# Nineteenth century analysis as philosophy of mathematics

## Book Section

# 19th century Analysis as Philosophy of Mathematics

Jeremy Gray
Faculty of Mathematics and Computing
The Open University
Milton Keynes, MK7 6AA U.K.

January 27, 2008

## 1 Abstract

Historians and philosophers of mathematics share an interest in the nature of mathematics: what it is, what features affect its growth, how it informs other disciplines. But much of the work done in history and philosophy of mathematics suggests that the two groups largely work in isolation. A reconsideration of the history of mathematical analysis in the 19th Century suggests that history and philosophy of mathematics can be done together to the advantage of both, and also how legitimately different enquiries need not drive them apart.

## 2 The graph metaphor

Consider this schematic attempt to survey all of mathematics. A typical paper in mathematics written at any time in the last 200 years establishes a result, or perhaps several results, here called theorems, that are, in the author's opinion, of greater significance than some other results established in the paper that are steps on the way to the theorems; let us call these lemmas. If the author is not so obliging as to stratify his results, let us do so for him. We now imagine a directed graph in which all the lemmas and theorems are vertices, and the directed edges are the proofs, and we will pretend that the paper is so carefully argued that the edges are entirely composed of routine, valid arguments. So an edge means something like: 'the start vertex is used to show the end vertex'.

Of course, the paper is not self-contained; it rests, implicitly and explicitly, on other results in other papers, and so we need to fit this paper together with all the others that have been written, all the books, and so forth. The result would be a monstrous directed graph. Such a thing does not exist, although I suspect it would not be impossible to create given the will to do so. What does exist is the informal awareness among mathematicians of greater or lesser parts of the graph. It is entirely possible to map out a course of study that will take a beginner to their chosen destination at the frontiers of research in much this fashion; that is what books and well-structured lecture courses collectively do.

Let me call this picture the graph of mathematics, or the graph metaphor (for mathematics). My first question is: what, if anything, is significantly wrong with this picture of mathematics? Of course, I believe there is something wrong with it other than its naiveté. It is not merely over-simplified, it is wrong in ways that would not be remedied by refining the graph; it is the wrong metaphor for mathematics. Certainly we can locate neither the history nor the philosophy of mathematics in it, and, I shall suggest, they cannot be fitted into it in any satisfactory way. But notice that the graph has at least some merit in the eyes of professional mathematicians: it joins up results by chains of routine arguments and it forms a structure to which new results can be joined.

History first. The edges of an ideal graph would all be valid proofs. Those in any actual version of the graph are the written-down proofs, so the graph does not reflect the (let us suppose) timeless logic of implication but the temporal facts of discovery. We could colour code them, display the graph as it evolves every day, week, month or year. For some people that's all history is – one damn thing after another – but not, I hope, for anyone seriously interested in history. So historians could only accept the graph provided they were allowed access to all the graphs. When I have in mind the graph metaphor seen chronologically I might speak of it as a picture of linear growth: new results are simply joined on to old ones. Occasionally a vertex might have to be cut off, if it is found to have been joined on by a fallacious argument. But the historian of mathematics is likely to reject even this dynamic graph evolving over time as sufficient: it lacks any element of historical analysis, explanation, or structure. There are no mathematicians in it, no motivations, no connections to any society in which this mathematics is done.

Philosophy next. The edges are unproblematic from this standpoint too, so the philosophers' attention shifts to the vertices (or small collections of vertices) that have no incoming edges. These typically will be the axioms and fundamental definitions in a branch of mathematics. Philosophers may also focus on vertices with few incoming and many outgoing edges, to allow for those cases when a fundamental definition or a key theorem is incorporated in a larger structure. This happens when, for example, real analysis is seen as a part of topology, or when topology is seen as an aspect of set theory. Insofar as these assumptions are extra-mathematical they seem like the best target for philosophical enquiry. But if the graph suggests certain philosophical tasks, notably those in foundations, it does not suggest all of them: it lacks enough structure to highlight significant structural features of the mathematical enterprise.

