Mathematicians as Philosophers of Mathematics: Part 1

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I want to stir up some connections between mathematics, the philosophy of mathematics and the history of mathematics. I want to suggest, from the standpoint of a historian of mathematics, that hard and fast distinctions between these areas might be difficult if not impossible to make, and are ill-advised on all counts - mathematical, philosophical, and historical To this end, I begin here in this first part with some discussion of Kant's views, and show that they were prevalent among mathematicians attracted to Kronecker's style of algebraic geometry; these mathematicians were active epistemologists.

In the next issue of this journal, I will continue with a shorter middle third which glimpses the situation a generation or two later: Weil, van der Waerden and Zariski as ontologists. And finally, I do the stirring, arguing as a historian of mathematics for a position close, I believe, to that of Maddy and Kitcher as far as the philosophy of mathematics is concerned, although I disagree with Maddy about mathematics

It is often suggested, say in the pages of the *Mathematical Intelligencer*, that philosophy of mathematics is of no longer of interest to mathematicians. This is in contrast to the time of Hilbert, when such as Poincaré, Hilbert himself, Brouwer, Weyl and Gödel enjoyed the most vigorous of debates. What is said to have happened is that Hilbert saw off Brouwer, leaving nothing of intuitionism but the gentle admonition that a constructive existence proof is more informative than a non-constructive proof - something with which everyone can agree. Mathematicians could not, it is said, imagine doing mathematics under Brouwer's constraints

On the other hand, the technical implications of intuitionism became absorbed, and probably neutralised, by mathematical logic, a vigorous subject usually kept at more than arms length by mathematicians, who often regard mathematical logic as almost disjoint from mathematics *per se*. Then Gödel put an end to Hilbert's programme, a whole approach to the foundations of mathematics died and Gentzen's modifications of Hilbert's programme also disappeared in the fog of mathematical logic. Lastly, the traditional problems in the philosophy of mathematics (realism, Platonism, the nature of mathematical objects and of mathematical truth) can be discussed just as well with truly elementary questions as with questions of interest to mathematicians. That being the case, mathematicians have no interest in philosophy of mathematics

It is also possible to detect an element of *hubris*, which perhaps in reaction generates the unkind thought that philosophers have got their come-uppance. There is a whiff of a feeling that the philosophy of X is about sorting out the ground rules for X. After that, the details can be left to the technical experts, who thus occupy a slightly less distinguished place in the hierarchy of thinkers. Gerald Edelman (1992) certainly strikes an anti-philosophical note of this kind in the present debates between cognitive scientists and philosophers about the workings of the brain and the nature of consciousness. And indeed, linguistics has become a science, something that, whatever the words mean, philosophy is not.

It could indeed be argued that the last two decades have not been kind to philosophy of science any more than to the philosophy of mathematics. Freeman Dyson may well speak for many scientists in having more sympathy for historians of science than its philosophers, and it has been claimed that the large, essentially normative literature of the middle of this century that sought to characterise 'the scientific method' and solve such thorny questions as the problem of induction produced little more than a pile of harmless banalities Whatever may be the reasons for this state of affairs, if such it be, one problem for philosophers has been the intractable nature of the evidence. It seems that neither practicing scientists not those examined by the eye of history conform to the rules one would like to teach. Not only is the best practice of physicists not that of biologists, but even within physics (no longer the great exemplar) practice is not exemplary. Undone by historical examples, philosophers have turned more and more to the actual history of mathematics and science

At the same time, recent work, much of it emanating from the circles that have organised this Workshop [1], has profitably re-contexualised philosophers: I might mention Friedman's (1992) work on Kant or that of Tappenden (1995) and others on Frege. By insisting upon the question of what various terms might have meant to their authors, and less upon what we, at risk of anachronism, take them to mean, it becomes possible to understand better what the old philosophers meant. In going from philosophy to the philosopher's education and back, profitable new entries are made into the history of thought

In this article, I wish to run that circuit the other way round: from mathematician to philosophy and back. I want to argue that for a historian the dichotomy between mathematics and philosophy of mathematics can be superficial; it may lie on the boundaries between the disciplines, but not necessarily between the subjects. If philosophers of X are people who, from a good knowledge of X, reflect critically

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upon it without, at that moment, trying to add to X, then I see no implicit hierarchy.

This view is, I believe, quite usual in Germany, where I first heard it. It has the advantage that a number of practitioners of X, the best among them, count as philosophers of X. It is in the spirit of Quine's naturalised epistemology and Maddy's advice that it is almost perverse to argue with success, but it adds to the picture the important historical point that the philosophers, in the cases I have in mind, were themselves active mathematicians. Such, at least, is my starting point.

