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MATHEMATICAL KINDS, OR BEING KIND TO MATHEMATICS

David Corfield

1. Introduction

In Chapter III of his *Science et Méthode*, published in 1908, Henri Poincaré claimed that:

> Les faits mathématiques dignes d'être étudiés, ce sont ceux qui, par analogie avec d'autres faits, sont susceptibles de nous conduire à la connaissance d'une loi mathématique de la même façon que les faits expérimentaux nous conduisent à la connaissance d'une loi physique. Ce sont ceux qui nous révèlent des parentés insoupçonnées entre d'autres faits, connus depuis longtemps, mais qu'on croyait à tord étrangers les uns aux autres. (Poincaré 1908: 49)[1]

Towards the end of the twentieth century, with many more mathematical facts since discovered, several mathematicians proposed overarching schemes to organise the facts they considered most significant. In this paper, I shall briefly discuss three of these schemes (those of Arnold, Atiyah, and Baez and Dolan), before drawing some philosophical consequences from their attempts. Rather than the kind of claim made by Frege that with the entry of imaginary numbers we reach the 'natural end of the domain of numbers', we are dealing here with a more open-ended

1 The mathematical facts worthy of being studied are those which, by their analogy with other facts, are capable of leading us to the knowledge of a mathematical law, just as experimental facts lead us to the knowledge of a physical law. They are those which reveal to us unsuspected kinship between other facts, long known, but wrongly believed to be strangers to one another.

sense of conceptual growth. I shall illustrate this theme by discussing the elaboration of algebraic structures designed to measure symmetry.

What emerges is that at any one time mathematicians are operating with a notion very similar to that discussed in the philosophy of science under the heading of 'natural kinds'. We find 'quasi-causal' talk of properties being 'responsible' for a phenomenon, projectability, the transfer of robust mechanisms between domains, and reference to entities not yet fully determined. Debates in philosophy of science prompt further questioning as to the 'naturalness' of these mathematical kinds, whether one should expect them to be dependent on varying human interests, whether there is a distinction between artefactual and real kinds, and whether there is convergence of kinds. I believe that these questions present a wonderful opportunity for a philosophy of mathematics to treat 'real' mathematics, while making powerful points of contact with philosophy of science, and philosophy in general.

As regards science, these themes relating to laws and kinds have received extensive treatment. From where we stand today it seems intuitively obvious that some events that occur in the world are of greater potential scientific interest than are others:

(1) The plastic tea tray I left on the cooker melted.
(2) The ball-point pen I found under my armchair was blue.

This conviction relates to my knowledge that every time I am fool enough to leave my tray on an extremely hot cooker it will melt, while my elusive ball-point pen might have been lost and found whatever its colour. In the former case, if it pleases us we know we can tap into the scientific literature on the temperature at which different plastics melt, and perhaps then proceed to a larger exploration of the stability of polymer structures. Fact (2), meanwhile, does not present itself as a promising starting point. At best, we might hope to find a statistical regularity that blue pens are lost more or less frequently, or found more or less quickly than pens of other colours.

Much has been written about facts being necessitated by laws. During the zenith of logical empiricism, the hope was that one could distinguish between general scientific statements as to their lawlikeness merely on the grounds of elementary syntactic or semantic considerations. Any such hope has long been extinguished by the

comparison of pairs of statements such as:

(1) All solid spheres of enriched uranium have a diameter of less than one mile.
(2) All solid spheres of gold have a diameter of less than one mile.

While the second statement, if true, appears to be so for contingent reasons, the possibility of a falsifier of the first is ruled out, it would appear, by the iron rule of physical law, in this case the laws of nuclear physics. Of course, fact (2) is not completely divorced from the web of science. We have theories concerning the synthesis of elements within stars. Knowledge of the likely distribution of gold in a planetary system must make it highly implausible that a large golden sphere could be formed without intelligent intervention, and even then it would require technological resources far greater than our own. On the other hand, whatever those resources may be, large solid spheres of enriched uranium just cannot be put together for more than an instant, or so we believe.

