</sup> Emile Borel was much more sweeping in his criticisms, as the famous *Cinq Lettres* attest (Borel [1904] 1914). Such a debate does not suggest that the line from Cauchy to Cantor and Dedekind and beyond was a simple case of progress.

But neither did the sharp division between the continuous and the differentiable that Cauchy had introduced prove comfortable. The existence of continuous, nowhere differentiable curves, first suspected by Bolzano and more conclusively established by Riemann and Weierstrass independently, led via the work of Klein and Poincaré in Non-Euclidean geometry to the discovery of a host of such curves, famously repellent to Hermite.<sup>15</sup> It is not that Hermite denied the existence of such curves. As a reading of the whole passage in which this remark occurs, it was not that they exist which bothered him, but that people found them an interesting topic of research. In his view, there were more important things to do, mostly in the realm of complex analysis. Every mathematician has to decide for himself what to study, and some, in leading positions in the field, must decide for the profession as a whole. But Hermite's strong language is more consistent with an active dislike of the topic than merely finding it trivial, or likely to be a dead-end, or otherwise irrelevant to serious mathematics. And since it concerns a crucial point in real analysis, one may feel that Hermite's strong dislike for an existing topic contained an element of anxiety.

The realization spread steadily among mathematicians that their intuitions about curves applied only to differentiable curves, and not, as they had always thought, to continuous ones. So Wilson in 1905 could write plausibly and at length on the collapse of this intuition, observing in effect not only that mathematicians had been wrong all along but that with their new-found clarity on the issue there was no chance

<sup>13</sup> For an unusually informative account of these debates, see Epple's essay in Jahnke 1999.

<sup>14</sup> But he did not say, as is often alleged, that "Set theory is a disease from which later generations will say we have recovered" (see Gray 1991).

<sup>15</sup> Hermite to Stieltjes (in Hermite and Stieltjes 1905, vol. 2, 318).

of restoring the status quo ante. At the very least, rigorous mathematics had to be conducted according to much more stringent rules. The foundations of real analysis were no longer certain, and the capacity to match intuitive and formal reasoning had been thrown into doubt.

Complex analysis has not received as much attention from historians of mathematics as real analysis, which says more about historians than the nineteenth century.<sup>16</sup> In fact, complex analysis is one of the major success stories of the century, but it too was not without its problems. Fundamental to any approach derived from Cauchy or Riemann was the Cauchy integral theorem, relating the sum of residues inside a contour to the value of an integral taken round the contour. But as Jordan observed, this requires that one can distinguish the inside and outside of a contour, a task he admitted he had not been able to do precisely. A long debate then ensued concerning the meaning and validity of the Cauchy integral theorem for arbitrary contours. The highly counter-intuitive space-filling curve of Peano, and its refinements due to E. H. Moore and Hilbert have an extra significance when placed in this context.

The Cauchy integral theorem was not a problem for close adherents of the Weierstrass school, because Weierstrass eschewed the use of integral methods whenever possible in favor of power series ones. As Pringsheim pointed out, this was particularly awkward when it came to the Laurent Theorem, and Weierstrass had to fudge this point for many years before Scheefers and Mittag-Leffler independently found proofs of the Laurent Theorem that did not involve the Cauchy integral theorem.<sup>17</sup> But whatever anxiety they thereby avoided, Weierstrass' strictures against Riemann's ideas, in particular his view that defining a complex function by the Cauchy-Riemann equations was dangerously imprecise, did so much to polarize the community of German function theorists that a unified approach to subject largely free of error had to wait for the twentieth century.

The case of complex analysis enables a useful distinction to be drawn. It is clear that almost anyone drawn to the subject hoped for a theory of several complex variables to match the single-variable theory, if only to illuminate the formidable subject of Abelian functions. This was at the heart of Weierstrass' whole approach, and accounts for his emphasis on algebraic methods (as Umberto Bottazzini has argued to me privately) but the view was widespread. Yet several complex variables held their secrets, and a general theory was not to be created until well into the twentieth century. Such a failure is not necessarily anxiety-provoking, and I do not suggest that it was in this case. But the process of solving a problem only to fall into worse disarray, then to solve that problem but only at a still-higher price, that can cause anxiety, and I have suggested that it did, not only in a wide variety of fields but generally. There were simply too many cases for the mathematician to believe in business as usual.