I wish to argue also that echoes of these debates have not always been picked up. My two different examples will deliberately be drawn not from the familiar debates about set theory and the usual arguments about the foundations of mathematics, but from the murkier shores of algebraic geometry. First, I wish to define my terms a little more precisely.

Perhaps one of the classic distinctions between mathematics and philosophy was made by Kant, when he described what a philosopher and what a mathematician knows about triangles (all excerpts from Kemp Smith, 1970, pp. 577-80):

> Philosophical knowledge is the knowledge gained by reason from concepts; mathematical knowledge is the knowledge gained by reason from the construction of concepts. To construct a concept means to exhibit *a priori* the intuition which corresponds to the concept. For the construction of the concept we therefore need a *non-empirical* intuition.

Kant explained that this construction could proceed by imagination alone, in pure intuition. Then he went on to offer what became a notorious illustration:

> Suppose a philosopher be given the concept of a triangle and he be left to find out, in his own way, what relation the sum of its angles bears to a right angle. However long he meditates [...] he will never produce anything new [...] Now let the geometer take up these questions. [...] through a chain of inferences guided throughout by intuition, he arrives at a fully evident and universally valid solution of the problem.

The solution Kant foisted upon the mathematician was, of course, taken from the proof in Euclid's *Elements* that the angle sum of a triangle is equal to two right angles. No matter that it is flawed - as the discovery of non-Euclidean geometry shows - Kant went on to ask:

> What can be the reason of this radical difference in the fortunes of the philosopher and mathematician?

And he answered:

> We are not here concerned with analytical propositions, which can be produced by mere analysis of concepts (in this the philosopher would certainly have the advantage over his rival) but with just those synthetic propositions that can be known *a priori*. [...] I must pass beyond [my concept of a triangle] to properties which are not contained in this concept, but yet belong to it. There is indeed a transcendental synthesis from concepts alone, a synthesis with which the philosopher alone is competent to deal. [...] But in mathematical problems there is no question of this, nor indeed of existence at all, but only of the properties of the objects in themselves.

There are some directions we could go in from here that I do not wish to pursue: I shall ignore the intellectual hierarchy Kant wishes to insinuate, and drop the charge that he misconstrued the foundations of geometry. But I do wish to take on board that there is a discourse (philosophy) that is concerned with concepts, and another discourse (mathematics) that is concerned with properties belonging to but not contained in concepts. I shall recall that one has knowledge of these properties by the construction of concepts, which in turn requires non-empirical intuitions. I also observe that making these kinds of distinctions in a serious way, so as to draw productive conclusions, is what philosophers do, not mere practitioners. And finally, I observe that these distinctions can be normative; indeed, philosophy of mathematics, like philosophy of science, is often normative.

Let me now move forward about 100 years from when Kant published these views, to 1886, and the first of my examples.

> Definitions must be algebraic and not only logical. It is not enough say "Either a thing is, or it is not". One must show what one wants to be and what not to be in the particular domain with which we are concerned. Only then do we take a step forward. If we define, for example, an irreducible function as one which is not reducible, that is to say which is not decomposable into other functions of a definite kind, we do not give an algebraic definition, we only state a simple logical truth. For us to give a valid definition in algebra it is necessary that it be preceded by an account of the method which allows us to obtain the factors of a reducible function by means of a finite number of rational operations. (Molk, 1886, p. 8)

The point at issue is definitional: what is it to say that certain objects exist? The distinction is between the merely logical guarantee and the mathematician's warranty, which is harder to come by and signifies much more. If we change just one word in this statement, from 'logical' to 'philosophical', we have an utterance that is in many ways Kantian. The criterion for a mathematical object is that it comes with a construction. We are asked to deal with properties belonging to, but not contained in, a concept. And, as I said, making these kinds of distinction is the act of a philosopher.

The author may surprise you, although the philosophical stable may not. It is the French mathematician Jules Molk, writing a 166-page paper in *Acta Mathematica*; the philosophy that of Leopold Kronecker, being insisted upon here with all the zeal of an acolyte. So, a few pages later on, having proved a certain theorem using algebraic functions and observing how much their use simplifies the proof, Molk then set about giving:

> the method to follow precisely to avoid the use of these functions. The complications of this method are only apparent and bear only on the mechanism of the proof

Far from making the demonstration itself more difficult, on the contrary it makes us see more clearly the relationship between the hypotheses which we make and the result to which they lead, between our point of departure and our point of arrival; it alone merits the name of an algebraic method because it alone moves in the particular domain of algebra. (p 65)

Molk was born in 1857, and died in 1914. For the last few years of his life, he was the editor-in-chief of the French edition of the *Encyclopädie der Mathematischen Wissenschaften*, the *Encyclopédie des sciences mathématiques*, which made him France's answer to Felix Klein. This editorship gave him the opportunity to publish revised, updated and often considerably extended versions of the German originals, and we shall see that when it came to algebra that is exactly what he did. But to set his work in context, we must go back to Kronecker and to the theory of divisors, as it is called (for a more detailed discussion of this material, see Gray, 1997).