With much of the discussion of laws and necessity carried out in the metaphysical language of possible worlds, the notion that mathematical facts might vary similarly as to their lawlikeness has appeared to be hopeless. Where I can imagine possible worlds in which very large golden balls exist, I cannot imagine a possible world in which the number denoted in the decimal system by '13' is not prime. But we are not forced to let this observation stand in our way. Instead, we might choose to question the extent to which the possible worlds conception is our only way of dealing with lawlikeness.

Here I shall not be directly treating the question of whether possible worlds are a worthwhile metaphysical debt to incur for those working in philosophy of science. I believe van Fraassen, Harré and others have produced powerful arguments in this regard. Rather, the burden of this paper is to point to the presence of a parallelism between mathematics and science in respect of facts being made more or less significant by the degree of their participation in a grand network of theories. In other words, the claim to be substantiated is that Poincaré was largely right in the quotation with which I began this paper. In doing so, I do see myself as making a significant contribution to van Fraassen

et al.'s crusade against key components of modern analytic metaphysics: "However plausibly the story begins, the golden road to philosophy which possible-world ontologies promise, leads nowhere" (van Fraassen 1989: 93). For, once we have come to terms with Poincaré's perception, we shall then be in a position to confront two opposing intuitions:

> (A) Our best metaphysicians have shown us that the way to deal with necessity and contingency is through the apparatus of possible world semantics. All mathematical truths are necessary in the sense that they hold in all possible worlds. The distinction being treated here is thus merely a reflection of human psychology.
> (B) There is a distinction here very salient for our best mathematical reasoning. If our metaphysics does not speak to this distinction, something is wrong with our metaphysics.

This is largely a matter of one's philosophical stance. For my part I am on Collingwood's side when he says:

> There are two questions to be asked whenever anyone inquires into the nature of any science: 'what is it like?' and 'what is it about?'… of these two questions the one I have put first must necessarily be asked before the one I have put second, but when in due course we come to answer the second we can only answer it by a fresh and closer consideration of the first. (Collingwood 1999: 39-40)

One feature of what it is like to think mathematically is to know that some facts look, and remain looking, highly unpromising as places to begin an investigation, whereas others look likely to, and often turn out to, tap into very deep waters. To put it in a blunt Collingwoodian way, if as a philosopher you do not recognise this difference, you have amply demonstrated that you do not understand what it is like to think mathematically, and therefore should not pursue philosophical investigations of mathematics.

2. 'Quasi-Contingent' Mathematical Facts

Lakatos was always keen to emphasise the similarities he saw between mathematical and scientific reasoning. In particular, he used the term 'quasi-empiricism' to denote the common pattern to the flow of truth operating in mathematics and science. Extending the range of these similarities, what I am arguing for here is the existence in mathematics of 'quasi-contingent' facts, i.e., facts which are shallow or 'happenstantial'. For example, consider the two following elaborations of the primality of 13:

(a) '13' is prime, and, reversing its digits, so is '31'.
(b) '13' is prime, and $13 = 2^2 + 3^2$.

Each of these naturally prompts a study of its generality. Starting from (a), we quickly find that 17 and 71 are both prime, but, alas, while 19 is prime, 91 is not. Furthermore, we observe that primes beginning with an even digit or a '5' will not reverse to give a prime. This route to generalisation seems hopeless. Another line to pursue is to question whether the mild coincidence that '13' and '31' are both prime is dependent on the arbitrary base choice of the decimal system. A worthwhile mathematical fact should not be so beholden. For example, had our number system been expressed in base 8 rather than base 10, what we call '13' would have been written as '15'. Reversing these digits, '51' in base 8 expresses our '41', which is prime. The following table records the results of the same calculation for bases between 2 and 12.

