The Architecture of Modern Mathematics
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REVIEWED BY ANDREW ARANA[1]

This collection of essays explores what makes modern mathematics 'modern', where 'modern mathematics' is understood as the mathematics done in the West from roughly 1800 to 1970. This is not the trivial matter of exploring what makes recent mathematics recent. The term 'modern' (or 'modernism') is used widely in the humanities to describe the era since about 1900, exemplified by Picasso or Kandinsky in the visual arts, Rilke or Pound in poetry, or Le Corbusier or Loos in architecture (a building by the latter graces the cover of this book's dust jacket).

Though it is hard to say precisely what modernism is, or what distinguishes it from other eras, Gray attempts a definition in his closing essay in this collection:

> Modernism can be defined as an autonomous body of ideas, pursued with little outward reference, maintaining a complicated, rather than a naive, relationship with the day-to-day world and drawn to the formal aspects of the discipline. (374)

This is a good start. Gray mentions modern algebra, topology, and logic as examples fitting this description, and explains why they fit. These characteristics, though, fit high-profile examples of mathematics before this era also. Ancient geometry as in Euclid's *Elements* seems to have been pursued as an autonomous body of ideas, at least as far as I understand what this means. D'Alembert's work on differential equations, for instance on the vibrating string problem, was criticized for failing to model empirical reality adequately, though d'Alembert disputed this: hence d'Alembert's work had a complicated, rather than naive, relationship with the day-to-day world. Lastly, Euler and Lagrange, among many others in the eighteenth century, were drawn to the formal aspects of analysis. Perhaps Gray's definition of modernism could be tightened to disqualify these examples. But if we are going to make a case for the continuity of modern mathematics with modernism, we must look beyond Gray's definition for another account.

I think we can explain how the essays in this collection contribute toward answering what makes modern mathematics modern if we instead view modernism as a crisis concerning foundations. Let me explain.

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Each of the artists and architects mentioned above took themselves to be finding a new way of practicing their art, because the old ways had been discredited or had ceased to speak to them. This loss opened up many new possibilities for their work. As a result, each experimented with form and content. The results were radically new, and many found it alien upon first contact. Think of people asking if Kandinsky's spirals of color are really 'art'. What would it be to *really* be art? Modern artists realized many answers to this question that would not have seemed open in earlier times. This sense of openness, in contrast to a time in which some options would have seemed inescapably 'correct', is what I want to call a foundational crisis.

As has often been remarked, mathematics in the early twentieth century underwent a foundational crisis. This is usually said to be a result of the paradoxes in set theory, which threw into question whether mathematics was consistent. I agree that this was a crisis of sorts, but there was another crisis contemporaneous with this one that had wider reach. This wider foundational crisis mirrors the crisis in art just discussed. There had been consensus in the past on what makes a mathematical theory such as arithmetic true: it was true if it described the way things really were. By the turn of the twentieth century, this view had lost much of its credibility. It now seemed open whether there were any true mathematical theories, and if so, there were a variety of possible answers to this question that had not seemed open in earlier times. As with art, I want to call this sense of openness in mathematics a foundational crisis. Viewing modernism as a foundational crisis in the sense described here gives a sharper answer to what makes modern mathematics modern. The modern turn in mathematics happens in parallel with the modern turn in the arts, both trying to progress despite an awareness that old orders which used to underwrite their ontologies and values had been discredited.

The essays in the collection under review address this modernist foundational crisis in mathematics in a variety of ways. These essays concern the years following 1800, when non-Euclidean geometries were beginning to receive attention. These geometries gave new urgency to the problem of what it was for a mathematical theory to be true. Which one is the real geometry of space? On this question, there had been consensus in the past: it was true if it described the way things really were. Until the mid-eighteenth century, there were two main explanations of how this worked: either this description of reality was a result of abstraction from nature, or an expression of Platonic forms and their ordering. On either view, Euclidean geometry was thought to be a true description of space.