With divisors, progress has steadily covered the tracks of each previous generation. Even a work like Edwards (1990), closely as it sticks to the spirit of Kronecker's endeavour, does not do justice to the size and scope of that enterprise (nor, in fairness, was it intended too). The problem is compounded by Kronecker's notoriously difficult style. Edwards, who quite openly identifies with Kronecker's famous finitism, had earlier commented that:

Kronecker's theory [...] did not win wide acceptance The presentation is difficult to follow, and the development leaves gaps that even a reader as knowledgeable as Dedekind found hard to fill. (1980, p. 355)

Edwards' recent book tackles the matter head on and teaches much of what Kronecker found so difficult to put across. Even Weyl (1940), who in this matter is a friend of Kronecker's, had to admit that:

Kronecker's approach [...] has recently been completely neglected (p iii)

As is well known, Kronecker laid great store by explicit algorithmic procedures, and that is what you get. It does not make for excitement and it is easy to get lost in the march of detail But there is real excitement for the historian here, and we can get a sense of it by attending to the sheer ambition of his project, and above all to why it held arithmetic in such high regard. The paradigm for all who worked in this tradition is not algebra but arithmetic, and it will be worth attending carefully to what they meant by that

It might be best to try and set aside what one thinks one knows about Kronecker's philosophy of mathematics. This is usually expressed in negative terms: his factorisation theory eschewed Dedekind-style naïve set theory and could therefore happily announce that some number was divisible without having an object that represented its divisors (Dedekind was appalled by this); Kronecker was a strict finitist with no place for transcendental numbers - even, on some views, algebraic numbers.

The matter is, as so often, better put positively. The first thing is the enormous range of the project. So far as possible, Kronecker wanted a common method for dealing with all the problems of mathematics that come down to properties of polynomials in any finite number of variables over some field - usually the rational numbers, but for his successors at least it could be the complex numbers or some pure transcendental extension of one of these. So the subject matter included all of algebraic number theory, the theory of algebraic curves and, in so far as it existed, the theory of algebraic varieties of any dimension This is why he occupies what might otherwise seem an unexpected place in the history of early modern algebraic geometry (see Dieudonné, 1974).

The analogy between these different topics, which will be discussed a little below, is a real one and, by refining the question they share of finding common factors, Kronecker sought to exploit it to the benefit of all its various aspects. It is the analogy with algebraic number theory that drove him to call his theory *arithmetic*, rather than merely algebraic The basic building blocks were two things: the usual integers and the rational numbers, on the one hand, and variables on the other. These were combined according to the usual four laws of arithmetic; root extraction was to be avoided in favour of equations (for example, the variable x and the equation $x^2 - 2 = 0$, rather than $\sqrt{2}$) As Molk made clear, the dividing line was to be drawn between algebra and arithmetic on the one side and analysis and geometry on the other

Kronecker (1881a) himself set out the thinking that led him to his general programme in a fascinating preface to a paper 'Über die Discriminante algebraischer Functionen einer Variabeln', a lengthy historical account indicating how much he had already proposed in lectures at the University of Berlin (and who his audience had included) and at a session of the academy in 1862 The guiding aim, which he traced back to 1857 (the date, one notices, of Riemann's paper on Abelian functions) was to treat integral algebraic numbers (for which the modern term is algebraic integers - roots of polynomial equations with leading term 1 and integer coefficients).

He encountered certain difficulties, which is where discriminants come in. The resolution of these problems came with the insight that it was a useless, even harmful restriction to consider the rational functions of a quantity x that satisfies an algebraic equation of degree n only in the form of polynomials in x (i e , as linear homogeneous functions of $1, x, \ldots, x^{n-1}$). It would be better, he realised, to treat them as linear homogeneous forms in any n linearly independent functions of x This made it possible to represent complex numbers by forms in which every algebraic integer appeared as an integer, while circumventing the difficulties. The insight may be put another way: an irreducible polynomial of degree n with distinct roots defined n quantities at once, and it can be shown that it cannot share a subset of these roots with any other irreducible polynomial. By picking on one root, problems arise that can be avoided by treating all the roots simultaneously (if you like, study $\pm\sqrt{2}$, but not just $\sqrt{2}$).

