Mathematicians as Philosophers of Mathematics: Part 2

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Let us now move forward one mathematical generation. My claim now is not so much that mathematicians behaved like philosophers, although they did, but that we must behave like philosophers if we wish to follow them.

The problem they considered arose from the study of algebraic varieties over fields of arbitrary characteristic. This raised an intriguing ontological issue. When algebraic geometry was the study of varieties defined over the complex numbers, a variety was defined as the common zeros of a (finite) set of polynomials, and was therefore a subset of $\mathbf{C}(n)$ (allowing for the wish to count multiple points and multiple curves with their multiplicity). With the move to abstract algebraic geometry, it became necessary to ask: what is an algebraic variety?

In his book *Moderne Algebra*, van der Waerden (1930) considered a set of polynomials in n indeterminates and the ideal they generate in the polynomial ring $k[x_1, \ldots, x_n]$, where k is a field large enough to contain the coefficients of the polynomials. He then defined a variety as the set of points in K^n that satisfy all the polynomials in the ideal, where K is any algebraic extension of k. So the field $\mathbf{C}$ is replaced by all algebraic extensions of k. This differs from Weil's later idea of a universal co-ordinate domain, which is an algebraically closed extension of infinite transcendence degree, as only Weil's idea allows one to speak of a variety as a set of points.

In Weil's formulation, this enormous field plays the role of the complex field, and it is large enough to contain all of van der Waerden's extensions. The major shift van der Waerden, Weil and others were negotiating was from algebraic geometry as a branch of complex geometry to algebraic geometry as the study of varieties over arbitrary fields. We shall see that it raised philosophical questions about the nature of an algebraic variety, in particular about how a variety can be defined within the framework of set theory.

Thus alerted, let us consider how and why algebraic geometry went from classical or complex algebraic geometry to the so-called abstract algebraic geometry of the period 1920–1950. This is part of a long, complicated and by no means completely analysed story. One strand concerns the evolution of the concept of an abstract field. A defining moment is the publication of Steinitz's long essay of 1910, but of course there were many occasions before then when fields were studied from quite an abstract point of view – some of which I have mentioned already.

Another strand emphasised the importance, if only for the rigour of the findings, of treating geometry algebraically. Most complicated of all to piece together is the long-running struggle between the Italian mathematician Francesco Severi on the one hand and the leading young algebraic geometers of the 1930s: van der Waerden, Weil and Zariski.

These three have determined the received history of what took place: according to them it had become necessary to give algebraic geometry firm foundations which, necessarily, were to be drawn from modern algebra. At his address to the International Congress of Mathematicians in Cambridge, Massachusetts in 1950, Zariski put it in this way:

> the lack of rigor in algebraic geometry has created a state of affairs that could not be tolerated indefinitely [...] a complete overhauling and arithmetization of the foundations of algebraic geometry was the only possible solution. [...] It is a fact that the synthetic methods of classical algebraic geometry [...] in the end became victims to the law of diminishing returns, as witnessed by the relative standstill to which algebraic geometry came in the beginning of this century. I am speaking now not of the foundations but of the superstructure which rests upon these foundations [...] an arithmetic theory of algebraic varieties cannot but be a theory over arbitrary ground fields and not merely over the complex numbers. (1952, p. 77)

van der Waerden, looking back in 1970 in a talk to the International Congress of Mathematicians, described how shortly after his arrival in Göttingen he was given a reading list by Emmy Noether, and:

> Thus, armed with the powerful tools of Modern Algebra, I returned to my main problem: to give Algebraic Geometry a solid foundation. (1971, p. 172)

André Weil opened his *Foundations of Algebraic Geometry* in 1946 with the words:

> Algebraic geometry, in spite of its beauty and importance, has long been held in disrepute by many mathematicians as lacking proper foundations [...] there is no doubt that, in this field, the work of consolidation has so long been overdue that the delay is now seriously hampering progress in this and other branches of mathematics. (p. vii)

Recently, Italian historians of mathematics have been fighting back. A vigorous campaign (if I may adopt the militaristic metaphors of van der Waerden, Weil and Zariski) has tried to rescue the initial Italian work on the theory of algebraic curves and that of Castelnuovo and Enriques on

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complex algebraic surfaces, while surrendering that of Severi, Fano and the Italian geometers of the 1920s and 30s on higher dimensional varieties and complicated questions such as intersection theory. There seems to be little doubt that it was the work of Severi which inspired the younger men: van der Waerden quotes Shaw: "There is an Olympian ring in it. It must be true, for it is fine art" (p. 171), and the others are equally effusive. But it is probably true that the critical view has formed part of the education of most present-day algebraic geometers and in this the old Italians have not helped themselves.