Philosophers of mathematics have, it seems, concentrated well and fruitfully on topics in the foundations of mathematics and the related field of mathematical logic, but this conference addresses a feeling that there is work to be done elsewhere in what can be called the philosophy of mathematics. My tribe, the historians of mathematics, are an easier group for me to criticise, and I think we must agree that too much history of mathematics is a walk along part of the graph, bulked out with matters the graph does not try to reflect (biographical, social, motivational, etc.).

I offer the graph as a challenge. It is a picture of mathematics superficially attractive to mathematicians, it allows for a routine kind of history, and it suggests that philosophy of mathematics should be a study of the foundations of mathematics. We are out of business if our call to action does not result in something better than what this graph allows.

### 2.1 Are we there yet? (And why we aren't)

We need to think what the defects of the graph are, and why the problems they suggest in the history and philosophy of mathematics weren't raised and solved a generation or two ago. There are many answers.

1. Mathematical logic is not philosophy of mathematics, but it is close, and it is powerful and precise. Analysis of fundamental presuppositions in domain X is rightly regarded as a prime focus in philosophy of X, but somehow, those questions ceased to engage mathematicians, who lost interest in the philosophy of mathematics and often (but not always) kept mathematical logic at a considerable distance.

2. There is a division of labour that assigns to the mathematician the task of pronouncing on the validity of a proof. This plays to a view that philosophers need not concern themselves with mathematical validity but can accept the verdict of the mathematician. The historian of mathematics can also subscribe to that point of view, and when these groups do so subscribe they elect not to study the historical and philosophical issues hidden in the edges of the graph.

3. The nature of mathematical knowledge has an awkward place in theories of knowledge, and not all philosophers engage with it even when epistemology is their subject.

4. An overweening pride among philosophers of science gave their work a disagreeably prescriptive or normative edge which did not endear them widely.

5. History of science as practised in Britain or America is done these days with almost no attention to the issues that engage historians of mathematics or, all too often, philosophers of science.

Lakatos was one of the few philosophers of mathematics to contest this image. His discussion in (Lakatos 1976) of a relatively trivial topic in mathematics, the classification of surfaces, was intended as a metaphor that displayed other features of mathematical life, chiefly centred on the analysis of purported proofs. He was not interested in the quality of routine arguments, but pointed out two more global processes that are at work. Monster-barring concerns counter-examples, concept-stretching the way in which the fundamental concept can seemingly change. A monster might be excluded – 'the theorem is not intended to cover that' – and it might be included later on when the concept has been suitable stretched.

There are two criticisms of the graph metaphor here. One says that the graph does not adequately deal with discovery but is too focussed on its aftermath. The other says that the process of drawing edges from existing vertices to a new one is subject to an analysis that goes beyond the quality of those edges, which is why I called it global earlier on. It is reasonable, I believe, to think of these points as concerning the meaning of the key mathematical terms; one did or did not intend by, say, 'polyhedron' to mean, refer to, include something 'like that'. These observations of Lakatos are entirely sound, but they do not have the force in history and philosophy of mathematics that he must have hoped for, and one reason for that is that they are simultaneously too radical and too timid. Too radical in their opposition to foundationalist reductionism (the idea that all philosophical questions in mathematics concern the foundations of mathematics as laid down in various ways in the 20th Century). Too timid because I think it came to be felt that there might be many ways in which mathematical practise departs or has departed from the linear growth model implied by the graph metaphor, so many that the philosopher of mathematics would do well to regard the actual growth of mathematics as too ad hoc, culture specific, or downright psychological to be grist to any philosophical mill; better to stick with the foundations of mathematics where honest philosophical work is to be done.

# 3 A look at the history of analysis

## 3.1 Dirichlet on Fourier series

I want to look at the history of analysis in the 19th Century again, and suggest that there are ways of thinking about it that on the one hand show clearly what is wrong with the graph metaphor for mathematics, and that on the other hand show clearly how history and philosophy of mathematics can proceed together to their mutual advantage.

