Toward a Philosophy of Real Mathematics
by David Corfield
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When mathematicians think of the philosophy of mathematics, they probably think of endless debates about what numbers are and whether they exist. Since plenty of mathematical progress continues to be made without taking a stance on either of these questions, mathematicians feel confident they can work without much regard for philosophical reflections. In his sharp–toned, sprawling book, David Corfield acknowledges the irrelevance of much contemporary philosophy of mathematics to current mathematical practice, and proposes reforming the subject accordingly.

Reading the introduction, it is hard not to be swept up by Corfield's revolutionary fervor. Most contemporary philosophical writing on mathematics focuses on elementary arithmetic or logic, but that is not a representative sample of mathematical practice today or at any time since Euclid. Corfield's push to widen the investigative reach of philosophers of mathematics will be welcomed by readers whose love of mathematics extends broadly. But is this widening important merely because it may be more attractive to lovers of mathematics? Or are there important philosophical questions about mathematics that cannot be answered well without the widening?

Corfield suggests that there are such questions. For instance, we can ask why some concepts (such as groups and Hilbert spaces) have received a great deal of attention while others have not. It could be that our interest in those concepts is just a fad, like our interest in wearing blue jeans; or tied to matters of current cultural interest (such as the way we are currently approaching science) that may change dramatically in time. Or it could be that there are some parts of mathematics that we can't help but run into when our inquiry gets serious enough. We can put the question this way: are some mathematical concepts *inevitable*?

This question is far from a new one: defending a 'yes' answer to it was one of Plato's chief goals, not just for mathematics but for every knowledge–seeking activity. Furthermore, the view *derived* from Plato's that we now call 'Platonism'—that mathematical objects exist, independently of human minds—continues to attract followers. This is true, but notice how the question 'Platonism' is answering is different

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from Corfield's. Corfield's question asks us to account for the inevitability that mathematical practice suggests groups and Hilbert spaces possess. By contrast, 'Platonism' answers whether or not mathematical objects exist objectively or are instead human constructs. 'Platonism' could be true and yet leave us with no story about why certain of these allegedly 'objective' features of reality, such as groups and Hilbert spaces, turn out to be so important.

Corfield wants to turn the philosophy of mathematics toward what is important for mathematics. I want to consider one example of what he has in mind in some detail, both to show what Corfield does, and what he does not do. Chapter Four is a study of analogies in mathematics, cases where two evidently distinct domains seem to be related. His chief example, which I will survey in a moment, is the analogy between algebraic numbers and algebraic functions emerging out of Dedekind and Weber's work in the 1880s. Their work engenders an algebraic approach to the theory of algebraic curves that would rival Riemann's geometric approach and Weierstrass' function–theoretic approach, as it is developed in the twentieth century by Chevalley and then Grothendieck. Corfield motivates this discussion by quoting several famous mathematicians on the importance of this analogy; and also by remarking that analogies might indicate a 'deeper structural similarity' between the domains that might in some sense be inevitable. Far from being merely an example of the sort of question a reformed philosophy of mathematics might answer, the question of inevitability turns out to be one of Corfield's fundamental questions.

Though Dedekind and Riemann had a relatively close personal relationship stemming from their time together in Göttingen, their approaches to mathematics were quite different. This is especially clear in their attitudes toward the area related to what we today call the Riemann–Roch theorem. In lectures in 1855–6 that Dedekind attended, Riemann presented work on meromorphic functions over Riemann surfaces that he would publish in his 1857 paper *Theorie der Abel'schen Functionen*. Let $\mathfrak{F}$ be a Riemann surface of genus p. Among other things, Riemann considered the question of, given m points on $\mathfrak{F}$, how many linearly independent meromorphic functions are there on $\mathfrak{F}$ that have at worst simple poles at the m specified points. Riemann answered this question as follows: there are n many such functions, where $n \geq m - p + 1$. (Riemann's student Roch's later identified the error term, incorporating Riemann's inequality into a more general equality.) In order to prove this result, Riemann used topological considerations, in particular what we now call the 'Dirichlet principle', which yields the existence of a function minimizing a particular integral involving that function. Dirichlet's principle was controversial in the years following Riemann's work, since it was unproved and was shown by Weierstrass in 1870 to fail in certain cases (though Hilbert later showed that Riemann's use of it was defensible). However, the Riemann–Roch theorem was recognized as fundamentally important. Naturally, people began to try to eliminate the use of Dirichlet's principle in its proof.

