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RESEARCH ARTICLE

Poincaré and counter-modernism

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Argument

It would have been easy for a less imaginative historian of mathematics than Herbert Mehrtens to have portrayed the work of Hilbert, Hausdorff, and other modernists as pioneers, and those who did not subscribe to their program as people who failed, were not good enough to make the turn, and were eventually and convincingly left behind. That he did not do so is not only because this would have been a shallow, selective view of the facts: it is incompatible with his Foucauldian approach to the relations between knowledge and power. Instead, he defined what I see as the most intriguing category of actor in his *Moderne—Sprache—Mathematik* (1990), the *Gegenmoderner,* or counter-moderns. The three men who characterize this position are Felix Klein, Henri Poincaré, and Luitzen Brouwer, and each merits a section in the book.

Of the three, Poincaré is the hardest to contain within that category. The range of his work, the nature of his influence, and the shifting standards by which mathematical significance has been evaluated by mathematicians, historians of mathematics, and society at large, all contribute to the problem. After thirty years, the methodological presumptions and aspirations of historians of mathematics have also changed, and I shall suggest that one way to appreciate the richness of Mehrtens' book, to gain insight into what is meant by mathematical modernism, and to acknowledge a generation of work by other historians since 1990, is to re-examine aspects of Poincaré's life and work and scholarship about him. Prodded by remarks by Leo Corry, Moritz Epple, and David Rowe, I shall suggest that the simple but useful dichotomy modern/counter-modern must be seen as a way into a more complicated situation, one in which different aspects of mathematics, specifically applied mathematics and the relationship of mathematics to contemporary physics, require fresh accounts of the role of modern mathematics in society.

Keywords: Modernism; pure mathematics; applied mathematics; Poincaré

Moderne—Sprache—Mathematik

When I first read *Moderne—Sprache—Mathematik* (henceforth, *MSM*) what excited me was the place Mehrtens had found from which to tell his story. Many histories of one or another part of mathematics are straightforward histories of ideas. A theorem, an idea, a body of work is discussed within its mathematical context: what led up to it, how and why it was received the way it was. There might be a nod towards some social context, but this was scene-setting, not an integral part of the account. There were attempts to integrate the social context into the history, some by Mehrtens himself (see Mehrtens, Bos, and Schneider 1981) but these were often shallow.

Indeed, it is not easy to see why Newton, in his isolation, would invent the calculus for reasons that significantly depend on the social world of his time and his place in it, or Euler revive number theory, or any one of a number of mathematical events of historical significance. In fact, I was writing such "internal" histories myself, and to this day I have no quarrel with this way of writing the history of mathematics. But in *MSM* Mehrtens offered something different, deeper, and altogether more sophisticated: he promised (Chapter 6, 403) not to take the rationality of mathematics as self-evident, good, and assumed forever, but to seek to interpret it using discourse analysis, semiotics, linguistic theory mixed with psychoanalysis (Lyotard and Lacan), and ideology theory.

Not all of this heady mix of post-Marxist theorizing, this Foucauldian account of power and its deployment, of how mathematical language is used, and even becomes the essence of mathematics appealed to me. Some of it is an over-heated bubble, but it promised to provide a position outside mathematics from which its historical development could be understood. It also pointed towards the dangerous question of the extent to which any position about the nature of mathematics might be amenable to Nazi ideology and play its part in the horrors to come, which was known to be what Mehrtens hoped to investigate in more detail (see the essays by Reinhard Siegmund-Schultze and Mark Walker in this volume). I knew already, as Mehrtens made clear, that in his Nazi phase Bieberbach tried to make his ideas about types of mathematics sound like things that Felix Klein had said two generations before, but was it possible that in some ways some kinds of mathematics were reactionary in nature? This was not sociology of mathematics as often proposed, in which economic needs brought forth mathematical techniques, but a proposal to rethink what mathematics is as an ideology.

In this context, the counter-moderns in *MSM* are crucial. They provide the dialectic of the book. Without them it could seem that advanced mathematics moved into a new level of abstraction by some inevitable process, albeit one that several able mathematicians found difficult to accept: as the foundations of mathematics were laid in set theory, some people objected, and there was a crisis in the years on either side of the first world war, until somehow the heat was taken out of it and "modern mathematics" won. But the counter-moderns articulated various versions of a mathematics grounded in intuition (more naïve than Kantian), which gave it meaning, and without which it was—for them—an empty game of symbols. For them, as it had been for most if not all mathematicians before them, mathematics was about something, a something that it captured and refined. The moderns wished to denude mathematics of meaning, and this seemed to the counter-moderns to be a threat both to the individual mathematician and to the subject itself. Mehrtens' *MSM* offered to explain this in a way that tied it to other contemporary shifts and fractures our perception of ourselves, our place in the world, how we understand and talk about it, and who gets to determine the rules.