I will start not with Cauchy but with Dirichlet, because I think Dirichlet was the first to see clearly the philosophical implications for mathematics of the new proof methods proposed by Cauchy for the treatment of real analysis. In 1829 Dirichlet published one of the best-known and widely appreciated papers in mathematics, in which he set down his ideas about the convergence of Fourier series. Fourier had claimed (most evidently in his *Théorie de la Chaleur* of 1822) that any function $f(x)$ on the interval $[0, 2\pi]$ can be written as an infinite series of the form

$$\frac{1}{2} + \sum_{n=1}^{\infty} a_n \cos(nx) + b_n \sin(nx),$$

where

$$a_n = \frac{1}{2\pi} \int_0^{2\pi} f(x) \cos nx dx$$

and

$$b_n = \frac{1}{2\pi} \int_0^{2\pi} f(x) \sin nx dx.$$

His argument was little more than a naive calculation; his claim was entirely general.

As put, this provoked Poisson and Cauchy into attempting proofs, but Dirichlet's intervention in (Dirichlet 1829) proved decisive. What attracts me to this proof in the present context is Dirichlet's willingness to restrict the claim to a class of functions for which he can offer a proof. These are the functions which are monotonic increasing or decreasing except at a finite number of points in the interval $[0, 2\pi]$ and are continuous in that interval except again at a finite number of points. For these functions the apparatus of Cauchy's calculus can be used to prove convergence. On the other hand, as Dirichlet noted with the 'toy' example of the function that takes one value at the rational numbers and another value at the irrational numbers, the theorem cannot be true for every function on the interval (because this one cannot be integrated, so there is no way of evaluating $a_n$ and $b_n$). He could have offered a meatier example, $\sin(\frac{1}{x})$, which is not piecewise monotonic in the interval $(-\pi, \pi)$.

With this theorem, Dirichlet abandoned the idea that a function is something we know about, and which has automatically a number of properties. You might argue that Cauchy had earlier reached this position, but I would rather interpret Cauchy as a sophisticated naturalist: there are functions out there in the same way that there are snakes. Some snakes may be poisonous while others are not, just as some functions may be analytic, differentiable, continuous, while others are not. And of course there will be cases where it is too hard to say. Dirichlet would not have disagreed, but the 'toy' function shows that Dirichlet knew that Fourier's claim would not cover all the range of functions, because some eluded the fundamental techniques of the calculus. He did envisage extending the range of functions for which Fourier's claim could be proved, although he never succeeded, but the realisation that the calculus was not adequate to describe the full range of functions was first made by Dirichlet.

### 3.2 Riemann on Fourier series

This raised another question: what precisely were the functions that the calculus applied to, and what were out of range? Until that question was answered, mathematicians could not claim to understand the calculus after all, despite all Cauchy's fine work. This was Riemann's opinion, in the magnificent paper (Riemann 1854) he wrote for his Habilitation in 1854 (first published in 1867). He was willing to agree, he said, that Dirichlet's examples covered all the functions that arise in nature – that is, in the study of natural phenomena. But there were applications of Fourier series methods outside the physical sciences, for example, to number theory, where precisely the cases omitted by Dirichlet 'seem to be important'. And there was, in any case, the need for clarity and rigour in the principles of the infinitesimal calculus, and that plainly could not be fully met until this whole question was fully resolved.

Riemann then proceeded to open the subject up completely by giving a method for finding explicit examples of functions that failed in various ways to agree with their Fourier series, or failed to have a Fourier series at all. The details need not concern us, but we should note that in exploring the range of functions to which the differential calculus applied Riemann spoke of investigating functions without making any particular assumptions on their nature. Later workers were going to spend some decades talking about what they called 'assumptionless' functions. At least in Riemann's opinion, these functions were not only entirely arbitrary, they were the exclusive property of the mathematicians because they do not occur in nature. Of course nature was to reject these constraints, but it is clear that at this stage Riemann was quite clear that mathematics did not stop with the objects one might say were (possibly idealised) abstractions from things in the 'real' world.[1]

Mathematics then, for Riemann, is about concepts created by the mathematician, and one important problem here is to evaluate the methodological tools by which such functions are created.