<sup>16</sup> There is to be a book by Bottazzini and Gray.

<sup>17</sup> See Pringsheim 1896 for a discussion and further references.

Nor do I claim that anxiety was the only response, or that it was disabling; merely that it was there, and it is worth documenting. But just as one can see mathematicians in the eighteenth century engaged in a long struggle over the foundations of the calculus, one that they never resolved, one can see mathematicians of the late nineteenth century engaged in a similar soul-searching about mathematics as a whole.<sup>18</sup>

This being the case, one can ask why this soul-searching became acute as the century drew to a close. One answer is almost philosophical. By 1900, the nature of mathematics objects was contentious and obscure. The program of the arithmetization of analysis broke the naïve identification of mathematical with physical objects, which could no longer be carried out by some unproblematic task of abstraction. Geometry was no longer true in any simple sense, and was increasingly abstract in its formulation. To these ontological shifts can be added a related epistemological shift, as the nature of proof and the relation of mathematics to logic became a matter of animated research.<sup>19</sup> One might well suppose that these changes in the very essence of mathematics, none of which were fully worked through and generally assimilated in 1900, were anxiety-provoking enough. But to them can be added some points of a sociological nature.

The growing separation of mathematics from physics was quite marked by 1900.<sup>20</sup> In many universities across Europe there were separate departments for mathematics and physics, there were separate journals, and recognizable divisions between the subjects and between their practitioners. This weakened mathematicians' ability to rest their subject on the security (such as it was!) of science. The more critical mathematicians were aware that they therefore had to base their claims for the quality and value of mathematics on more intrinsic grounds. This raised the stakes. One traditional argument for mathematics — that it is a species of pure, abstract, science — was weakened, and the new discipline of physics could compete very favorably with mathematics on utilitarian grounds. When academic turf wars were to be fought, as they were when Klein sought to enlarge Göttingen in competition with Berlin, or when American universities expanded, or when times were harder, as they were in the 1890s in Germany, mathematicians knew that their arguments for their subject had to be re-cast.

In addition, the professionalization of mathematics also forced mathematicians to be more self-critical. A profession has a declared set of standards, to which its members are held, and which is open to public scrutiny. In principle, there is no hidden authority to whom appeal can be made, no governing body meeting in secret that decides who is, and who is not, a member of a profession. Someone who meets the criteria,

<sup>18</sup> Here perhaps I exaggerate, inasmuch as the astute reader may object that I have not argued for anxiety in the world of algebra, despite the introduction in the nineteenth century of so much of "modern algebra." But formal algebra has ever been the place where mathematicians feel most secure, finding, like most of us, that syntax is simpler than semantics.

<sup>19</sup> Most thoroughly described in Grattan-Guinness 2000.

<sup>20</sup> There is a large literature on this, see the introductory essay to Gray 1999 and the papers cited there.



perhaps by passing a public examination, and who continues to remain competent, is in, and all others are out. To be sure, in the case of mathematics it is only other mathematicians who are capable of making such judgments, nor did the various national mathematical societies as they came into existence in the nineteenth century, police their members. But the fact remained that a research mathematician was only as good as his (or, sometimes, her) arguments as they were set out in journals and books. The failure of mathematicians to live up to these standards was in theory open and public, however much a matter of likely public indifference. But part of the burden of being a professional is to care about these standards even without the threat of public sanction, and some of the publicly fought debates were provoked by just such a sense of responsibility. In addition to which, the great popular literature on mathematics around 1900 is evidence that in fact the educated public was interested in some of the problems that also animated mathematicians (see Gray 1999, 58–83).