Eighteen years after *MSM* came out, I published *Plato's Ghost*, which more or less opens with a statement of where I agreed with Mehrtens and where I did not. We largely agreed over what defined modernist mathematics. What makes something modernist, I thought, was that it belonged to an autonomous body of ideas, having little or no outward reference, placing considerable emphasis on formal aspects of the work and maintaining a complicated (indeed, anxious) rather than a naïve relationship with the day-to-day world. This had also been the view of a specifiable group of people, who were very serious about it.

For Mehrtens, it was more complicated—pages of *MSM* are given over to analyzing and redefining terms—but ultimately much the same. The modernists, he said, saw mathematics as a "free creative art" (13), and he interpreted this as the expression of the professional autonomy of a scientific discipline inside a social system for which the legitimation of a higher order is no longer necessary. Modernist mathematics was written in a formal language that was self-sufficient, and was to be analyzed as such, thus: "The mathematical language is one of written texts. As such, it is worked out in its modernity" (9).[1] Hilbert, to whom Mehrtens often referred as the General Director (e.g. on page 13), and the people around him in Göttingen, were certainly the serious leaders of the modernist forces (to use another of Herbert's terms, this one echoing some of Poincaré's remarks). What is largely absent from *MSM* is attention to what the modernist mathematicians made of applied mathematical questions. Mehrtens saw these as remaining the province of the counter-moderns.

[1]"Die Sprache Mathematik ist eine der schriftlichen Texte. Als solche wird sie mit ihrer Moderne erarbeitet".

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As for disagreements, in brief, the largest was because I was dissatisfied with the socio-political dimension. I could not make the Marxist language of markets and forces and means of production feel other than grafted on, and Foucauldian power games were no longer so attractive. Nor could I find much that was rigorous in the use of the term "modernity" or the "modern." Many writers used it in many overlapping and often conflicting ways, with or without the same ingredients. I was willing to agree with Mehrtens that there were moderns and counter-moderns, even the same ones, but the cultural approach that made these terms resonant was not for me, and so I dropped the terms. Mathematics, it seemed to me, might actually be as loosely tied to social developments as most people took it to be, which it to say that it is what you see on the surface: once you have airplanes, you have theorists of flight, but why anyone looks for higher reciprocity laws and can make a comfortable secure living doing so is mostly explained by looking at previous mathematics, and to a limited extent at the place of universities in modern societies.

I now think I underestimated the fundamental difference between us. I wanted simply to write a book about the history of mathematics at a time when it seemed to be changing its nature. Mehrtens wanted to analyze that change as part of larger change that might be called the creation of modernity, with specific reference to finding the place of mathematics in changes that led in Germany to the Nazi time. People were asking, some with relish, whether Enlightenment ideas were not themselves part of the problem, and whether seeing the rise of the Nazis as a failure of the Enlightenment in Germany was simply a naïve, misleading disjunction. Could mathematics, the epitome of rational thought, be another damaged component of the intellectual world? The final chapter of *MSM* has some of this flavor.

There are smaller differences between our two histories. For example, Mehrtens focused more of his attention than I did on the so-called foundational crisis of mathematics after the first world war, and indeed he leaned more towards the 1930s and I to the 1890s. But we both agreed that there had been a profound shift in the nature of mathematics. Much of it was modernist in ways that resembled what was modernist about contemporary music, painting, and novels. So I set out to see in what ways that was true, and—to the extent that it was—how it had come about, and what were the implications.

The unity I found was in a broadening and disturbing sense of failure across all the different domains of mathematics. Each well-established subject became dubious, then rigorized, then that rigorization too became questionable. Mathematicians had kicked away the props of intuition, only to find that there was no security in the formalisms in which they had put their trust instead. It was the rough synchrony of these endeavors, their emerging commonalities, and the anxieties that this provoked that attracted me and seemed to reflect something of what had been going on. Moreover, these anxieties afflicted modernist mathematicians, not (or not just) the counter-moderns.