[1] Note here that when a mathematician speaks of a function disagreeing with its Fourier series this already involves a distinction between what the function is and how it is represented; Fourier had only imagined that a Fourier series would equal the function of which it was the Fourier series.

May I use the word nonlinearity here in a metaphorical sense? It is not the case that there are functions out there which are brought to the laboratory where they may baffle existing techniques. The functions cannot be defined without these techniques; they are a product of the techniques used to analyse them.

## 3.3 Cantor

I can only look briefly here at another well-known part of the story of 19th Century analysis that bears retelling: the pre-history of Cantor's transfinite ordinals. The original, and significant, question that Cantor took over from his colleague Heine and answered very much more profoundly was a question about the limits of mathematics. Cantor wanted to know how, or at what point, does the theory of Fourier series break down and a function have more than one Fourier series, which would surely spell trouble. It was to that end, via his theory of derived sets, that he constructed not only strange subsets of the real numbers (at that time nowhere properly defined) but also the process that was to lead him to the transfinite ordinals a decade later.

## 3.4 Paul du Bois-Reymond

The first volume of Paul du Bois-Reymond's *Allgemeine Functionentheorie*, but the only one to appear, was published in 1882. It provoked at least one sharp rebuttal, by Benno Kerry, (Kerry 1890), which shows how divergent views had become in the philosophy of mathematics.[2] Much of his book was motivated by the following question. One often asks in mathematics if a certain expression has a meaning and, in particular, a numerical value. Typically, the expression is obtained from a sequence of meaningful expressions, so the question is whether this sequence has a limiting value. Du Bois-Reymond's *Allgemeine Functionentheorie* deals with two themes: certain types of infinite sets of numbers, and philosophical questions in the foundations of the calculus. They are themes on which he could speak with some authority. Du Bois-Reymond considered the sorts of infinite subsets of the real line that can arise, alerted by his study of Fourier series, and he wished to give some kind of scientific or even philosophical account of what his infinite sets are. Here he continued a dispute between two groups he called idealists and empiricists that he had raised in the first half of his book. Roughly speaking, the idealist is able to go to the limit, the empiricist is not.

By a simple construction he cooked up a sequence of complicated infinite sets of points, $B_n$, that exemplified Cantor's iterative treatment of sets. For each value of $n$ the point set $B_n$ is a subset of the point set $B_{n+1}$, and the nature (some might say, even the existence) of the union of the sets $B_n$ is at issue; it is a subset of the interval $[0, 1]$, but not the whole thing. Du Bois-Reymond concluded that the infinite point set of all numbers between 0 and 1 existed only for his idealist. His empiricist, he believed, could accept only countably infinite sets, whereas what remains when the union of the sets $B_n$ is removed is uncountable.[3]

[2] I deal with these and related issues at greater length in my forthcoming book *Plato's Ghost*, 2008.

[3] The union of the sets $B_n$ is the union of a countable union of countable sets, so it is countable, but the set of all real numbers is not countable, a fact Cantor had proved in 1874.

The idealist and empiricist positions are irreconcilable. To do analysis, du Bois-Reymond therefore proposed a neutral language, set out under the motto of 'Empiricist language, idealist proofs' (p. 156). Although the idealist may say a limit exists, the empiricist may say that the existence is nothing more than the existence of the sequence – it matters not. What matters is that the talk of limits and limiting values has been made precise in a way each can, on different grounds, accept. This neutral approach resolved not only the question of the meaning of infinite sets, but, as du Bois-Reymond put it, broke the spell and allowed analysis to be the mistress of the house (p. 167).