During the eighteenth century, another explanation gained currency. Gray describes this view as follows: mathematics is "what is presented by idealized common-sense." (p. 390) On this view, "every rational person can recognize a straight line when they see one". The true geometry is thus the one acknowledged by all rational people; naturally this was thought to be Euclidean geometry. Around the turn of the nineteenth century, Kant offered a more sophisticated version of this view. He held, roughly, that the world appears to us the way it does (for instance, as having unified objects) because our minds structure it to appear that way. We can't help but experience the world as we do, but whether the world *really* is as we experience it is unanswerable. A theory of space is true, on this account, if it expresses the structure that our minds are constrained to experience space as having. (Kant seems to have thought that Euclidean geometry expressed this structure.)

During the nineteenth century, mathematicians became aware of alternatives to Euclidean geometry, known together as non-Euclidean geometries. In light of this new work, it neither seemed obvious that space really is Euclidean, nor that the mind structures spatial experience as Euclidean. The foundational crisis for mathematics that I described earlier begins here.

One option is to conclude that no geometry is the 'true' one. Instead, there are different geometries that express possible perspectives on space, and none of them is any more true than the others. The best we can say is that some geometries suit our individual situations and present purposes better than others, while noting that our situations and purposes can change. This view is quite similar to the philosopher Friedrich Nietzsche's 'perspectivalism' on moral and scientific matters. Nietzsche thought that there is no single 'true', 'God's eye view' of morality and the world, but rather only individual perspectives. Each individual perspective is biased, and to see things more clearly we should learn to view morality and the world from many different perspectives. As Moritz Epple explains in his fascinating essay in this volume, one mathematician who explicitly took up this Nietzschean banner in his work was Felix Hausdorff. Epple describes how Hausdorff lived a double life in print, publishing as a mathematician under the name Felix Hausdorff, and as a Nietzschean philosopher under the name Paul Mongré. Hausdorff's view, which he called "considered empiricism", was that mathematics is useful for constructing axiomatic theories that 'model' empirical phenomena. He thought of each theory as representing a 'perspective' on the empirical matter it concerned. Whether a theory is good is a practical question, to be evaluated based on how well the theory describes, explains, and predicts data. Since

the empirical data may be consistent with several different mathematical theories, each theory should continue to be developed; Hausdorff thought this was the case with the dimensionality of space, and saw his work on Hausdorff dimension in this way. He also viewed the ongoing development of various axiomatic geometries this way. None of these were absolutely true; each was merely a perspective to be adopted inasmuch as it suits our purposes.

Relativism regarding the truth of mathematical theories is a radical departure from what had been believed in the past. It constrains mathematical activity to the construction of theories, without any pretensions to 'getting it right'. On this view, there is no 'right' way to think of space—there are just different perspectives, expressed as different geometries. There is no single true analysis of the concepts of circle or line or continuity—there are just different definitions of these concepts that are to be adopted when useful and ignored otherwise.

Relativism can extend beyond geometry, even into the allegedly 'foundational' areas of arithmetic and set theory. The relativist concerning arithmetic says that there is no single true arithmetic. There are many axiomatic theories of arithmetic that we can study freely for their mathematical structure, but we must not confuse any of them with the 'real thing', for there isn't such a 'real' thing. There are just different theories of the natural numbers, to be adopted or rejected based on how well they suit our aspirations. Similarly, while many want to claim that set theory is what mathematics is 'about', there are many different set theories, and so the relativist can claim that none is absolutely true. As Epple explains, this is what Hausdorff did: he became interested in a set-theoretic approach to topology in seeking a continuous model of space and time, which led him to Cantor's point set analysis of the continuum. Yet he thought this analysis should not be taken as absolutely true, but just as a perspective on the continuum.

Alfred Tarski's work on logical consequence, nicely discussed in this collection by Paolo Mancosu, gave more tools to the technically-minded relativist. Tarski gave an analysis of logical consequence, that is, of when one sentence follows logically from another. Today we follow Tarski in saying that a sentence σ is a logical consequence of a set of sentences Σ if every interpretation or 'model' of all the sentences in Σ is also a model of σ. But the 'every' in this analysis of logical consequence raises a question. Suppose we are asking whether a sentence in the language of arithmetic is a logical consequence of the axioms of arithmetic. Should we consider just models with the intended domain $\mathbb{N}$ for arithmetic, or do we consider models with other domains also? This could be taken to bear on relativism: the more radical conception

is to vary domains widely, perhaps out of skepticism that the notion of an 'intended domain' makes any sense. Mancosu argues that in 1936 at least, Tarski avoided the more radical option. Against other recent readings, Mancosu argues that Tarski thought we should only consider models with a fixed universe in his watershed 1936 paper on logical consequence. I find Mancosu's argument convincing, but more work is being done on this topic and there may be new arguments worth considering. In any case, Mancosu's article is an excellent starting point for understanding this active area of research.