Base	'13'	Reversed	Decimal
12	11	11	13
11	12	21	23
10	13	31	31
9	14	41	37
8	15	51	41
7	16	61	43
6	21	12	8
5	23	32	17
4	31	13	7
3	111	111	13

2	1101	1011	11

We may also extend this table upwards: 13 in base 13 is '10', which reversed is '01'; '13' in any higher base is represented by a single symbol, so left unchanged by reversal. We arrive, then, at the near universal result that the reversal of '13' in all bases except 6 is prime or unity, or the full universal result that the reversal of '13' in all bases is either prime or equal to '1' or to '8'. Coining a term 'cubeprime' to characterise any natural number which is either prime or a perfect cube, we arrive at the result that: In any base, the reversal of what we call 'thirteen' is cubeprime.

Notice that, rather like the way that the non-existence of large gold spheres ties in with our scientific knowledge to some extent, there are some mathematical considerations relevant to this 'law'. For example, the entries in the right hand columns of rows 7 to 12 are forced to be odd, increasing the likelihood of primality.

Pushing our investigations further, we find the same result holds for 2, 3, 5 and 7. At 11, to produce a 'law' we need a new property 'cubeprimeproduct' to describe any natural number which is either cubeprime or the product of two primes. In any base, the reversal of any prime less than or equal to what we call 'thirteen' is cubeprimeproduct. I shall leave further explorations of this fascinating corner of number theory to the eager reader.

Let us now choose (b) as our point of departure, and see whether other primes can be expressed as the sum of two squares. We find that: $5 = 1^2 + 2^2$; $17 = 1^2 + 4^2$; $29 = 2^2 + 5^2$. However, 7, 11, 19 and 23 cannot be expressed in this way. But all is not lost. We note that 5, 13, 17 and 29 are all of the form $4n + 1$, while 7, 11, 19 and 23 are all of the form $4n + 3$, for integer n. Since squares leave remainder 0 or 1 on division by 4, it should be clear that primes of the latter form can never be expressed as a sum of two squares. What can we say in the opposite direction?

Now I meet a problem in my presentation. I expect that most of my readership will know something no human knew 80 years ago, namely that uranium 235 has a critical mass. If the mass of a body of this uranium exceeds the critical mass, there will be a chain reaction causing a huge liberation of energy. On the other hand, experience has shown me that I cannot similarly rely on their knowing a simple mathematical fact known for at least 350 years, namely, that the pattern noted in the

previous paragraph holds for all primes:

> A prime may be expressed as a sum of two squares if and only if it is either 2, or of the form $4n + 1$.

When Fermat demonstrated this in the seventeenth century he used what was later called the 'method of descent', which establishes that for any potential counterexample there is a smaller one, and hence there can be no minimal counterexample to this rule. The notion of descent is still being vigorously elaborated to this day. On the other hand, Gauss's approach to this field introduced the notion of Gaussian integers 200 years ago, and proved that prime factorisation holds for them. Fact (b) is linked to the factorisation $13 = (2 + 3i)(2 – 3i)$, i.e., 13 is no longer prime in the ring of Gaussian integers. Gauss's work marked the origins of the enormous field of algebraic number theory.

So our fact (b) looks less happenstantial because it is an instance of a law, which in turn is a surface indicator of the presence of a richly entangled network of ideas. Now, some philosophers of science are coming to a similar conclusion in their own field. My sympathies lie there with the model-based approach, not in the logicist form of Suppes, but that of the 'Models as Mediators' programme (Morgan and Morrisson 1999), where laws become indices of deeper theorising:

> What has happened to the laws of Nature? They were once thought to be the very heart of scientific achievement. We can now see how superficial a role they play. Laws of Nature are sometimes no more than records of conceptual relations involved in classificatory systems. Sometimes they are descriptions of the workings of models, analytic or explanatory. (Harré 2002: 55-56)

I indicated above a tiny part of the tangled network which may be quickly reached from the solitary fact (b). Unfortunately, the educated lay person is not expected to know anything of this network. We have not moved on so far from the "common superstition", alluded to by Hilbert, "that mathematics is but a continuation, a further development, of the fine art of arithmetic, of juggling with numbers" (Hilbert and Cohn-Vossen 1952: iv). This ignorance presents a huge obstacle for a philosophy of mathematics sensitive to what mathematicians have discovered, but let's try to delve a little.