In 1890 the philosopher Benno Kerry's posthumous book *System einer Theorie der Grenzbegriffe* appeared. It represents his definitive criticism of du Bois-Reymond's ideas. It did not seem to Kerry that the existence of a limit in either the idealist or the empiricist senses that du Bois-Reymond had invoked mattered in mathematics, unless one wanted it to be true and applicable. Rather, all that was required was that a concept, such as limit, was clear and fruitful for making hypothetical judgements. Du Bois-Reymond, Kerry noted, had an exclusively geometrical sense of existence in mind, but the way in which geometric points exist should not be confused with the way in which limits exist. Things exist in different ways, and failure to exist in one sense does not preclude it in another.

As for limits, said Kerry, often their existence is undoubted: the sequence $1/n$ tends to 0 with increasing $n$, $1/3$ is the limit of $0.3333\ldots$, and so on. Some limits are taken to exist even when they are never seen – 'a pure black colour', for example. In the case of the physicist's perfect fluid, what matters is the applicability of the deductions, not their truth. Such limits are different from the 'limit' of the sequence $1, 1-1, 1-1+1, \ldots$ because that 'limit' is a self-contradictory idea.

Du Bois-Reymond's book bears witness to a deeply held belief that mathematics must be about something, its objects should exist, and should do so in a way closely akin to the way physical objects do. Existence should mean something like existence in space and time. Kerry's alternative was much more radical. Existence is freedom from contradiction, mathematical objects may exist in many ways and have merely to imply coherent conclusions. Du Bois-Reymond's approach can seem remarkably limited, but it is not, ultimately, that different from Frege's.

## 4 A second look at the histories and philosophies of mathematics

My first point is that if you look at even the best histories of mathematics – and this is a topic with very good coverage – the above account is not what you see. There is much more emphasis on the difficult mathematics, the discovery of point sets of various kinds (such as the famous Cantor set), the failure to distinguish topological properties of sets from measure-theoretic ones, and so on. Increasing rigour is a prominent theme. I have no quarrel with that, except that a dimension is missing.

If, on the other hand, you look at philosophy of mathematics at any time in the last fifty years for a discussion of this, you also don't see this part of the story. There is considerable attention to the reductionist narrative, which reduces the real numbers ultimately to sets. It picks up foundationalist questions, it looks at increasing rigour and follows them until the reduction is done. On one account the process ends with Weierstrass and Frege and the transition form reductionism to logicism. On another, it ends with Weierstrass, and Frege has failed to appreciate what the mathematicians did. Recently, Tappenden has contested the accuracy of each version on the grounds that it neglects the arguably more important developments in complex analysis, and argued that this has contributed to a failure to appreciate Frege's mathematical concerns; see (Tappenden 2006).

What is missing in each account, the historians' and the philosophers' of mathematics, is exactly the overlap: the fact that these mathematicians were behaving like philosophers of mathematics, and the fact that we need to be both historians and philosophers to see it.

## 5 Mathematicians as philosophers of mathematics

The change from mathematics as the study of the idealised objects of nature to mathematics as an abstract enquiry with its own rules, independent of nature, occupied the whole of the 19th Century and the early decades of the 20th. On the first description, existence questions in mathematics are merely disguised versions of existence questions of the usual sort. Rules for correct reasoning are guided by their correctness when applied to the objects under discussion. Mathematics is true, because it is about (idealised) objects and its statements are meaningful. Rigour is a matter of reasoning more carefully about things we find it harder to access epistemologically.

On the second description, all this is changed. Existence questions cannot be settled by appeals to nature but require new criteria. Rules for correct reasoning must, ultimately, be adapted to the new, abstract, conceptual objects, even if they are grounded in, as it might be, day-to-day logic. Mathematics is not in any simple sense true, its statements are not meaningful. It may be useful, correct in some sense, but its applicability becomes both mysterious and possible in new, more indirect ways. Rigour is involved in the very creation of the objects.