In his essay, Gray notes that an even more radical relativism arose in the early twentieth century, concerning logic itself. He writes

> The Modernist foundations of mathematics ultimately dispensed with the idea that the subject matter of logic was the correct rules of reason—those that would be followed by any undamaged mind. A part of logic does consider such rules, but it seemed ever more obvious that the logic needed to create genuine mathematics is not a candidate for even an idealized description of the way people think. Not only geometry, not only the conception of number, but eventually any simple-minded association of logic with correct thinking was made anew. (p. 396)

Hausdorff vigorously embraced what I am calling relativism, both within and without mathematics. Many others have found this view unattractive, and have sought to retain the traditional view that we can get right or wrong what a circle or continuity really is, or that there are true and false geometries. Is there a principled reason to reject relativism, and maintain the traditional view? Because this question is a live one, I call the ongoing situation a foundational crisis. Epple and Gray's essays explore (without advocating) this crisis directly, while Mancosu's essay bears on this issue without addressing it explicitly. I also see the other essays as responding to this view, in one way or another, as I will now explain.

David Hilbert was acutely aware of these philosophical matters, exploring the new geometries and refining their axiomatizations in his *Grundlagen der Geometrie*. He cannily declared his philosophical allegiance by beginning that text with a quote from Kant. Two of the essays in this collection concern, in different ways, Hilbert's attempt to reconcile a Kantian approach with these new mathematical developments. Wilfried Sieg's essay concerns ongoing developments in the

spirit of "Hilbert's program" in proof theory. Hilbert thought mathematical methods could be divided into two categories, the 'real' and the 'ideal'. Historically natural and real numbers, and points in ordinary diagrams, were thought of as 'real', while imaginary numbers and points at infinity in projective geometry were thought of as 'ideal'. At the time of the *Grundlagen* Hilbert was mostly concerned with this way of drawing this distinction. Later, he identified the finitary mathematics of the natural numbers as real and the infinitary methods of higher mathematics as ideal. In drawing a real/ideal distinction, Hilbert was echoing Kant's distinction between constitutive and regulative principles, where the former are realized in experience, whereas the latter are not but instead are tools for organizing our thoughts concerning experience. Theorems proved by real methods were contentual, while theorems proved by ideal methods were useful tools for theorizing, but lacked content. Hilbert hoped to show that every theorem provable by ideal methods could be proved by real methods—thus Hilbert's "program" of showing the consistency of ideal mathematics by using just real methods.

Sieg agrees with the received view that Hilbert's program is dead, as a result of Gödel's second incompleteness theorem. But he thinks ongoing work in proof theory might salvage something like Hilbert's program. This work shows that several formalized theories of classical mathematics, including many impredicative theories, can be proved consistent in (arguably) constructive theories such as intuitionistic number theory. This could salvage something like Hilbert's program, Sieg says, if the base theory is "accessible"—that is, if it has "a unique build-up through basic operations from distinguished objects", so that it consists of "principles that are evident". For then the base theory would be like Hilbert's finitary mathematics: contentual and thus capable of yielding knowledge. Sieg doesn't offer criteria for evaluating when an operation is basic, or for when a principle is evident. Instead, he raises this as a project for future work, but points out that proof theorists have many related results that would be worth further philosophical reflection.

Though there are important Kantian elements in Hilbert's thought, Hilbert rejected the details of Kant's views on intuition as "anthropomorphic garbage". Nevertheless, intuition and experience, and in particular visualization, played an important role in Hilbert's thought. In his essay in this collection, Leo Corry writes about Hilbert's views on the relation of experience to geometry. Corry explains that Hilbert believed that we are guided in our formulations of axiomatic theories of geometry by intuition and experience, and that Hilbert continued to

believe this even as he came to understand general relativity. While in the past he had thought Euclidean geometry was the true geometry of space, he recognized that general relativity cast doubt on this. This was no problem for Hilbert's views about axioms and experience, because as our experience changes, so should our axiomatic theories. Such a view, though, seems to leave open the question of whether mathematics can 'get it right' when describing the world, or if instead it is just our way of describing things, which can only be judged pragmatically by what those models can do for us. That is, Hilbert's view as described by Corry leaves open the possibility of relativism for geometry.