Two examples may help make more precise the way in which this anxiety was manifested. In 1891, the distinguished German mathematician Leopold Kronecker gave what would prove to be his last lecture course at the University of Berlin.<sup>21</sup> The course was entitled “On the concept of number in mathematics,” deliberately, Kronecker explained, so as not to engage with matters of philosophy (Boniface and Schappacher 2001, 223). He distinguished three mathematical domains: mechanics, geometry, and the “so-called pure mathematics,” which he preferred to call arithmetic, noting that mechanics involved time, geometry involved space but not time, and that arithmetic was free of both these concepts and was more rigorous (*ibid.*, 227). The language of anxiety then crept in, when he said:

If one wants to define the idea of magnitude quite generally, so that it is also valid for geometry and mechanics, it becomes more and more blurred. . . . I am not of the opinion that the mixing of disciplines, and also the expressions, will bring trouble; I only believe that if one goes down to foundations an absolute separation is necessary. . . . But if one wants, for example, to discuss the idea of number, one must define it in its most precise sense, namely as *Anzahl*, and may not mix it with what originally does not lie within it. . . . If this is not the case then number becomes a battered coin whose impression can no longer be correctly recognized. (*Ibid.*, 231)

The message is clear: unless mathematicians retreat to the secure heartland of arithmetic, what they do will become contaminated and devalued; the core concepts of pure mathematics must be separated absolutely from the rest, even though they have been traditional domains of mathematics. Rigorous separation and a willingness to abandon part of what was hitherto one’s own but has become dangerous — the classic language of anxiety.

<sup>21</sup> The text has recently been published, with a commentary, as Boniface and Schappacher 2001.

Another example is offered by the distinguished Italian algebraic geometer and writer on scientific and philosophical issues, Federico Enriques.<sup>22</sup> He was originally close in his opinions to his friend G. Vailati, but they drifted apart over the issue of foundations. In 1911, Vailati had summed up the role of postulates in the modern axiomatic conception with an appropriate metaphor:

Postulates had to give up that sort of divine right which their assumed obviousness entitled them to, and they had to become the lowliest of the low, the mere servants of the associations of propositions which characterise the various branches of mathematics. (Vailati 1911, 690)

Enriques was hostile to this point of view, and in his address to the International Congress of Mathematicians held at Cambridge (England) in 1912 he replied:

An Aristophanes could object that the availability of unlimited choice risks turning democracy into demagogy; too often *dishonest* functions replace the simple but honest functions satisfying the theorems of infinitesimal calculus, some bizarre Geometries (justified initially as a means of investigating some relations of subordination) affirm the freedom of the inspiring idea in the same way as the governments which follow one from the other in the Principality of Monaco under the auspices of Rabagas.<sup>23</sup>

What Avellone et al. charitably call “baroque exaggerations” is highly charged, emotional language: democracy will decay into demagogy, simple honesty give way to dishonesty, good government collapse into parody. Enriques continued:

The history of mathematics has shown the work of logical criticism in the reworking of notions over the centuries. Contrary to the consequences that would follow from a view of postulates as implicit definitions, history shows that the definitions of mathematical entities are not arbitrary because they are the outcome of a long process of acquisition and an effort revealing of some of the general aims of research. There is a *tradition* of problems and there is an *order* on the extensive and intensive progress of science, therefore there is a *subject matter of mathematics* that definitions aim to reflect. Hence the decisions of the mathematician are not different from those of the architect who lays the stones of a building according to a carefully arranged project. . . . The act of free will that the mathematician embarks upon in the formulation of problems, in the definition of concepts, or in the assumption of a hypothesis, is not arbitrary. It is the opportunity to get closer – from more than one perspective, and by *continuous approximations* – to some ideal of human thought, i.e. an *order and a harmony* that reflect its intimate laws. If this is the

<sup>22</sup> This information is taken from Avellone, Brigaglia, and Zappulla 2002 (see pages 409–411).

<sup>23</sup> Rabagas was the central figure in Sardou’s play of that name, a satirical attack on Gambetta first performed in 1872. He was a radical demagogue won over to the Court’s party by the simple expedient of being invited to dine at the Palace. His name became synonymous with lack of principle. Interestingly, one of the terms hurled at Mussolini in 1914, when he was expelled from the Italian Socialist Party, was “Rabagas.”

view that emerges from an historical view of science, mathematical logical pragmatism will have given research a better understanding of its aims rather than a proliferation of bizarre constructions. Moreover, by purifying Logic it will have made clear its *inadequacy* and the need to investigate more in depth *the other psychological elements which give meaning and value to mathematics*. (Enriques 1912, 77; emphasis added)

Threatened by chaos and collapse, mathematicians may yet find security in tradition if they subject the arbitrary exercise of free will to natural ideals of order and harmony; purifying logic will show what needs to be done to give meaning and value to mathematics. Again we hear the language of anxiety urging a strategic withdrawal, keeping (re-defining?) the correct traditional values and rejecting new ones.