3. The Huge Obstacle

Often the philosophical treatment of a given notion follows a pattern. Philosophers starting out with an overall conception of a domain want to treat some notion pertaining to that domain generically, to find what can be said uniformly of all its instances. Some will then come to realise that important aspects of the notion relative to a specific subdomain are overlooked. They then dig deeply into that subdomain and find distinctive new features. Some imagine that there is still a right way to think about the notion universally by reinterpreting these findings or else see this subdomain as distinctively 'foundational'; others choose to be philosophers of the specific subdomain and emphasise its uniqueness. The very hard task of patching together these local studies into a richer global picture too often doesn't get done.

In the case of the philosophy of science, for instance, one may either be a reductionist and take a few physical properties and their laws as fundamental or natural, or else think that some chemical or biological properties are not reducible, and so argue for a disparate array of local laws. The harder step is to attempt to patch together these local studies.

In twentieth century philosophy of mathematics, we start at the beginning of the century with a philosopher's conception of mathematics as a whole, perhaps what Russell includes in *The Principles of Mathematics*. This was already an impoverished conception, as Frege, Hilbert, Brouwer and Weyl could have told you. By 1930, from a philosophical viewpoint, it could be stated universally of any piece of mathematical reasoning that all the definitions employed are expressible in the language of set theory (the theory of classes included), and all reasoning represented within the confines of first order classical logic.

Towards the end of the century, a few were beginning to realise that set theoretic reductionism ignores distinctions between specific kinds of reasoning and went in search of local particularity, noting, for example, that the development of the most sensitive, while still computationally tractable, algebraic 'machinery' in algebraic topology is very different from Paul Erdös's combinatorial style of research in graph theory; and that the use of the analogical transfer of constructions between fields differs from the use of extensive computer calculation.

These moves are important, but in selecting local studies, and later in fitting them together, we needed to be guided by a larger conception of mathematics. Here the huge obstacle looms: we have no overview of mathematics as a whole. Where a philosopher of physics will be able to offer you some kind of sketch of the whole domain of physics, for most outside of mathematics, including most philosophers of mathematics, there is little more to work with than the idea gained from the generic view that everything is expressible set theoretically.

4. Mathematical Visions

I now want to offer the reader an impressionistic glimpse of some "big pictures" in mathematics, and subsequently show how they lend themselves to our asking rather different questions in philosophy of mathematics. We begin with a recent suggestion of the Russian mathematician Vladimir Arnol'd, who reckons that something about the Russian training alerts you to the connectivity of mathematics:

> One (…) characteristic of the Russian mathematical tradition is the tendency to regard all of mathematics as one living organism. In the West it is quite possible to be an expert in mathematics modulo 5, knowing nothing about mathematics modulo 7. One's breadth is regarded as negative in the West to the same extent as one's narrowness is regarded as unacceptable in Russia. (Lui 1997: 436)

Elsewhere, Arnol'd explains humorously:

> All mathematics is divided into three parts: cryptography (paid for by CIA, KGB and the like), hydrodynamics (supported by manufacturers of atomic submarines) and celestial mechanics (financed by military and by other institutions dealing with missiles, such as NASA. (Arnol'd 1999a: 403)

Then more seriously

> Cryptography has generated number theory, algebraic geometry over finite fields, algebra, combinatorics and computers.