Very little of these changes would have been apparent at the outset. Agreement about them was not to be expected. Some quite fundamental questions would seem absurd; the old certainties might be recaptured in an altered form.

The fundamental error implicit in the graph metaphor, I suggest, is the way it presumes a false definition of mathematics. On this definition, we know what mathematics is, if only in the sense that we can recognise it when we see it. We can recognise putative mathematical claims and their purported proofs, and attach vertices (theorems) to other vertices by the right edges (proofs). And so, indeed, we can, much of the time. All of the time for routine mathematics, much of the time even for the harder stuff. Just as, in the much-quoted phrase, we know what time is – until we think about it. I want to argue that there are times when mathematicians do not know what mathematics is, and to find out they have to behave like philosophers, in that they must analyse the very foundations of their subject.

The foundations need not be the past axioms of set theory. These days they are quite likely not to be, and certainly they were not in the 19th century. They may, for example, concern the way mathematical terms acquire meaning and mathematics can be used, or understood. They may, as here, concern the nature of mathematical objects: by what legitimate processes can they be constructed? And they may concern the processes themselves: what processes are in themselves legitimate?

Riemann took up a question that Dirichlet had only felt able to hint at when he (Dirichlet) limited his arguments and hinted at the existence of counter-examples to the original claim. Riemann made this into a question about the limits of arguably the most powerful branch of mathematics, the differential and integral calculus. If the philosopher and the historian see only technical mathematics here, if they just see the creation of a chain of definitions, theorems, and proofs, they miss a significant dimension of the original enterprise. This I believe was a profound investigation into the nature of mathematical objects. It was clear to several people in the field, I believe, that what had to be decided was what the calculus could do. If this was less than it had been thought (which was to answer any mathematical question about any function) then how could that be shown? What objects could serve as counter-examples, what would delineate the boundary between the knowable and the unknowable, and how could the mathematician be sure?

We know about similar questions in the early days of set theory. I suggest they were around before (and since, but I haven't argued for that). I suggest that if we look at the history of mathematics with philosophers' eyes and the philosophy of mathematics with historians' eyes we will find common ground, and we will find that some of the mathematicians of the past were there before us.

## 6 Bibliography


Dirichlet, Peter Gustav Lejeune 1829 Sur la convergence des séries trigonométriques, *Journal für die reine und angewandte Mathematik* 4, 157-169, pp. 117-132 in *Gesammelte Werke*, 2 vols, ed. L. Fuchs and L. Kronecker, Berlin. vol. 1, 1889 and 1897

Du Bois-Reymond, Paul 1882 *Die allgemeine Functiontheorie*, Tübingen

Fourier, Joseph 1822 *Théorie analytique de la chaleur*, Paris

Gray, Jeremy 2008 *Plato's Ghost — Mathematics and Modernism at the end of the nineteenth Century*, to be published by Princeton University Press

Kerry, Benno 1890 *System einer Theorie der Grenzbegriffe*, edited and completed G. Kohn, Leipzig and Vienna

Lakatos, Imré 1976 *Proofs and refutations : the logic of mathematical discovery*, John Worrall and Elie Zahar (eds), Cambridge University Press, Cambridge

Riemann, Bernhard 1854 Über die Darstellbarkeit einer Function durch einer trigonometrische Reihe, *K. Ges. Wiss. Göttingen*, 13, 87 132,in *Bernhard Riemann's Gesammelte Mathematische Werke und Wissenschaftliche Nachlass*, ed. R. Dedekind and H. Weber, with Nachträge, ed. M. Noether and W. Wirtinger, 3rd ed. R. Narasimhan, 227-271

Tappenden, Jamie 2006 The Riemannian Background to Frege's Philosophy, pp. 97-132 in *The Architecture of Modern Mathematics*, J. Ferreirós and J. J. Gray (eds), Oxford University Press, Oxford