Kronecker's objections are those of a profound mathematician drawn, despite his prudent disclaimer, to a philosophical position of his own that acutely prefigured intuitionism. It would be wrong to see it as the nervous, even prejudiced, stance of an old man opposed to so much of what was new around him. But it would also be wrong to just read it as another statement of Kroneckerian finitism; the language is too resonant of a desire for purity and protection for that. The case of Enriques is more clearly that of someone who, despite an equal enthusiasm for philosophy, is simply opposed as a mathematician to some trends in contemporary mathematics. They do not strike him as harmless and self-limiting; as can be seen from his choice of dramatic political language, they worry him. Kronecker and Enriques shared a fundamental attachment to meanings in mathematics, and in very different ways and for different reasons saw mathematics as closely related to natural science. Kronecker explicitly compared mathematics to science in the fourth lecture (Boniface and Schappacher 2001, 232); Enriques defended the pursuit of mathematics because of its utility throughout his major philosophical work, his *Problemi della scienza* of 1906. By holding onto meaning, they belong to the group of mathematicians Mehrtens called the *Gegenmoderne*, and I think that it indeed likely that mathematicians prone to the anxiety I am calling attention to in this paper will lean to *Gegenmodernismus*; but there is no reason why anxiety should not strike the *Moderne* as well.<sup>24</sup>

#### *Perron's Antrittsrede, 1911*

A remarkable expression of this anxiety is to be found in Oskar Perron's *Antrittsrede*, given when he became a professor at Tübingen in 1911 (Perron 1911).<sup>25</sup> He began by observing that few non-mathematicians know what interests mathematicians, whereas the public knows rather more about science. Moreover, what the public knows about mathematics is often of no interest to the mathematician: he cited the problems of

<sup>24</sup> Mehrtens 1990. In keeping with the German focus of that book, Kronecker is discussed but not Enriques.

<sup>25</sup> If I may make a personal remark, I found this paper by accident after I had thought to argue for anxiety as a component of the mathematical life of the late nineteenth century.

trisecting the angle and squaring the circle, and observed that Fermat's last theorem had recently joined the ranks of popular problems because of the prize of 100,000 marks offered for its solution (it is not clear what importance Perron attached to the problem itself). But, he went on, even if one can debate the utility of mathematics, one cannot doubt its truth; "of all the sciences this is the one whose results are the most securely grounded." Or rather, that perception of the methods and results of mathematics was exactly what he proposed to deny: "This complete reliability of mathematics is an illusion, it does not exist, at least not unconditionally" (ibid., 197).

Perron began his attack by observing that confidence in old mathematics was misplaced. Euclid's *Elements* was now beset by critics finding fault with it; Newton, Leibniz and other early workers might have reached correct conclusions in the calculus but only by unconvincing methods. Nor was it any better with contemporary mathematics. "Indeed, there is one branch of mathematics today over which opinion is divided, and some consider right what others reject. This is the so-called set theory, in which the certainty of mathematical deduction seems to be becoming completely lost" (ibid., 198).

He then asked rhetorically how mathematics had fallen into such a state of error and doubt, and answered his question by considering the sorts of errors that could be found. Apart from straightforward errors of calculation, there were errors where ideas were poorly expressed. Often mistakes crept in where the matter was thought to be true "in general" (a term Perron thought was quite useless). Words, of course, can have multiple meanings, and it is necessary to be sure there is no slippage between them. This could be done by using a language from which multiple meanings were excluded, such as formulae, and here Perron noted the efforts of Peano to enrich mathematical notation. This can be done advantageously not only in the statement of results, but in definitions, for example, in the  $\varepsilon - \delta$  formulation of key concepts in analysis. Such steps had led, he said, to the disentangling of convergence and uniform convergence.<sup>26</sup>