It is not difficult to document the sense that Severi and Enriques, confronted with demands for clear definitions, retreated to a level of heuristic that sought to confuse the roles of intuition and proof, to the ultimate detriment of both. It would be only human to be exasperated by this and to overstate the magnitude of the challenge. On the other hand, there is overstatement. "The relative standstill at the beginning of this century", the "long been held in disrepute" are from 1910, even 1920.

But it is not the historical accuracy of these statements that needs concern us, once we have seen how important they are as a record of how these mathematicians came to see their collective achievement. What is striking is the remedy that was proposed, apparently so naturally: "an arithmetic theory of algebraic varieties cannot but be a theory over arbitrary ground fields". The first thing that has to be said, if only to get it out of the way, is that restricting attention to the field of complex numbers would not have helped. Very difficult problems in purely complex algebraic geometry were only to be solved with the work of Grauert and others, for example Serre, in the 1950s. Another point is that, for Weil at least, deep questions in algebraic number theory were pulling him in the direction of arbitrary fields. Motivation and necessity played their parts.

Zariski, however, put his finger on exactly the point where mathematicians must also be philosophers. In his address in 1950 he explained clearly that, following Busemann and Weil, it had become usual to study algebraic geometry with a fixed field k of characteristic p and a fixed extension of it K called the universal co-ordinate domain. This is an algebraically closed extension of infinite transcendence degree; the variables which are adjoined are indeterminates. Intermediate fields may also be studied which are sub-fields of the universal domain provided this domain is also of infinite transcendence degree over them. As he then put it:

> The definition of a variety as a set of points having coördinates in the universal domain has some startling, and perhaps unpleasant, set-theoretic implications. We have populated our varieties with points having coördinates which are transcendental over k. Thus, if x and y are independent variables the pair (x, y) is a legitimate point of the plane; and – what is worse – if x' and y' are other independent variables, then (x', y') is another point of the plane, quite distinct from the point (x, y). [...] consequently, we have created infinitely many replica of that ghostlike point (x, y). (p. 79)

This is indeed a strange property that belongs to but is not contained in the concept of an algebraic variety, and which is made known by a construction, a pure intuition. Why was it thought to be a price worth paying? van der Waerden had been interested in the concept of a generic point, upon which the Italians had laid great store. This was, in an undefined way, a typical point of a variety. He decided to formulate the concept this way: if the variety is, say, the projective plane, then a point whose co-ordinates are all indeterminates is generic – there is no equation such a point satisfies that is not also satisfied by every point of the variety.

If the variety is, however, a subset of projective space then this definition must be modified. Kronecker had shown that all points of a variety of dimension d can in any case be obtained (after a suitable linear transformation) in the form of an arbitrary choice for the first d co-ordinates (say $x_1, \ldots, x_d$) and the rest algebraic functions of these. Emmy Noether had replaced these co-ordinates x by indeterminates x, and regarded the algebraic functions of the x as living in a field k(x) which she called the *Nullstellenkörper* (of the prime ideal p belonging to the variety).

van der Waerden saw that the point $(x_1, \ldots, x_d, x_{d+1}, \ldots, x_m)$ with $(x_{d+1}, \ldots, x_m)$ belonging to $k(\mathrm{x}_1, \ldots, \mathrm{x}_d)$ was the generic point he was looking for. Moreover, the Nullstellenkörper was isomorphic to the quotient field $k[X]/\mathbf{p}$, thus rendering very easily the idea of a generic point of an algebraic variety within the language of modern algebraic geometry. Only after Emmy Noether had accepted this paper for publication in *Mathematische Annalen* did van der Waerden learn that she had already presented the same ideas earlier in a lecture course.