All the same, both Mehrtens and I said what seems to have become accepted, by historians of mathematics and even by some mathematicians who care about how students perceive the strangely abstract nature of modern mathematics: that seeing changes in mathematics around 1900 as a significant transformation of nineteenth-century mathematics, and the resulting newer mathematics as a species of modernism, is a productive way to proceed. From that perspective, it originally seemed to me that there was nothing to be gained by writing about Banach, or Emmy Noether, or much else of the pure mathematics of the 1920s and 1930s. By then, the struggle to establish modernist mathematics had been won, and the importance of the later work is not in bringing about a transformation, but in exploiting it. Now, I wish I had said more about Emmy Noether, and especially about van der Waerden and his *Moderne Algebra*. Happily, Corry (2007) soon showed exactly how different and modern that book was from its predecessors, and how much that mattered. But my judgement at the time was that the case for modernism in mathematics had been made and it needed no more evidence.

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What constitutes pure and applied mathematics?

There are many definitions, and even book-length accounts, of what mathematics is, ranging from the definitions of philosophers and logicians (for a new, full-length account see Paseau's forthcoming book) to those of the philosophers of mathematical practice (see, e.g., Mancosu 2008). As for the division into what the pure and applied varieties are, accounts are even more varied. Some authors and mathematicians even deny that the division makes sense. However, for present purposes we can note that most practicing mathematicians in the period with which this essay is concerned knew which journals to send their papers to, not least because leading mathematicians in and around Göttingen promoted what might be called a strong definition of pure mathematics, and traced its lineage back through certain favored predecessors. As I discuss below, this is the view that mathematics (pure mathematics) is about abstract structures, axiomatically defined, bearing at most a complicated relationship to familiar objects in the day-to-day world. By contrast, the difficult mathematics of, say, electromagnetic theory, was "applied mathematics," even if aimed at solving a theoretical problem without a technological application in sight.

This crude distinction, insofar as it was made, was more apparent in Germany than, say, France, and this is one reason why Poincaré matters. In a relatively short working life of some thirty years, he innovated hugely in a number of different domains, from novel geometries to celestial mechanics. He was not known for his clarity or his rigor, and he did not have much to do with axiomatic, formal structures, and this makes him seem a counter-modern, the obvious foil to Hilbert—arguably a French foil to the German Hilbert. But does simply not working with axioms make a mathematician a counter-modern? Much of mathematics at any time in the last two hundred years, especially in France, is mathematical analysis, the mathematics of the rigorized calculus. Much of what Poincaré wrote can be classified as mathematical analysis, and much of it is applied mathematics. This makes him the natural candidate to test out Mehrtens' modern/counter-modern dichotomy, and I will suggest that it breaks down here and fails, therefore, to be a productive way for historians of mathematics to deal with mathematical analysis.

A critique of mathematical modernism

What the modernist thesis needed, and still does, is opposition. Even if you grant that the dominant form of advanced, research mathematics became modernist mathematics, does this mean that all mathematics went that way? Mehrtens' answer was a guarded "No"; the counter-moderns helped develop mathematics in a dialectical interchange with the modernists, and retained a considerable influence over school and technical education. I was less certain, although also much less explicit about my worries. Perhaps they came later.

Let us start with what is missing. I found it disappointing that much of what Mehrtens used as evidence was the polemical, reflective, or perhaps philosophical papers of his protagonists (I used those papers too). This was, as he said, partly his way of reaching the audience he wanted to reach, who were not all trained mathematicians, and partly because the years around 1900 were an exceptional time for such debates. These papers figure in his distinction between the language of mathematics and what mathematicians say (hence the "Sprache" in *MSM*). What this overshadowed was the mathematics itself. The subject has its own dynamics, and, as Corry was to point out in various lectures, mathematics is constrained in a way most subjects are not by it being almost impossible to publish something false (unlike, say, the world of politics, where the uttering of falsehoods appears much less constrained).

My own book lacks sufficient attention to the applications of mathematics at the time and its links to physics, which I now think would merit a book, and more attention to advanced analysis. Could it be that each of our theses about the rise of modernist mathematics is skewed by a then-fashionable elision of pure mathematics and mathematics, the very ideology that was supposedly

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promoted by the modernists (and was certainly dominant in university syllabuses from the 1960s to the end of the century). At its worst, could what we did be interpreted as taking a subset of mathematics for the whole and then writing over a thousand pages between us on how (pure) mathematics became pure mathematics?

I think *Plato's Ghost* is an adequate rebuttal of the latter charge. It took a broad sweep through late nineteenth-century mathematics and found modernist transformations everywhere. Mehrtens' modernists and mine were formalists, algebraists, set theorists: Hilbert, Cantor, Zermelo, Hausdorff. We presented them as pushing for the abstract, set-theoretic language of mathematics, within which numerous formal systems could be built with abstract rules (groups, rings, fields, topological spaces). Plainly, ordinary intuition has no place in this version of mathematics. My broader look at the mathematicians most active in the years around 1900 supported that picture, amplified it with some remarks about changes in mathematical analysis, and took it outside Germany to much of mathematical Europe.