He discussed these results with this friend Weierstrass, who urged him to apply the same principles to algebraic functions of a single variable and if possible to the study of integrals of algebraic functions, taking account of all possible singularities. This set him on the road to a purely algebraic treatment, shunning geometric or analytic methods. He sent the first fruits to Weierstrass in October 1858, but Weierstrass's own results rendered his superfluous in his own eys and so he refrained from further publication. He was brought back to the topic by discovering how much his thoughts coincided with those of Dedekind and Weber (an agreement which did not, he noted, extend to the basic definition and explanation of the concept of a divisor). Therefore, he presented his old ideas, abandoned in 1862, for publication in 1881.

I shall not describe the philosophical disagreements with Dedekind and Weber. They have been commented on several times in the literature: mathematically by Weyl (1940), while some of the more philosophical implications are relevant to the papers by Gray (1992) and Tappenden (1995). Here, I want to dwell on the approach of Kronecker and Molk. I want to stress that it grew out of attempts to solve *mathematical* problems, because I think that it is often presented as an arbitrary philosophical position, even a limitation, one that Hilbert and/or others in the modern tradition rightly brushed aside.

Thus, Felix Klein (1926) said of Kronecker that:

> He worked principally with arithmetic and algebra, which he raised in later years to a definite intellectual norm for all intellectual work. [...] With Kronecker, who for philosophical reasons recognised the existence of only the integers or at most the rational numbers, and wished to banish the irrational numbers entirely, a new direction in mathematics arose that found the foundations of Weierstrassian function theory unsatisfactory. (pp. 281, 284)

Klein then alluded briefly to what has become one of the best-known feuds in mathematics, the last years of Kronecker and Weierstrass at Berlin, offered his own wisdom as an old man on these matters, and observed that although Kronecker's philosophy has always attracted adherents it never did displace the Weierstrassian point of view. Finally, he quoted with approval Poincaré's judgement that Kronecker's greatest influence lies in number theory and algebra but his philosophical teachings have temporarily been forgotten.

It is usual to trace a line from Kronecker to Brouwer, and to see a hardening of the position into a fully-fledged intuitionism. This is a recognised philosophy of mathematics, albeit one that has never attracted many mathematicians, but it has its supporters to this day, Edwards among them. [2]

There is no doubt that this story is a fascinating chapter in the history of the foundations of mathematics, but I wish to suggest that it is not the whole story. Problems arise with it because of a natural desire to cut through the mathematical technicalities and extract the philosophical core, or, if you like, to insist too strongly upon Kant's distinction between philosophers and mathemematicians

Let us go back to what Kronecker had to say about algebraic integers. The point is not that they do not exist, but that thinking of them in isolation from how they come about does not help. The productive way forward was to recognise that every algebraic integer arises - in only one way - as a root of an irreducible polynomial equation over the rationals (irreducible equations do not share roots) and so it arises as with its conjugates. The concept is one thing; the construction brings out the property that what is presented is the family of conjugates. Pick one, by all means, but it is more in tune with reality to treat them all alike. It is entirely acceptable to adjoin one algebraic integer to the ground field, but more appropriate to adjoin all the conjugates separately (so, if there are n conjugates, you should form n conjugate field extensions)

The next point is the sheer weight of the analogy Kronecker is trying to draw. There are the integers and the rational numbers, there are as many variables as you like with which to form polynomial and rational functions. There are the roots of polynomial equations, presented in the manner just discussed. There should, Kronecker suggested, be a profitable unity here. The analogy is at its most powerful between algebraic integers over the rational numbers and over the field of rational functions in one variable.

The central concept is that of the divisor, which can be illustrated by listing some of the basic questions one asks about divisors: when does one object divide another, what is the greatest common divisor of two given objects, are objects that cannot be factorised further also prime? Notice that ordinary integers and polynomials in one variable have strikingly similar theories here, thanks to the Euclidean algorithm.

The analogy is at its most troubled with number fields. All writers (Kronecker, Molk, König) give the same example, due originally to Dedekind, because it is the simplest: algebraic integers of the form $m+n\sqrt{-5}$. It is easy to show that $2-\sqrt{-5}$ is irreducible, but it is not prime. Indeed, $(2-\sqrt{-5})(2+\sqrt{-5}) = 9 = 3.3$, but $2-\sqrt{-5}$ does not divide 3 (the solutions, x and y, of the equation $(2-\sqrt{-5})(x+y\sqrt{-5}) = 3$ are not integers). So the algebraic integers 9 and $3(2-\sqrt{-5})$ have no greatest common divisor: their common divisors are 1, 3 and $2-\sqrt{-5}$ - neither of 3 and $2-\sqrt{-5}$ divides the other. It was exactly this problem that caused Dedekind to formulate his theory of ideals, which invokes ideals that are not principal (generated by a single element) precisely to get round this problem. In the Kronecker approach, there are what are called *natural* fields, where this problem does not arise, and others, where it does.