I give this example as one of many that together would document the wholesale transcription of geometrical concepts into those of modern algebra. But the view I wish to take of it is that defining a generic point this way has an ontological dimension, which mathematicians like Zariski were perfectly well aware of. In order to work with a universal co-ordinate domain, one had to confront these ontological problems. The domain, however, solved another ontological problem: it enabled mathematicians to speak of the set of points forming an algebraic variety.

In conclusion

I have suggested that there have been occasions when mathematicians have operated as philosophers of mathematics. I have chosen examples from algebraic geometry because I wanted a mainstream subject, but not one of the few where philosophical issues are generally recognised. I want to argue that it is not only in set-theoretical topics that philosophy gets done. And I want to show that the philosophers on these occasions are, in fact, mathematicians.

It seems to me that one advantage of this position, if you are a philosopher, is that philosophy intervenes where it matters, at some of the crucial moments in the discourse of mathematicians. But maybe you feel, as Alastair MacIntyre did on being confronted with Rorty's attempt to dissolve philosophy, that he did not want to spend his time wading through an awful lot of bad philosophy done by one or another kind of specialist, so now I wish to try and dissolve the discipline boundaries a little.

In my main example (in part 1 of this article), that of Kronecker and Molk, I showed that they advocated a strong, indeed radical epistemological position, in terms of the construction of objects that made them mathematically known In my second example, in this part, I have tried to show that the next generation of algebraic geometers engaged in a genuinely ontological debate about what a point is, about what a generic point is on a algebraic variety and indeed about what a variety is. They were behaving as philosophers and we must do likewise to follow them

These were important matters for them to discuss, which they addressed openly and on important occasions. I believe very strongly that we should watch mathematicians deciding such questions as how one can speak of a generic point or of the set of points forming a variety Mathematicians' decisions make pragmatic sense; they are effective in their work. To the historian of mathematics, watching Kronecker and Molk disparage merely logical existence and choose the existence proofs of algorithmically-based, arithmetic algebraic geometry looks so close to urging constructions and properties into the discourse that I wonder how much of a Kantian substrate there was even in this unlikely area

In making sense, therefore, of mathematicians' work, a historian may have to pay attention to their philosophies But if one pushes further, it begins to look as if the distinction Kant offered between properties not contained in a concept, but yet belonging to it, might be a difficult one to sustain I speak, of course, as a historian. I could be gratified that history of mathematics has enjoyed a modest, if tenuous, boom in recent years, which perhaps philosophy of mathematics has been denied, and be pleased that history of mathematics enjoys a modest part in the agenda of each International Congress of Mathematicians, which philosophy of mathematics does not. I am not, however, because I do not see how historians of mathematics can stay out of philosophy when they attempt to summarise their findings For me, at least, philosophy of mathematics provides a language for discussing what some mathematicians on some occasions have been doing What I find hard to sustain is the normative element in some philosophies of mathematics.

Kant's critique was, of course, aimed at establishing the pre-conditions for any kind of rational discourse Mathematics, then and since, serves as a handy example requiring a minimum of subject knowledge to appreciate For this reason, as I said at the outset, this kind of philosophy of mathematics does not engage the sympathy of mathematicians. These days, there is another route into some of these questions about the workings of the mind Just as linguistics differs from ordinary language philosophy, so does the heady mixture of cognitive science and neuro-anatomy.

Quite recently, Penelope Maddy (1990) has written a whole book, *Realism in Mathematics*, on the implications if not of cognitive science for the realist position in mathematics, then at least of those of Hebb's theory of neural anatomy. It would be interesting to take that book and set it alongside Kitcher's (1984) *The Nature of Mathematical Knowledge*: now is not the time. I do not for a moment suggest that philosophical problems go away, but they are re-located where both clarity and important distinctions of method and purpose are needed.

In this spirit, it is probably harmful to insist on a hierarchy of intellectual endeavour and more advisable to watch closely what mathematicians do. The import of a historically based philosophy of mathematics, supported by the insights of cognitive science, might be to suggest fruitful ways in which the antithesis between Frege's insistence on logic freed from all psychology and Poincaré's insistence on the role of intuition is a false one.