The best places to test the modernist thesis and see if it is skewed are mathematical analysis and applied mathematics, but it will be helpful to begin with a brief discussion of the problems involved in discussing interactions of modern mathematics and physics. Cantor, Zermelo, and Hausdorff surely are straight-forward modernists. But as Corry was to show in his *David Hilbert and the Axiomatization of Physics* (2004), Hilbert also took physics very seriously, calling in his sixth Paris problem for the axiomatization of successive branches as they reach something like a stable state, and, in 1915, becoming particularly involved in a way of presenting a theory of general relativity (in a competition with Einstein that strained their relationship for a while). This does not refute Mehrtens' analysis, but it raises the question of how to comprehend modernism in applied mathematical contexts, which neither he nor I discussed sufficiently. On the one hand, it seems that Carathéodory's attempts in his paper (1909) to axiomatize thermodynamics, in line with the sixth Paris problem, failed to attract much attention among practicing physicists. On the other hand, it is also worth noting that a disdain for applicable mathematics and a resolute attachment to pure mathematics does not make one a modernist mathematician. L.E.J. Brouwer was prepared to stick with his intuitionism, whereas Weyl abandoned his with much regret, because it seemed that mathematical physics was out of reach for the intuitionist.

The problem is deeper. Modern physics begins with two revolutions: relativity (special and general) and cosmology, and quantum mechanics.[2] Whatever else, the word "modern" in that context does not simply mean recent; it stands for a huge conceptual shift in our understanding of the world. So although it would be simplistic to say Hilbert's ideas were modernist just because they are about a central topic of modern physics, it would be equally unreasonable to dismiss them as counter-modern just because they are written in the language of analysis. Rather, we have to engage with that language and see if it is modernist in some definable sense or not. And this is where the analytical historian's job gets hard.

Mathematical modernism is about a change in mathematics from a naïve abstractionism to a formalized self-sufficiency, but the "modern" in modern physics is the realization that physics can only be about the world as it is seen. In quantum mechanics this runs from Heisenberg's uncertainly principle to the Copenhagen interpretation; in relativity it is the insistence on what observers and their frames detect, and the associated invariants.[3] This permits the interpretation of empirical results in the terms of the relevant mathematical theory.

The mathematical and physical modernisms are different, although it is important to note that both modernist mathematics and modern science are about talk and how we talk. That said, it is worth remarking that the mathematics of mathematical physics certainly did become modern

[2]It is true that general relativity and cosmology took longer than quantum mechanics to enter mainstream physics, but the conceptual seeds were planted and then cultivated by a significant minority of mathematical physicists. See Lalli et al. 2020.

[3]I do not mean to imply that everybody accepted the Copenhagen interpretation when it was proposed, still less that everyone does so today.

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mathematics, namely von Neumann's axiomatic presentation of quantum mechanics and the less axiomatized but equally heavily mathematical treatment of Dirac in the 1920s and '30s. For the theory of general relativity, we have the axiomatic accounts of Hermann Weyl. I suspect that a case could be made that modernist mathematics invaded contemporary physics, but opponents would argue that, in the 1920s, leading physicists, even in Göttingen, were surprisingly unaware of what their mathematical colleagues were doing. Even if that often-encountered claim is exaggerated, the extent to which working physicists in fast-moving fields adopted modernist mathematics remains to be examined in detail and case by case.

Unfortunately, there are very few discussions of mathematical physics in the early twentieth century that get close to addressing modernism in mathematics. Two that do provide relevant material in detail are Kosmann-Schwarzbach (2004) on Emmy Noether's work on conservation laws and group actions, and Schneider (2011) on van der Waerden and the development of quantum mechanics. Any serious book that grapples with general relativity must deal with mathematics that goes back to Riemann, one of the founders of mathematical modernism.

If modern physics is modern in a different way from the way modernist mathematics is modern, what about the applied mathematics and physics of the years in which modernist mathematics was being worked out? Applied mathematics is a vague term, covering many things from the mathematical treatment of specific open problems in technology, through the writing up in rigorous mathematics of problems already solved, to mathematics done in the hope it will be useful. This includes aerodynamics, hydrodynamics, wired and wireless telegraphy, the transmission of sound, elasticity, and so much else it would be impossible to survey everything in a short paper. But almost all these domains involve one mathematical domain more than any other, and that is analysis. Indeed, many would argue that analysis is the core domain of mathematics itself, and perhaps the largest. So it is unfortunate that neither Mehrtens nor I paid sufficient attention to it. I did argue that Lebesgue's measure theory could be seen as a modernist invention, but I left aside anything to do with differential equations.