Dedekind was one of those who attempted to find a new proof of the Riemann–Roch theorem, avoiding not only the Dirichlet principle but also any 'transcendental', topological considerations whatsoever (in practice, this meant avoiding continuity). In an paper published in 1882, Dedekind and his colleague Heinrich Weber showed how the Riemann–Roch theorem could be expressed in algebraic terms, involving fields of algebraic functions defined on a Riemann surface (which themselves could be thought of algebraically). Indeed, Riemann himself seems to have understood this. What is striking about the Dedekind/Weber paper (and of importance for Corfield's project) is the analogy Dedekind and Weber located between fields of algebraic *numbers* and fields of algebraic *functions*. In the 1870s Dedekind had made great progress in algebraic number theory, developing his theory of ideals in his numerous 'supplements' to Dirichlet's lectures on number theory that he edited for publication. Dedekind showed that in rings of algebraic integers, ideals enjoyed unique factorization into prime ideals. Thus, in Dedekind's terms, the algebraic integers obeyed the same "laws of divisibility" as did the ordinary integers: in particular, unique prime factorization. This seems to have confirmed his view, presented in his acclaimed 1888 essay on the foundations of arithmetic "Was sind und was sollen die Zahlen?", that "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."

Further confirmation of this view arrived in the 1882 work with Weber. What was needed, and what they found, was an analogue of ideals of algebraic integers in fields of algebraic functions. Consider, for simplicity, just algebraic functions defined on the Riemann sphere—the complex plane together with a point at infinity—which is a surface of genus zero. Following Dedekind and Weber, rather than starting with the Riemann sphere, we'll start instead only with the field of algebraic functions $\mathbb{C}(\zeta) = \{\frac{f}{g} : f, g \in \mathbb{C}[\zeta], g \neq 0\}$. Dedekind and Weber took the ring of algebraic integral functions $\mathbb{C}[\zeta]$ to be the analogue of $\mathbb{Z}$ and the algebraic integers in this setting, and took the field $\mathbb{C}(\zeta)$ to be the analogue of $\mathbb{Q}$ and the algebraic numbers. They took this as their analogy because ideals in the ring $\mathbb{C}[\zeta]$ enjoyed unique prime factorization, like the integers and algebraic integers; and as noted, it was this "law of divisibility" that Dedekind had identified as critical to the developing analogy. We can see the prime factorization in this setting by noting that $\mathbb{C}[\zeta]$ (and hence each ideal of $\mathbb{C}[\zeta]$) consists of elements $c_n\zeta^n + c_{n-1}\zeta^{n-1} + \cdots + c_1\zeta + c_0$, with each $c_i \in \mathbb{C}$, and that, by the fundamental theorem of algebra, each such expression factors into products of linear terms $(\zeta - z_i)$ for $z_i \in \mathbb{C}$. Accordingly, the prime ideals of this ring will be those generated by these linear factors, $(\zeta - z_i)$, in addition to the zero ideal, which will turn out to be very important in the later development of scheme theory in algebraic geometry. All of the non–zero prime ideals are maximal, and these maximal ideals yield all points $z_i \in \mathbb{C}$, and thus all points of the Riemann sphere, except for the point at infinity which corresponds to the maximal ideal $(\xi - 0)$ in the polynomial ring $\mathbb{C}[\xi]$ under the identification $\xi = \frac{1}{\zeta}$. The maximal ideals $(\zeta - z)$ (with $z \neq 0$) in $\mathbb{C}[\zeta]$ and the maximal ideal $(\xi - \frac{1}{z})$ in $\mathbb{C}[\xi]$ are identified. As a result, we have that the maximal ideals of $\mathbb{C}[\zeta]$ and $\mathbb{C}[\frac{1}{\zeta}]$ (through the above identification) are in one–to–one correspondence with all points on the Riemann sphere. This observation allowed Dedekind and Weber to shift talk of the Riemann surface to talk of the correlated ideals, ultimately giving a proof of the Riemann–Roch theorem (and others) using purely algebraic considerations. [Detlef Laugwitz' text *Bernhard Riemann 1826–1866* (Birkhäuser, 1999), especially p. 159, is quite helpful in understanding these parts of the Dedekind/Weber paper.]