> Hydrodynamics procreated complex analysis, partial derivative equations, Lie groups and algebra theory, cohomology theory and scientific computing.
> Celestial mechanics is the origin of dynamical systems, linear algebra, topology, variational calculus and symplectic geometry. (Arnol'd 1999a: 403)

He continues:

> The existence of mysterious relations between all these different domains is the most striking and delightful feature of mathematics (having no rational explanation). (Arnol'd 1999a: 403)

Mathematicians have been screaming to us for years to look at this kind of phenomenon, but their pleas have largely fallen on deaf ears. Arnol'd's own suggestion is that there is a way of thinking systematically about these 'mysterious relations' via various informal processes:

> The informal complexification, quaternionization, symplectization, contactization etc., described below, are acting not on such small things, as points, functions, varieties, categories or functors, but on the whole of mathematics.
>
> I have successfully used these ideas many times as a method to guess new results. I hope therefore that in the future this method of the multiplication of mathematics will be as standard, as is now the transition from finite-dimensional linear algebra to the theory of integral equations and to functional analysis. (Arnol'd 1999a: 404)

He goes on to consider examples of three way parallels or *trinities* as he puts it, where one seeks to fill in places in a long three-columned table. In this table, you find mentioned the ubiquitous Dynkin diagrams. The associated A-D-E metapattern, linked to singularity theory and subfactor theory, Arnol'd had earlier (in Browder 1976) picked out as a phenomenon meriting the same sort of attention as has been given to Hilbert's problems. He gives the following examples of their appearance: Platonic solids; finite groups generated by reflections; Weyl groups with roots of equal length; representations of quivers; singularities of algebraic hypersurfaces with definite intersection form; critical points of

functions having no moduli.[2] Elsewhere, he claims that "The belief that all simple (having no continuous moduli) objects in the [*sic*] nature are controlled by the coxeter groups is a kind of religion." (Arnol'd 1999b: 1123)

I'm certainly not suggesting that Arnol'd has a monopoly on systematising schemes. Here's another scheme which explicit argues for the continuity of mathematics over the centuries, this time from Sir Michael Atiyah (1976):

19th century	The study of functions of one (complex) variable
20th century	The study of functions of many variables

Of course, this is not to say that in the nineteenth century linear algebra in higher dimensions wasn't being developed right up to algebras of functionals operating on infinite dimensional function spaces. It's a claim about global, non-linear mathematics. Where Riemann's explorations of the links between algebra, topology and geometry on the surfaces named after him mark a high point of the nineteenth century, by the 1890s Poincaré still saw the need to justify the same kind of treatment of spaces of dimension higher than three.

Can we fill in Atiyah's scheme for our current century? Well, recently he has done so himself:

> What about the 21st century? I have said the 21st century might be the era of quantum mathematics or, if you like, of infinite-dimensional mathematics. What could this mean? Quantum mathematics could mean, if we get that far, `understanding properly the analysis, geometry, topology, algebra of various non-linear function spaces', and by `understanding properly' I mean understanding it in such a way as to get quite rigorous proofs of all the beautiful things the physicists have been speculating about. (Atiyah 2002: 14)[3]

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2 John Baez (1995) explains what is going on here and mentions two more instances: Minimal models; quantum categories.

3 This work requires generalising the duality between position and momentum in classical mechanics: "This replaces a space by its dual space, and in linear theories that duality is just the Fourier transform. But in non-linear theories, how

In chapter 10 of Corfield (2003), I amalgamated Atiyah's scheme to one of Baez and Dolan, which derives in part from another giant of the twentieth century, Alexandre Grothendieck. Extending this amalgamated scheme a little suggests:

19th century	The study of functions of one (complex) variable The codification of 0-category theory (set theory).
20th century	The study of functions of many variables The codification of 1-category theory.
21st century	Infinite-dimensional mathematics The codification of *n*-category theory, and ω-category theory.

I shall not repeat here what I have said about this scheme. Interestingly, Baez has had the idea that the objects of Arnold's interests, the A-D-E metapattern, and the processes of complexification, quaternionization (and octonianization) may in future be brought into his *n*-categorical vision (personal communciation).