Let us, for present purposes, take it as enough that the language of applied mathematics is (or more accurately, was then) mathematical analysis, and reduce our question to: How did mathematical analysis fare in the production of modernist mathematics? Or, is a modernist thesis productive in thinking about mathematical analysis and its applications? These questions are not easy to answer.

One reason mathematical analysis is difficult to discuss is that mathematicians of all sorts can go a long way with an early nineteenth-century idea of real and complex numbers. As van der Waerden indicated in his *Moderne Algebra*, the real numbers belong in modernist mathematics as a fully axiomatized structure, and yet the axioms merely encode precisely one's prior, naïve beliefs about the real numbers, albeit in a way that minimizes one's assumptions about them and precludes any self-contradictions. Since advanced analysis is often a long way from its informal assumptions or its corresponding axioms, this makes it hard to see what is in play. Furthermore, rigorous convergence arguments depend on knowing that the real numbers form a complete, Archimedean, ordered field (modulo some decisions of a purely logical and set-theoretical kind), and Dieudonné was pleased to note in his *Foundations of Modern Analysis* (vol. 1, vi) that "in the proof of each statement, we rely exclusively on the axioms and on theorems already proved in the text," except for elementary linear algebra and the fundamental properties of (positive and negative) integers.

If this is "modern" in the sense Mehrtens or I intended, then so is Euclid's *Elements*—a truly absurd position. It follows that rigor is not an identifying component of modernist mathematics, only of mathematics—and possibly not even that if one admits heuristic arguments of various kinds in mathematics. Worse, a free-standing real analysis, not based on geometry or kinematics, that generates existence proofs of results hitherto taken for granted, finds fault with some accepted intuitive arguments, and proceeds without a hint of applications, is of course what Cauchy produced in the 1820s. To be sure, he left the nature of the real numbers unexplained and so his talk about infinitesimals was vague, and his own proofs were often poorly expressed, but to admit Cauchy as a modernist when he died in 1857 is to wreck the chronology that allows us to speak of modernist mathematics. (One can just about allow Riemann, who died in 1866, by analogy with Rimbaud, who stopped writing in the 1870s, but the case is awkward.) We need to define more precisely what modernist mathematical analysis might be before we can decide whether it existed, and if so when.

Here, in the context of this paper, the example of Poincaré is crucial. He is an important counter-modern in *MSM*, and I have tended to see him as divided: modernist in some ways, counter-modern in others. He is a modernist in that not only was he comfortable with non-Euclidean geometry, his geometrical conventionalism made pure geometry meaningless, and the way meanings could be introduced into the fundamental terms (straight line, angle) seemed to show that it was impossible to say when any geometry was true. He advocated something like point-set topology as the basis of any analysis of space, and was one of the prime creators of algebraic topology. That said, he seldom worked in the parts of mathematics the modernists preferred. He was not someone with much enthusiasm for algebra, he was famously not convinced by Zermelo's axiomatization of set theory, his conventionalism led him to reject Einstein's special relativity, and he may well not have liked the fascination with "mathematical monsters" that theorists of real functions were producing.

Two topics are now well documented and can help us proceed. The first is Poincaré's work on flows on surfaces, later extended to celestial mechanics and the three-body problem. The second is his work on partial differential equations.

His work on flows on surfaces was carried out in the early 1880s, and done with an eye on questions in celestial mechanics. As Poincaré knew very well, much work in celestial mechanics is the laborious handling of long power series in several variables. He introduced the ideas of studying the solutions to the equations of motions as curves on a surface, and looking at their long-term behavior. The surface was determined by the differential equation that defined the flow, and very quickly he extended his analysis from flows on a surface to flows on a surface of arbitrary genus. All this was highly novel, abstract, and not at all obviously applicable. That changed with his prize-winning essay on the restricted three-body problem in 1886, which made his name internationally. For a thorough history of this, including Poincaré's mistake, see Barrow-Green's *Poincaré and the Three-body Problem* (1997). Various three-body problems had long been the essence of celestial mechanics. Poincaré's paper, and later his books (1892, 1893, 1899) on celestial mechanics from his new point of view were unmistakably contributions to science. Furthermore, they built on a new approach discovered by the American astronomer G.W. Hill, and were later taken up by the astronomers George Darwin and F.R. Moulton. For a recent account, see Stephenson (2021).