Another class of mistakes arises from statements that are thought to be self-evident but may indeed be false, however attractive. Multiple limit processes are adduced as a case in point, where the exchange of their order is a common source of error. Another kind of error is often to be found in Steiner's work on the isoperimetric problem, where he assumed that there is a curve of a given length that contains the greatest area. Perron might have noted at this point that a similar error was often taken to be the fatal flaw in Riemann's use of the Dirichlet principle.<sup>27</sup>

Spatial intuition is a very frequent source of error, especially when it is used to supplant a proof, as, for example, in proofs of the intermediate value theorem.<sup>28</sup> "Intuition is a crude instrument that lets us make out true relationships only

<sup>26</sup> For a historical account of how entangled these were, see Lützen's essay in Jahnke 1999.

<sup>27</sup> A well-known refutation of this method of argument is this: if there is a largest integer, it is the integer 1. Proof: any integer  $n > 1$  has the property that  $n < n^2$ , and so cannot be the largest integer!

<sup>28</sup> The intermediate value theorem states that a continuous function on an interval which is positive at some point and negative at another is zero somewhere in between.

imprecisely” (ibid., 204), and this is particularly so of our understanding of curves, which may fail in all sorts of ways to have the intuitive properties one suspects.

Perron then gave two lengthy examples that I omit, and went on to declare that it was not, however, his intention to pronounce the death sentence on intuition, far from it. “Intuition and Imagination are the tools with which one *finds* new mathematical truths” (ibid., 207; emphasis in original). However, after potential truths have been found in this way it is absolutely necessary that they be exposed to the light of logic. The way forward will be difficult and dangerous, and yet, he said, it is very simple to avoid all the errors just mentioned. One has merely to keep clearly in mind all the definitions one is using and all the hypotheses one is making. In this way, in fact, “a degree of reliability has been reached in many partial domains of mathematics” (ibid., 208), but not in all, and certainly not in set theory.

The problem is that every mathematical theory must begin with undefined terms and results that cannot be proved but are taken as axioms and postulates. The question then arises as to the consistency of a system of axioms, for only a system of axioms free of contradiction can be taken as definitions. What can be done in this way for geometry, however, is not so easy for set theory. There is not, presently, a system of axioms known to be free of contradiction, but there is an idea (the set of all sets) that is known to engender contradictions. So one cannot be sure that the other basic ideas of set theory are not also flawed. The result is that at present “we do not always know if the methods of mathematics, the most exact of the exact sciences, are really exact” (ibid., 211).

To conclude his talk, and in keeping with the generally up-beat occasion of an inaugural address, Perron urged his audience not to despair. Some exactness has been obtained, the situation is much better than a hundred years ago, and we can hope that in the near future many problems will be solved, and we can console ourselves with the words “better than a known truth is the search for truth.”

This rhetorical exercise is not difficult to understand. Perron was not going to stand up and say, what in any case he did not believe, that mathematics was in a total mess. The purpose of the occasion was to demonstrate to a wide audience in the university and beyond, that he was worthy of his appointment, and that he had a sufficient command of his subject to lead it forward. As he delicately reminded his audience at the start of his talk, this is not as easy for a mathematician to do as a scientist. His own specialism, Diophantine approximation, might have served from the standpoint of its results, but certainly not from its methods, and indeed even the results can seem obscure and pointless to those outside the field. What then was he to do? It was quite usual on these occasions to find a philosophical perspective on mathematics, but what is striking is the critical tone of the lecture. It is unrelenting in its catalog of errors. The different types of error are carefully described, but the exactness of some domains is merely asserted. Perron did not assert that all mathematics reduces to set theory, but he gave the impression that it was not merely new but somehow fundamental, and very much in trouble.

It is striking that someone in Perron's position should stand up and point to the profusion of error in his subject. It is not the occasion for mere pedantry; plainly Perron felt that the matter was so grave that the scale of the problem was exactly what could and should interest his audience. And while he had a standard of mathematical practice that he felt mathematicians should be kept to, he offered no remedy, and his remarks about set theory, prominently placed near the start and finish of his talk, were such as to suggest the problems there might be insoluble. It is reminiscent of the situation that developed in quantum mechanics, where it became common to talk about the failure of classical physics. But even then there was a more precise description of the way forward. What Perron felt the need to do was to spell out the pervasiveness of error in mathematics, which went far beyond simple slips of the pen to loose reasoning, ambiguous terms, failure to be aware of one's presuppositions, and undue reliance on intuition as a source of proof. In setting out his view of the present state of mathematics in this way he plainly thought it was a matter of some concern that the subject was as he found it to be.