These matters troubled Hilbert's student Hermann Weyl, whose fascinating views Erhard Scholz nicely discusses in his essay in this collection. Like Hausdorff and Hilbert, Weyl thought mathematical activity was largely a matter of producing systems of symbols. But Scholz explains that Weyl was no relativist. According to Scholz, Weyl thought that

> mathematics did more than offer mere tools for the formation of mathematical models of processes or structures, in a purely pragmatic sense. A good mathematical theory of nature... expressed, if well done, an aspect of transcendent reality in 'symbolical form'. (p. 296)

That is, a good mathematical theory must include a "*metaphysical belief* in some *transcendent* world core", so that the meanings of the symbols used in ordinary practice are not merely the stipulations of individual practitioners (as the relativist would have it), but instead are rooted in a transcendent reality. On Scholz's reading, Weyl thought this because he thought otherwise "no meaningful communicative scientific practice would be possible." Hilbert had also been concerned with the intersubjectivity of mathematical practice, but Weyl did not follow Hilbert's approach by invoking intuition. Instead, Scholz explains, Weyl was interested in taking an approach inspired by post-Kantian German philosophy, particularly the work of Wilhelm von Humboldt, Martin Heidegger, Karl Jaspers, and Ernst Cassirer. Following the writings of these philosophers, Weyl conceived of our use of symbols in communicative practices by an analogy with the use of tools by carpenters and other craftsmen. What it is for a tool to be good for the practice of a carpenter is not up to him. The raw materials, the abilities of the carpenter, and the item the carpenter wants to build, all put demands on what makes a tool for that task good. Similarly, Weyl seems to have wanted to say, mathematical language is a tool

for the mathematician, and it is not entirely up to the mathematician what makes that tool good. Scholz does not explain what Weyl thought would constrain the goodness of mathematical tools, or how this constraining would work. He seems to have thought the ordinary practices of mathematicians would play a key role, but this isn't fully worked out. I agree with Scholz that Weyl's idea is more a plan for future work than a complete solution. But it is a fascinating idea, and one that is worth developing. (In his fine article, Jean-Pierre Marquis also takes up the idea of tool production and usage in mathematics, arguing that *some* mathematical theories, specifically homotopy theory, are worth knowing only because of their practical value for work in other subject-matters of intrinsic interest, even if these theories are not themselves of intrinsic interest.)

I want to turn now to the essays on Gottlob Frege. Frege was a key instigator of the contemporary Anglo-American approach to philosophy, in which the logical analysis of language is of central importance. Though this approach has been adopted by more recent thinkers throughout philosophy, including ethics, Frege's foremost concern was mathematics. For while Frege is mostly studied by philosophers nowadays, he was very much a mathematician: his doctorate was in mathematics; he was employed in the Jena mathematics department; and he regularly taught courses in complex analysis, elliptic functions, and potential theory. Within the philosophy of mathematics, he is best known for his 'logicist' project, the goal of which was to show that all the truths of arithmetic and analysis (though not geometry) were really truths of logic.

Like the others mentioned above, Frege was concerned about the problem of relativism, particularly for the concept of number. As he wrote in the introduction to his *Foundations of Arithmetic*,

> Yet if everyone had to understand by this name ['the number one'] whatever he pleased, then the same proposition about one would mean different things for different people,—such propositions would have no common content. (p. i, [FA])

Like Hilbert and Weyl, Frege was concerned that if there is no single correct answer to what a number is, then the intersubjectivity of arithmetic and analysis would fail. Frege understood his work as having given the correct answer. He accounted for his answer's correctness by appealing to his logicism, that is, to his view that the laws of arithmetic are reducible to the laws of logic, which are laws of thought and thus common to every rational person.