But if all this shows Poincaré as a creator of applicable mathematics, nonetheless it was also adopted by Hadamard in his study of geodesics on surfaces of negative curvature to represent trajectories in a dynamical system. This drew on some of Cantor's ideas about uncountable sets, and gave rise to a theory of symbolic dynamics. This is an entirely abstract theory of flows, in no way restricted to Newtonian dynamics. Mention should also be made of the so-called Poincaré's last theorem and its proof by G.D. Birkhoff (1913), with its implications for the three-body problem. This is a largely topological problem.

Is this modernist or counter-modernist? Celestial mechanics until Poincaré had been a matter of trying to solve the coupled differential equations that Newton's laws of motion provide. Laplace had shown that these equations could solve all the problems presented by the solar system, with the possible exception of the motion of the planet Mercury, and others had shown that double stars in the nearby parts of the galaxy also obeyed these laws. It was also possible, although extremely laborious, to produce almanacs that could predict such things as the motion of the Moon up to a year ahead. Flows on an abstract surface, interpreting the equations of celestial mechanics as defining a flow in a three-dimensional space that was not Euclidean space (Weierstrass thought this would prove hopelessly confusing)—why is this not modernist?

If it is not modernist, it must be because it depends on the crucial terms, retaining, even relying upon, their meaning, and some naivety about the relationship of mathematics to physics. It must not simply be a language with its own rules, but must acknowledge the higher authority of the language of applied science. I suggest that it was indeed a self-sufficient discipline, in Hadamard and Birkhoff's hands if not in Poincaré's, and so it is not only helpful to ask if dynamics became modernized, but to grasp that the conclusion is that it did.

Poincaré was not the most precise of mathematicians. His contemporaries often found him hard, even too hard, to understand as a result. He was not one for axioms and a painstaking reliance on them when elaborating a theory, and for this reason his reputation was low in the heyday of Bourbaki, and rose only as their influence declined. On many occasions, he wrote with a view to imparting a way of thinking, as in his famous papers on three-dimensional topology, and he was unhappy that Hilbert's abstract geometries could not draw on intuition and experience. But he did not dispute the validity of those geometries, and he had no problem greatly increasing the level of abstraction in celestial mechanics, to the point that it spun off an independent, abstract mathematical subdiscipline concerned with flows and geodesics on manifolds. His concept of a manifold was not the modern, rigorous one any more than Riemann's was—it relied on one's naïve intuition about regions of R^3—but equally it was an extraordinary generalization of the concept of space, from three-dimensional Euclidean space to a wide variety of hitherto unknown objects.

If we now turn to look at Poincaré's work on partial differential equations (PDEs) my conclusion will be somewhat different. Second-order linear PDEs, as Poincaré noted in his article (1890), account for almost all the PDEs of interest in physics, and they share the property of being difficult to solve rigorously. The subdivision into elliptic, parabolic, and hyperbolic was only made in print by du Bois-Reymond in 1889, although it is hard to believe it was not appreciated long before (a case can be made for claiming that it was known to Euler in the 1750s). The wave equation, the prototypical hyperbolic PDE, was solved in the eighteenth century and generated a rich debate about what counts as a solution, but the first rigorous, general solution of the Dirichlet problem is due to Schwarz (1870). Existence theorems for hyperbolic and elliptic PDEs with first-order terms are due to Picard in his articles (1890) and (1893). As late as 1923, Hadamard was complaining that mathematicians had a poor understanding of the difference between boundary and initial conditions, and had hitherto confined their attention to PDEs with analytic coefficients.

Poincaré's contributions to this field were many, but we may concentrate on two. The first is his simple call for rigor, a matter for the physicist as much as the mathematician, accompanied by a new rigorous method of his own, called *balayage* or sweeping out, for the Dirichlet problem that, like all other methods of the day, was useless for finding approximate solutions (Poincaré 1890). As we have seen, that alone does not make him a modernist, although it does indicate his dissatisfaction with physicists' handling of partial differential equations. The second is his exploitation of eigenvalue methods to the Dirichlet problem, and the problem of the vibrating membrane that had been introduced by Weber and used to good effect by Schwarz (for a lucid and accurate account, see Dieudonné 1981 and the references there). Poincaré's contributions included several analytical insights and, more important here, a study not only of the first or lowest eigenvalue but of the infinite collection of eigenvalues.