Enough has been said in this paper to indicate that mathematicians who heard Perron's talk or read it in the pages of the *Jahrsbericht den Deutschen mathematiker Vereinigung* would have had to agree with its criticisms in the main. That there were many mistakes of the kinds he described, from the routine lapses in calculations to the more damaging failures to be scrupulous about one's assumptions and the uncritical but misplaced reliance on intuition was common enough. It might be that Perron, by taking the side of logic against intuition, was at least implicitly opposing Felix Klein, who had contrasted the logical, formal, and intuitive styles of mathematics in his Evanston Colloquium lectures in 1893, to the advantage of intuition, and had sought to steer the arithmetization of analysis away from a total adherence to Weierstrassian rigor in his lecture in Vienna in 1895. Klein's own mathematics always inclined to the sort of imprecision that Perron was adamantly against. Even if Perron had not had Klein in mind, Klein's example was the kind of thing he was against.

The example of Klein enables us to distinguish the routine attitude to mistakes from the more elevated, anxious kind I am trying to point to. No mathematician tolerates a mistake once it is pointed out. But they differ over the extent to which they are prepared to tolerate mistakes in pursuit of discoveries, and the extent to which they think mistakes are harmless and will be corrected. Klein was undoubtedly more prone to mistakes than Perron, and more likely to regard them as, ultimately, harmless. Perron, in making this public attack on the many errors of mathematicians, was much more concerned about them.

### 3. Some conclusions

If anxiety is admitted into the historian's lexicon, we may ask what it can do for us. One answer is simply that it was there, a fact of mathematical life in the period

that has not hitherto been noted on the scale that it actually operated. A second answer is that historians of mathematics have for some time wanted to break away from histories of mathematics which are surveys of problems, methods, and results (fine and necessary as such work is) and to reach out to historians of physics and to historians of philosophy. One way commonly adopted has been to look at the growth of the profession: the creation of departments and of universities and rivalries between them.<sup>29</sup> Mathematicians have been placed into schools, found to be developing a novel research ethos, to have visions, even competing visions. The point of such historical analyses is not merely that indeed such schools and rivalries were there, but that this dimension of the mathematical life enables historians to side-step the dry, rigorous, seemingly ahistorical accumulation of mathematical truths. It permits access to a world of motivation and aspiration, which can be personal, contingent and, in short, historical. The aspect of anxiety can augment this enterprise, where appropriate, by diluting the triumphalism of much historical writing with a hint of uncertainty and the risk of failure.

The third and final answer is that there is one famous case where a mathematical community did give way to genuine doubt and there was a real struggle for power fought over the nature of mathematics and of people's ability to know it. This is the famous foundational crisis in the 1920s, which ranged Brouwer against Hilbert and drew in many, mostly German, mathematicians.<sup>30</sup> If it can be shown that, before the First World War, there was steady drip of disquiet about mathematics, then the intensity of the crisis after the War becomes that much more explicable. The pre-War matters of concern were not, precisely, the ones that Brouwer or Weyl picked up. Brouwer in particular was concerned with the limitations he believed intrinsic to the human mind. But he focused his criticisms of mathematics on the nature of proof, and Hilbert replied with a remarkable analysis of proof. Their disagreements were not resolved by the victory of one over the other so much as the assimilation of Brouwer's point of view in the attenuated form of intuitionistic logic, and this shows that what was at stake was not a point of mathematics but a point about mathematics. It is easier to understand how much was apparently at stake in the foundational crisis if one sees that much of mathematics was already known to be insecure, and that attempts to make it secure led firmly into foundational questions.

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<sup>29</sup> For a good example of this approach, see Parshall and Rowe 1994.

<sup>30</sup> See, in a bustling literature, van Dalen 1990, van Dalen 1999, Hesselting 1999, and Mehrrens 1990.

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