It is beyond the scope of this article to engage with the ideas of Maddy and Kitcher Were I to do so, I would want to argue that there is more to mathematics than set theory and it would be interesting to see what a philosophy of mathematics might be that were not so restricted in its gaze I think that Kronecker's remarks which this article has discussed, and those of the next generation of algebraic geometers, are suggestive in that context. But to stick with Maddy (1990), I should like to pull out her remark that:

> epistemology naturalised [...] has renounced the classical claim to a philosophical perspective superior to that of natural science. (p 13)

Such a renunciation she endorses and seeks to extend to the philosophy of mathematics

It will be obvious that I am sympathetic to this position. The disparity between what mathematicians accomplish and the disunity among philosophers, coupled with the commonly-agreed stricture that we cannot step outside ourselves (and so explanations that purport to do so are illegitimate), suggests to me that stepping alongside mathematicians might be the best we can do But I have to observe that this is not as easy to do as perhaps she suggests

The first problem is that mathematical concepts become refined, re-defined, axiomatised and otherwise formalised in various ways This does not make it impossible to say that someone opening a fridge door sees a set of three eggs, rather than an aggregate (Maddy, 1990, pp 50-67 *passim*), but it does complicate the matter. The sets perceived by children differ from the sets of mathematicians in many ways, and their theory is surely not immune to the paradoxes that the formal theories avoid René Thom has even argued (1992, p. 8) that there is something utterly unnatural, even wrong, about the idea of sets of disparate objects. He objects to sets, even those chosen from a finite universe, consisting of all the objects that are either 'large or blue' or 'short or intelligent' on the grounds of their linguistic oddity.

This is not to suggest that one should agree with Thom, but rather to point out that a further complication for Maddy's position arises when the mathematicians themselves do not agree. At times, this disagreement is marked I recently argued (Gray, 1992), in a chapter for a book on the theme of revolutions in mathematics, for a revolution in mathematical ontology in the 19th century If I am right, then there was a shift from various naïve realist or Kantian positions, and indeed from a minority linguistic view, to a set-theoretic orthodoxy.

This orthodoxy, as formalised by Zermelo-Fraenkel or Gödel and von Neumann, is what Maddy seeks to give a realist interpretation of. My argument is that this ontological revolution was a social change, with its roots in a changing mathematical community and a divorce from physics. While the revolution was in progress, it could at best be a disputed position as to what the mathematical opinion was that a naturalised epistemology would seek to explicate; some mathematicians would accept it, others deny it Indeed, this article is partly an attempt to work over for epistemology what that earlier one did for ontology, if only because I approach these difficulties as a historian of mathematics

I shall end, however, with some remarks about two aspects of mathematics that are germane, and strengthen my case that mathematicians can be philosophers of mathematics: intuition and the unity of mathematics

Intuition is a hugely vexed term in mathematics. Somewhere along the line, it went from being a technical term in Kant's lexicon to the elementary term used in its naïve sense by Poincaré and Klein One reason for this change may have been the impact of Pestalozzi's philosophy of education, where it was also a central term; here, *Anschauung* meant direct apprehension of the object. To know what a cow is, or better, to teach what a cow is, you should exhibit a cow, not read about it in books and trot out definitions This is not so far from certain philosophies of science, after all.

Intuition was also a term in the growing science of the mind, and often Poincaré and Klein nodded in that direction when invoking it. In a succession of writings and lecture courses, David Hilbert also used the term with its naïve meaning, as a source, along with experience, of ideas that the mathematician must sort out and refine. The common feature of all these naïve uses is that intuition applies to discovery. Intuition may suggest the results one then proves, even ways in which they might be proved Where Hilbert differed most starkly from Poincaré is in the nature of axioms. For Poincaré, the most logical starting point for mathematics was in the simple intuitions of the mind about number and shape; conclusions spread out from there in, perhaps, ever more elaborate ways. For Hilbert, the most logical starting point was a set of axioms which were the most efficacious and easy to apply; they might well not be intuitively obvious at all.