Less than a decade after Poincaré had done this work in the 1890s, and inspired by the contributions of Fredholm (1903), who knew Poincaré's work very well, Hilbert took up the subject—the last five of his twenty-three Paris problems are problems in analysis. Hilbert's results are more rigorous, they mark a decisive turn to functional analysis, and they rapidly led to what have since been called "Hilbert spaces."

To account for this breakthrough, Dieudonné pointed to the contemporary confluence of geometry, topology, and analysis and "the emergence of a new concept in mathematics, the concept of *structure*" (Dieudonné 1981, 115, emphasis in original). Both Mehrtens and I underestimated the force of this simple observation. I now regard it as a key to attempting to test out the concept of mathematical modernism. Functional analysis involves the extension of linear algebra and Euclidean geometry to spaces of infinite dimension. It is highly abstract and general, and seemed to offer, in Fréchet's article (1906), a variety of topological tools for practicing analysis on abstract sets. As such, it was several steps removed from the study of individual PDEs, and is a good candidate for a modernist turn in PDE theory.

If we accept that, then plainly Poincaré's contributions to PDE theory, insightful as they are, are not modernist. They are too tied to specific equations. But that does not make him, on this account, a counter-modern, rather a precursor of modernism in this field. For Mehrtens, Poincaré stands out as a counter-modern chiefly for his frequent hostility to the attempts to found mathematics on set theory and logic, at least as they were presented in his lifetime, and for his attempts to find some unity in scientific and moral truth. However, it is worth noting that he was an early supporter of Cantor and used his ideas in his own work on Fuchsian and Kleinian functions in the mid-1880s.

Was there a modernist mathematical analysis?

It may be helpful to set out three positions, two historical and one of Mehrtens'. Poincaré (in "Analysis and Physics," in *The Value of Science*, 1905) argued that mathematical analysis should be cultivated for its own sake, with theories that have not been applied to physics as well as others that are, and yet that the aesthetic aim of mathematics and the aims of science are inseparable. Analysis furnishes the only language the physicist can speak, physics furnished the mathematician with the idea of the continuum. Hilbert, in the preamble to his Paris Address, spoke of Fermat's last theorem and the three-body problem as seeming to be almost like opposite poles, the former a free invention of pure reason, the latter forced upon us by astronomy, but went on to claim that the outer world "forces upon us new questions from actual experience, opens up new branches of mathematics, and while we seek to conquer these new fields of knowledge for the realm of pure thought, we often find the answers to old unsolved problems and thus at the same advance most successfully the old theories" (Hilbert 1976 [1901], 3). On page 237 of *MSM*, Mehrtens set out the crucial distinction this way: The difference between moderns and counter-moderns comes to a head when the question is asked: Reality and eternal truth or freedom of design and freedom from contradictions?

This last distinction may seem very clear, and I certainly endorse it as a useful test. But like all such stark dichotomies, it is too easy and too precise. It is not that Hilbert also at times actively sought reality and eternal truth, for example in his program to axiomatize physics, or that Poincaré wanted mathematics for its own sake and subject only to be free of contradictions. The debate here is not to make a whole out of these remarkably diverse and yet different mathematicians and pronounce the one modernist, the other counter-modernist. Rather, it is to characterize certain types of mathematics in a widespread transformation of mathematics around, say, 1900, with a view to understanding the nature and significance of that change. Specifically, those parts of mathematics that are applied.

If we agree that a high degree of abstraction, a prolonged foundational concern with set theory and logic, and a desire to provide mathematics with an autonomy, even if that severely strained the traditional link to such domains as astronomy and physics—in short, a modernism—produced new algebra, new foundations, and a revival of logic, we are left with the question of what to say about mathematical analysis. Here, it seems to me that Dieudonné's indication that a structuralist mode of thought was being introduced is helpful. It is in line with what one sees in Dedekind's work in number theory, which is one of the crucial aspects of modernist mathematics, and the structuralist foundations of functional analysis are far too general to be tied to any specific problem in PDE theory. It is one of the ironies of the whole story that, to everyone's surprise, Hilbert space methods became the right mathematics for quantum mechanics some twenty years later.

It seems to me that Mehrtens may have loaded too much on the backs of his counter-moderns when he juxtaposed eternal truth and freedom from contradiction. I have thought at times of mathematics as meaningless, its axioms telling us how to use a concept but not what that concept is. I think that physics, which so often proceeds by changing its mind about what objects really are, is over-simplifying when it says certain things are true. Better to say "validly handled according to such-and-such a model." But the fact is, applied mathematics and physics are about finding truths about the world, at the very least in the pragmatic sense that practitioners will stake their careers on some claims, and spend sums of money investigating them that mathematicians can only dream about. How the putative truth of some claim in physics might import truth into mathematics was a long- running concern of Hermann Weyl, who found hope in the way the whole of physics and the whole of mathematics are related. Such matters are too complicated to be pursued here, but pursuing truth cannot be the hallmark of the counter-modern mathematician.