This long detour into the details of the Dedekind/Weber paper shows how interesting Dedekind and Weber's work was, as they defended their view that fields of algebraic integers and of algebraic functions could, and *should*, be treated as obeying some of the same laws. On this significance of this, Corfield quotes Dieudonné:

> [T]his article by Dedekind and Weber drew attention for the first time to a striking relationship between two mathematical domains up until then considered very remote from each other, the first manifestation of what was to become a 'leitmotif' of later work: the search for common structures hidden under at times extremely disparate appearances. (p. 96)

This detour also illustrates one of the problems with this book: Corfield's account of this material only skims the surface of this deep topic. Corfield gives his own sketch of the Dedekind/Weber analogy, discusses its connection with the modern–day notion of ramification, and then turns to another approach to Riemann's work, the "valuation–theoretic" approach developed first by Kronecker, later by Hensel, and extended in later work on p–adic numbers. All this in six breathtakingly concise pages! What Corfield intends to do with this sprawling case study is to discuss what is important about analogies in mathematics, but his purpose would have been better served if he had provided more a in–depth analysis of even just one aspect of this analogy. What we get instead is a series of *Bourbakiste* quotes, from Dieudonné, Weil, and Lang, extolling the virtues of analogies in revealing the 'structures' underlying the mathematics we ordinarily experience. The quotes are interesting, but I am left wondering what they (and hence, Corfield, who mostly lets them speak for themselves) mean by 'structure'. Corfield's indicated aim was to discuss how an analogy between two domains might indicate a 'deeper structural similarity' that could be said to be inevitable. However, we are not given any guide to what 'structural similarity' might be, aside from being shown an admittedly impressive analogy and a series of quotations from famous mathematicians commending this work. Indeed, Corfield spends less than three pages analyzing the case study (with four significantly–sized quotations left largely unanalyzed), less than half the pages dedicated to the details of the case study itself. This is one of the main problems with the book. Mathematicians reading Corfield's book may get the wrong impression that the allegedly 'revolutionary' philosophy of real mathematics is primarily the narration of existing mathematics, and thus by no means revolutionary.

I say this is the 'wrong' impression because I think Corfield has helped clear space for a variety of projects going far beyond both narration of existing mathematics and the types of questions ordinarily dwelt with in the philosophy of mathematics—though he himself seems unwilling to occupy that space. I'll be more specific, by turning back to the Riemann/Dedekind case already discussed. Corfield's heavy reliance on *Bourbakiste* quotations is no coincidence. As he makes clear elsewhere in the book, he is sympathetic to the category–theoretic development of mathematics that followed Bourbaki. That gives one answer to the question of what are the structures revealed by analogies: they are categories. This plays right into the ongoing controversy between advocates of category theory and of set theory as to the 'proper foundation' for mathematics. This is surely an interesting controversy, but one with opposing sides as entrenched as the sides of the Cold War. It is hard to see how taking sides in this debate is going to help Corfield's promotion of a new philosophy of mathematics, when he comes across looking like a mere partisan in an old battle. But worse, he brushes this debate under the rug, providing little defense of the category–theoretic view, and in fact marking such ‘foundational’ debates as having usurped the attention of philosophers for too long.