But, as Rowe (1992) has well argued (in his introduction to Hilbert's book), the point that is often forgotten is that for Hilbert mathematics was one subject, not a family of separate disciplines. This view might well have been a conventional piety, but he insisted upon it at length when he discussed the role of axiomatising. Mathematics, he said, had two tasks: it should discover systems of relations and draw out their logical consequences, as pure mathematics does - this is the positive task And it should give it a fixed structure with the simplest possible foundations - this is the regressive task. Axiomatics naturally answered the regressive task. But the two tasks *together* formed mathematics; the regressive task was not to be identified with the whole subject (as surely the choice of name, with its negative connotations, was meant to underline). Hilbert's language captures the dynamic element of mathematics, the shifts in meanings that complicate any realist's standpoint

The unity of mathematics is an important, if intangible, feature of most mathematicians' discourse about their subject. It is an ideology, by which I mean 'a systematic scheme of ideas [...] regarded as justifying actions, especially one that is held implicitly' (Oxford English Dictionary, definition 4). It plays a political role in keeping together the world's mathematicians, by suggesting that there is a reason why, let us say, group theory is part of mathematics and every Mathematics Department should have a group theorist, but computing science is not; a reason, moreover, which is logical and pertinent to the subject, not merely historical and at the mercy of politicians.

It allows mathematicians to affirm, however vicariously, the sense of knowing a subject even though few indeed can lay claim to Hilbert's breadth of knowledge. It underlies debates about discipline boundaries and licenses talk about whether or not mathematics is becoming unduly separated from physics. Like many implicitly held ideas that carry such a burden, it tends to evaporate when it is fished up from the depths and examined closely. But it is real, for all that.

This unity embraces a number of different things: the theorems that can be taken off a peg and used with confidence by mathematicians and non-mathematicians alike; the organisation of those theorems into theories; the often-unexpected interactions between those theories (for example, theorems in number theory that can only be proved using complex analysis); the methods of proof (themselves capable of being adopted and adapted across theories) To this list, every mathematician would add the problems that motivate mathematicians, the discoveries they hope to make and the interactions sought with other disciplines I would argue that part of this unity is also a varied philosophical reflection upon mathematics, even if it lies at an airless part of the deep.

There is no reason why philosophising about mathematics should be part of any given mathematician's activity, any more than group theory. But it might be expected among the work of some of the best mathematicians, those who feel called to shape and direct their subject. There are many questions which present themselves to mathematicians minded to take them up. Some are aesthetic: what are the important problems, what are the right proofs, what are the real reasons? Some are ontological or epistemological, as we have seen. Some are ideological, demarcating what does or does not belong to mathematics The philosophical challenge, if taken up, might then be addressed well, or not so well. But when it is done, the result is surely to blur Kant's tidy distinction and one might hope to challenge other, later, philosophers too. In any case, because the challenge is posed, I feel that historians and philosophers of mathematics are forced into each other's company and into what, let us hope, should be a fruitful union

References

Gray, J. (1992) 'A nineteenth-century revolution in mathematical ontology', in Gillies, D. (ed.), *Revolutions in Mathematics*, Oxford, OUP, pp. 226-248

Hilbert, D (1992) (Rowe, D E , ed) *Natur und Mathematisches Erkennen*, Basel, Birkhäuser.

Kitcher, P (1984) *The Nature of Mathematical Knowledge*, Oxford, OUP

Maddy P. (1990) *Realism in Mathematics*, Oxford, OUP.

Steinitz, E (1910) 'Algebraische Theorie der Körper', *Journal für die Reine und Angewandte Mathematik*.

Thom, R. (1992) 'Leaving mathematics for philosophy', in Casacuberta, C and Castellet, M (eds), *Mathematical Research Today and Tomorrow*, Lecture Notes on Mathematics 1525, Berlin, Springer-Verlag, pp. 1-12

van der Waerden, B L (1930) *Moderne Algebra*, Berlin, Springer-Verlag

van der Waerden, B. L. (1971) 'The foundation of algebraic geometry from Severi to André Weil', *Archive for History of Exact Sciences* 7(3), 171-180

Weil, A. (1946) *Foundations of Algebraic Geometry*, AMS Colloquium Publication, vol. XXIX, Providence, RI, American Mathematical Society

Zariski, O (1952) 'The fundamental ideas of abstract algebraic geometry', in *Proceedings of the International Congress of Mathematicians* vol 2, Providence, RI, American Mathematical Society, pp. 77-89