Conclusions

I conclude this paper by outlining how the idea that mathematics underwent a modernist transformation can be maintained when we consider how much applied mathematics was done and how intimately applicable mathematics remained part of mathematics, as Hilbert believed it did. For something to be modernist, there has to be a sense of autonomous foundations for the mathematics and a total reliance on its methods, which promoted an estrangement from reality as usually conceived.

This was the case with Riemann's vision of what geometry is, and Hilbert and his associates of what analysis in infinite dimensions is, and I would suggest that it is precisely the absence of any naïve association with reality that ultimately led to one being the right mathematics for general relativity and the other for quantum mechanics, the highly non-intuitive pillars of physics ever since.

It will be evident that my idea of mathematical modernism is still markedly more "internal" than that of Mehrtens. I am still not sure that the fashionable ideas of social criticism that Mehrtens used are the right tools for a social history of mathematics, but I remain attracted to the unfinished business of finding a place outside mathematics from which to analyze its history. I think, in fact, that much of advanced mathematics has been conducted in a freely floating world, precisely because it is so difficult for governments and other large organizations to successfully ask it to do things. I would be prepared to argue that not only, in some important ways, is the mathematics that its elite figures practice today a recognizable descendant of what Euler did, but so too is their relationship to the prevailing power structure (see Harris 2015). In particular, mathematics is both wanted and funded but little understood, and in that gap in comprehension lies the autonomy of mathematics to this day. It all depends on what is meant by mathematics.

Both Mehrtens and I circled round leading figures such as Hilbert and Poincaré. Analogously, today one might discuss the Langlands program. Or, one might discuss the much greater number of mathematicians engaged in the far greater number of applications these days than a hundred or more years ago, in which case it might be that the modernist perspective may not apply. That's fair enough: detective stories have surely vastly outsold James Joyce's *Ulysses*. However, the modern/counter-modern dichotomy itself is less easy to defend when applied mathematics is considered.

Every historian learns that the profusion of sources, and the gaps that nonetheless remain, speak to a multiplicity of activities in the past that have somehow been rendered comprehensible in a book or article, which may rely on previous work but surely also contests it in some way. Mehrtens' introduction of the modern/counter-modern dichotomy is a significant achievement in making it much easier to see what was going on in the period he discussed, and perhaps for approaching its socio-historical implications. Inevitably, it is the task of later historians of

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mathematics to restore complexity to the picture (see, for example, David Rowe's contribution to this special issue), until, if we are not careful, all is obscure again as if a jungle has reclaimed a once-tilled field. To some extent this depends on the aims of later historians, and even on their temperament (some luxuriate in detail, others prefer well-crafted generalizations).

The clearest thing that emerges from even a cursory look at mathematical analysis and its applications around 1900 is that it does not fit this dichotomy very well. Mehrtens and I could validly argue for the emergence of modernism in mathematics, and advances in mathematical analysis do not refute that—some indeed support it, such as the creation of Hilbert and Banach spaces. But that same analysis, when it is directed at applications, shows, I believe, that there was more going on than any dichotomy can encompass.

Afterword

There was, of course, a specific aspect of *MSM* that was vital to Mehrtens and which I did not seek to explore, and that was the way in which what he covered might be part of a larger story about the rise of the Nazis. My understanding at the time was that Mehrtens was blocked in pursuing that line of enquiry because too many archives remained closed, and would only be opened a generation later. But the situation was more complicated, as the essay by Reinhard Siegmund-Schultze in this volume shows. More was done in the next academic generation by Volker Remmert in his articles exploring the German mathematical community in the Nazi period, (1999) and (2004), but Mehrtens was also leaving the field of history of mathematics for some of the grander themes of post-modern intellectual life. A history of mathematics in the Nazi period was not something I could pursue, but equally important questions for our own time should not be dismissed. Only now, the appropriate target for critical investigation should be not mathematics but computer science. One of today's urgent issues—whether in the extensive surveillance and heavy social control underway in China, or in the more overtly monetizing information processing about all of us by Facebook and Google (see e.g., Zuboff 2019), or in the rise of AI and machine learning—is an analysis of the power, real social power, in and arising from the human use of what might seem as neutral as a lifeless, programmed machine.

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