Despite having appealed to deep features of human rationality in order to license the correctness of his definition of number, Frege recognized that human practices played a role in making explicit the sense of the number concept. Michael Beaney's essay in this volume carefully explores how Frege went about "elucidating" the basic concepts of arithmetic and analysis. Beaney explains how Frege drew on "our common conceptual heritage" (p. 53), revealed by the roles our concepts have played in practice throughout history, in making explicit what these concepts really are. (In her fine article on twentieth-century French philosophy of mathematics in this volume, Hourya Benis Sinaceur's description of Jean Cavaillès' project of unwinding the historical 'dialectic' of mathematical concepts suggests parallels with Frege's idea of elucidation, though these parallels are not explored in this volume.)

While Frege's most-scrutinized writings concern arithmetic, Frege was also actively interested in analysis. In his essay in this volume, Jamie Tappenden carefully situates Frege within the nineteenth-century struggle in Germany over how best to think about complex analysis. This was (roughly) a struggle between two camps, one led by Riemann, the other led by Weierstrass. Weierstrass and his followers thought complex analysis should be 'arithmetized', meaning in particular that analytic functions should be defined as functions representable by power series. By contrast, Riemann and his followers favored representation-independent definitions, which in practice meant defining analytic functions as those satisfying the Cauchy-Riemann equations. The Riemannian approach then develops the theory of analytic functions without having to consider particular explicitly defined analytic functions. Riemann pioneered the term "geometric" for this kind of approach to analysis, but this wasn't a stretch: he encouraged visualization in analysis, developing the notion of a Riemann surface to help. He even encouraged physical reasoning in analysis, using Dirichlet's principle freely even though his evidence for it was based on potential theory. (Riemann's own reflections on these matters were quite rich, provocatively engaging philosophical matters, as Ferreirós documents in his fine essay in this volume.)

Tappenden thinks the received view of the importance of Frege's work—in which Frege's definition of number is the culmination of the rigorization project that begins with the definition of reals by Cauchy sequences of rationals, rationals by pairs of integers, and integers by sets via Frege's definition—is wrong. That is, it is wrong to see Frege as a Weierstrassian. Instead, he argues, Frege's work should be seen within the Riemannian tradition, where a central aspiration was the

identification and clarification of concepts—in Frege's case, of the concept of number. Tappenden gives two main reasons for why it's important to see Frege as a Riemannian. Firstly, it is because Frege's logicist project was to be a reduction of arithmetic and analysis to logic, and so knowing that what Frege meant by 'analysis' was Riemannian analysis can help us better understand his logicism. Secondly, it is because we can understand Frege's peculiar demands for rigorous definition better when we understand the constraints Riemannians put on definition.

Tappenden remarks that the Riemannian school called their approach to mathematics "conceptual", because it sought definitions of concepts that used "internal characteristic properties" of the domain under investigation rather than mere "external" properties. Tappenden's essay nicely helps navigate this unfamiliar terminology, which goes back to Gauss and even further to Leibniz. He remarks that while it is a difficult philosophical question to say precisely what was meant by these terms, within the practice of e.g. nineteenth century German complex analysis the cash value of these terms was well-known. As he writes of the upshot of the Riemannian approach:

> Weierstrass holds that there can be no dispute about the kind of thing that counts as a basic operation or concept: the basic operations are the familiar arithmetic ones like plus and times. Nothing could be clearer or more elementary than explanation in those terms. Series representations count as acceptable basic representations because they use only these terms. By contrast, the Riemannian stance is that even what is to count as a characterization in terms of basic properties should be up for grabs. What is to count as fundamental in a given area of investigation has to be *discovered*." (p. 112)

As a result, Tappenden concludes, the Riemannian must embark on a "quest for the 'right' definition of key functions and objects", presumably one in terms only of what is 'fundamental' in that area. He argues that Frege's attempt to give a logicist definition of number should be understood as an instance of this general Riemannian quest.