I do not wish to enter here into the history of these difficult topics, which would take us into the contrasting approaches of Kronecker and Dedekind-Weber. It is enough to note that it exists, and that it too is a rich source of mathematicians acting as philosophers. Rather, I conclude this section of the article by observing that Kronecker and his followers, in pursuit of a profound analogy and therefore engaged in important mathematics, and confronted with real difficulties, felt the need to take a philosophical, even Kantian, stance in epistemology

Kronecker endorsed Gauss's *dictum* that: 'Mathematics is the Queen of the sciences and arithmetic the Queen of mathematics', along with the then-recently published observation of Gauss that arithmetic stands in the same relation to geo-metry and mechanics as mathematics does to science. Not only does each assist the other, but in each case the theoretical status and the validity of the former exceeds the latter.

Arithmetic is the pure product of the human mind, whereas space and time require a knowledge of reality.

In Gauss's case, disquiet was focused on the possibility of a non-Euclidean geometry. In Kronecker's, the line was to be drawn between arithmetic and algebra, on the one hand, and geometry and mechanics on the other; analysis was to go with algebra and arithmetic, but in a special sense. All of mathematics was to be arithmetised, as he put it, using purely and simply the idea of number in the strictest sense, without modification and extension of the idea to irrational numbers or continuous quantities

Kronecker then sketched out how this could be done. Ordinal numbers arise on encountering a set of objects and enumerating them: 'first', 'second', and so on Forget about the ordering, which is arbitrary in any case, and the cardinality of the set is obtained. Think about several sets at once in appropriate ways and the familiar rules for addition and multiplication of positive numbers are obtained. There is rather a lot tucked out of sight here: the set of numbers (Kronecker called it a *Schaar* or family) is tacitly taken to be finite Kronecker simply did not say if this was purely in order to get started, or in response to a position about the existence of infinite sets. Can one start with ordinals, or are cardinals really prior? A lengthy philosophical literature grew up around this one point, which we need not consult now.

It was harder to define subtraction and fractions I omit the details, and note only that subtraction comes about by introducing an indeterminate x and a congruence modulo $x+1$, which introduces a calculus of congruences equivalent to treating x as meaning -1. More complicated congruences dealt with fractions. Algebraic numbers were then introduced as he had been doing in his lectures for a decade and in the *Grundzüge* (1881b). He then gave the simple numerical rule for finding upper and lower bounds on the range of zeros of a polynomial with integer coefficients, and on this basis sketched how to find sequences of nested intervals shrinking down arbitrarily close to any real root Then, he said:

> The so-called existence of real irrational roots of algebraic equations is based purely and simply on the existence of intervals of this kind

This is the position later criticised by Poincaré and Klein.

The subsequent reception of Kronecker's ideas in algebraic number theory is a complicated and on-going story. As is well-known, David Hilbert inclined more and more to a position of opposition to Kronecker, and I think this opposition, which dates from Hilbert's activities in the foundations of mathematics, has been read back into the earlier period The set-theorists won, of course, and Dedekind's ontology is now our naive ontology Emmy Noether and her students put ring theory and algebraic geometry into the language of ideals, not divisors But, for reasons that Weyl (1940) discussed at length, the approach advocated by Kronecker has its merits, and has indeed recently become more popular

Still, it must be said that mathematicians can make unprincipled philosophers, if that is not a contradiction in terms Even the literature I cited above shows a drift away from the philosophical positions of Kronecker and Molk and towards mere mathematical efficacy I claim only that philosophy assisted at the birth of these Kroneckerian ideas, not with their reception.

Notes

[1] This article - the second part of which is to appear in issue **19**(1) - was first presented as a talk at the joint Conference of the British Society for the History of Mathematics and the Canadian Society for the History of and Philosophy of Science, at Oriel College, Oxford, in July 1997. I would like to thank the audience for a helpful discussion.

[2] Off to one side stand two more heavyweights, Poincaré and Weyl, and it is supposed that Weyl was the adherent Klein had in mind. Recently these figures (with Weyl replaced by Klein) have been drawn up as anti-moderns ('Gegenmoderne') by Herbert Mehrtens (1990) in his rich and fascinating book *Moderne-Sprache-Mathematik*

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