What is needed here are new ideas. I would like to suggest two, one even hinted at by a quotation of Weil’s in Corfield’s text. Firstly, instead of talking of analogies as revealing ‘structures’, Dedekind talked instead of “laws” being obeyed in different mathematical settings. It’s not a far leap from this view to Hilbert’s axiomatic view of mathematics. Corfield describes Hilbertian axiomatics as merely one step in an “increasingly sophisticated” series, in which Noether’s algebra and Eilenberg and Mac Lane’s category theory are further developments (p. 83). I think Corfield is underappreciating the view that Dedekind and Hilbert can be read as suggesting. By focusing on “laws” rather than on ‘objects’ such as categories, Dedekind and Hilbert were able to focus their attention on the statements—on the “laws” themselves—thus opening up metamathematical avenues of progress. In addition, it allows one (though neither Dedekind nor Hilbert did this consistently) to avoid talk of mathematical objects altogether, talking instead only of the statements we want to assert. In practical terms, this makes little difference, since we ordinarily write mathematics in statements (though this could change with the development of new notation or media, as Corfield discusses in Chapter Ten). In philosophical terms, though, it means we no longer have to discuss whether certain mathematical objects ‘exist’, since we are no longer talking about objects. Since this is an outcome Corfield gives glimmers of favoring at times, I think it deserves our consideration.

The other idea for thinking about analogies that I would like to suggest follows up on Weil’s idea that in working out analogies, we are trying to decipher statements in the language of one domain into the language of other domains. In a 1940 letter to his sister Simone (published in *Notices of the AMS* 52:3 (March 2005), pp. 334–341), he describes himself as having worked in the “Riemannian” tongue for some time, but wishing for the “translation” of all the ideas of that work into the language of function theory (as developed by Weierstrass and his followers) and the language of number fields (in either the Dedekind/Weber ideal–theoretic dialect or the Kronecker–Hensel valuation–theoretic dialect). He saw work with analogies as attempts to fill in a “translation table” between the three languages, constructing a ‘Rosetta Stone’ for mathematics. In explaining analogies this way, Weil made no appeal to ‘structures’. Instead, he emphasized learning to move fluidly back and forth between these different ways of presenting mathematical things. True, each language offers distinctive benefits; for instance, working in the 'Riemannian' language may free us to think more visually or physically. At least as important, though, is the benefit in being able to switch languages freely, which in addition to granting us the advantages of each language whenever we choose, lets us work simultaneously in several languages at once. Doing so lets us anticipate results in one domain that we have not yet discovered but that we should expect, given the otherwise successful translation (Weil mentions cases of this in his letter). Weil's case of the Riemannian language is by no means the only example of this translation project in mathematics; a great deal of work since Descartes has been spent in geometry translating between analytic and synthetic languages, and in improving the translation. I admit that my defense of the advantages of this approach is still quite tentative. My purpose in offering these two ideas for how to think of analogies is just to show how interesting a problem Corfield has framed for us—and how much more remains to be said.

For all the revolutionary talk given in the introduction, Corfield's views end up quite continuous with the usual topics of the philosophy of mathematics. I have already highlighted his interest in the quite–traditional topic of conceptual inevitability in mathematics, and of his advocacy of the category–theoretic side in the ongoing struggle over the 'foundations' of mathematics. When he continues discussion of the Riemann/Dedekind analogy in Chapter Eight (otherwise dedicated to Lakatos' work), he turns our attention to Kronecker's role in its development, and highlights how later mathematicians such as Weyl took sides in the choice of an Dedekindian ideal–theoretic or a Kroneckerian valuation–theoretic approach. Corfield emphasizes how they based their decisions in part on how 'constructive' they perceived each approach to be. The value of constructive reasoning is yet another traditional topic in the philosophy of mathematics. Here again, Corfield's project is not nearly as radical as he would have us think.

Still, for all my frustration with the book's limitations, I think Corfield does a nice job of showing how the philosophy of mathematics can begin to engage areas of mathematics besides arithmetic and logic—a shift I strongly favor. There are interesting chapters on automated reasoning, Bayesian reasoning, and Lakatos' work, for those who are interested in these topics. I have tried to focus on the parts of the text that I think are the most daring, and probably of the widest interest among readers of this magazine. Corfield deserves to be supported for his daring, and for his hope that the philosophy of mathematics will be revolutionized, even if his book is not the revolution we might have hoped it to be. [Thanks to my colleagues Zongzhu Lin and Scott Tanona for their helpful comments on earlier drafts.]