To understand better what this quest is all about, I want to pose and try to answer two questions about these Riemannian definitional 'quests'. Firstly, does the Riemannian think there must always be a 'right' definition, and if so, what makes it right? Secondly, how does the Riemannian think we are to know when the 'right' definition has been found? On the first question, I think Tappenden's discussion is inconclusive. When he says that the Riemannian thinks what is fundamental

in an area has to be discovered, it is unclear whether he means that the Riemannian always thinks there's a fact about this to be discovered. If that if the answer is 'no', then the Riemannian is a relativist. As to the second question, Tappenden's answer is that for the Riemannian, definitions prove their correctness by their 'fruitfulness', for instance in organizing our practice well or in playing a role in important further research. Tappenden discusses this view in more detail in other work, but we can address it without leaving this volume by turning instead to the essays on the Riemannians Richard Dedekind and Emmy Noether, by Jeremy Avigad and Colin McLarty, respectively.

In his essay, Avigad writes about Dedekind's Riemannian approach to developing ideal theory, which he took to mean in practice avoiding computation as much as possible. Dedekind instead adopted the axiomatic, set-theoretic approach familiar to us from contemporary algebra. His work in turn influenced Noether and subsequently a central strand of twentieth-century algebra and algebraic geometry. McLarty gives an overview of how Noether brought this contemporary approach to topology. Continuing Dedekind's Riemannian quest to avoid computation in algebra, Noether took a "purely set-theoretic" approach that was, in her words, "independent of any operations" (p. 193). Instead of studying addition or multiplication of the elements of a ring, for instance, she proposed studying particular subsets and homomorphisms preserving the structure of those subsets. In Riemann's terms, she saw these structural properties as the 'internal characteristic properties" of rings, rather than the computational properties which she thought were merely 'external'. This approach gives special value to homomorphism theorems, as McLarty ably documents.

Thus both Dedekind and Noether had views on what the 'right' definitions are in algebra and algebraic topology, as required by the Riemannian program articulated by Tappenden. How did they think we were to know when we'd found those right definitions? Avigad suggests some answers for the case of Dedekind, and I want to consider three of these. Firstly, Avigad suggests that Dedekind thought the right definitions in algebra would avoid elements "extraneous" to algebra. This suggestion just pushes the question back, into what it is to be extraneous to algebra. Secondly, Avigad suggests Dedekind thought the right definitions in algebra would unify the domain being defined; as he puts it, "A single uniform definition of the real numbers gives an account of what it is that particular expressions are supposed to represent" (p. 178). But why should we expect that the right definitions will be uniform, rather than having lots of case distinctions? It would be nice if that were so, but wishing doesn't make it so, unless what

makes a definition right is that it's the one we want. Correct definition as wish-fulfillment: if this were Dedekind's view, he would have been a relativist.

Fortunately, there is a third possibility. Avigad suggests that Dedekind thought the right definitions for a domain would yield properties familiar from other domains. For instance, in ideal theory Dedekind's "overall goal [was] to restore the property of unique factorization, which [had] proved to be important to the ordinary integers" (p. 171). Then many results following from unique factorization in the integers could be carried over to ideal theory. This is surely an important labor-saving technique. But why should we think that this technique leads to the right definitions for a domain? There would have to be something 'inevitable' about those properties if this technique were to avoid being another type of relativism. And indeed Dedekind seems to have thought certain properties were inevitable. Like Frege, Dedekind thought the familiar laws of arithmetic are laws of logic, and seems to have believed that laws of logic are laws of thought; thus, we can't help but arrive at the properties we do in arithmetic, because of the way our minds are constrained to think. Furthermore, he thought that this made inevitable properties in higher mathematics also: as he wrote in his 1888 essay "Was sind und was sollen die Zahlen?", "every theorem of algebra and higher analysis, no matter how remote, can be expressed as a theorem about natural numbers—a declaration I have heard repeatedly from the lips of Dirichlet." Thus Dedekind resorted to logicism to solve the foundational crisis.

I've addressed these matters about the Riemannian project at length because they help make clear the unity of the subject matter of this essay collection. Each essay documents a reaction to the problem of relativism, a problem I've tried to argue here is central to understanding modernity not just in mathematics, but in our culture generally. The essays are uniformly a joy to read, and the bibliography is ample, giving interested readers an extensive springboard for further exploration. I recommend the book highly. [Thanks to my colleague Amy Lara for helpful comments on an earlier draft.]

Department of Philosophy
Kansas State University
aarana@ksu.edu

References

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