Although from Arnol'd's declaration of support for Poincaré's style over Hilbert's, characterised often, though rather inaccurately, as the geometric over the algebraic, one might guess that he would disapprove of this synthesis with category theory, he does remark that:

> The main dream (or conjecture) is that all these trinities are united by some rectangular "commutative diagrams". I mean the existence of some "functorial" constructions connecting different trinities. (Arnold 1997: 10)

This is important. Just because someone aligns themselves with the Poincaré style does not mean they are averse to working rigorously.

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to replace a Fourier transform is one of the big challenges. Large parts of mathematics are concerned with how to generalise dualities in nonlinear situations. Physicists seem to be able to do so in a remarkable way in their string theories and in M-theory…understanding those non-linear dualities does seem to be one of the big challenges of the next century as well." (Atiyah 2002:15)

Rather there is a fear on their part that the algebra will come to dominate and so cut us off from an immensely powerful source of ideas.

5. Real Mathematical Kinds

So we have a series of interrelated schemes covering large tracts of mathematics, but what to make of them? There is a temptation to relate these forms of mathematical classification to types of classification in other sciences: physical particles, periodic table of chemical elements, animal species. From there it is a short step to the idea that something like a notion of *real kinds* is operating in mathematics. Perhaps, then, it would be fruitful to explore the notion of 'quasi-real kinds'.

But there is nothing new here you might say. Don't we already have philosophers treating this matter? Well, certainly there are so-called 'realist' and 'nominalist' positions in contemporary philosophy of mathematics. However, recent realist/nominalist debates concerning mathematics have become largely between a blanket nominalism and a blanket realism. Hartry Field, an ardent nominalist, makes very little of the distinction between a cooked-up concept and a concept vital for the life of mathematics, between a concept of no interest and one first glimpsed in the course of some exploration, then years later brought into sharp focus, one that's then rediscovered independently a dozen times, one that systematises large domains and fits into many stories. Instead, Field restricts himself to mathematics directly mentioned in scientific theories.

The extent of philosophers attending to the notion of 'getting concepts right' is all but exhausted by the continued activity to refine the notion of a set, and by talk of whether the elaboration of the notion of number has reach its final stage:

> Stated in realist terms, the extended number system [of the complex numbers – DC] is presumed in effect to stake out a 'natural kind' of reality. Far from 'carving reality at the joints', however, the system can be shown to feature a flagrantly gerrymandered fragment of heterogeneous reality that is hardly suited to enshrinement at the centre of a serious science like physics, not to mention a rigorous one like pure mathematics. Couched in these ultra-realist terms, the puzzle might be thought to be one that someone with more

> pragmatic leanings–the system works, doesn't it?–need not fret over; and in fact such a one might even look forward to exploiting it to the discomfort of the realist. Fair enough. I should be happy to have my discussion of this Rube Goldberg contraption (as the extended number system pretty much turns out to be) serve as a contribution to the quarrel between anti-realist and realist that is being waged on a broad front today (Bernardete 1989: 106).

I quote from Bernardete at length because he covers many of the themes to be treated below. Not that I think he gives the complex numbers a decent run for their money. If you pass them off as pairs of real numbers, each of which is a Dedekind cut of rationals, each of which is an equivalence class of pairs of integers, each of which is an equivalence class of pairs of natural numbers, then gerrymandering is easy to argue for. In what I hope to be a more sensitive way, all of the chapters of my book (Corfield 2003) deal, with varying degrees of explicitness, with the possibility of there being real kinds in mathematics.

One of the key case studies (chap. 9) concerns the question of whether groupoids are a good way to generalise groups. Here are two definitions of the concept:

> (1) A groupoid is composed of two sets, A and B, two functions, a and b, from B to A, and an associative partial composition, $s{\cdot}t$, of pairs of elements of B with $a(s) = b(t)$, such that $a(s{\cdot}t) = a(t)$ and $b(s{\cdot}t) = b(s)$. Furthermore, there is a function, c, from A to B such that $a(c(x)) = x = b(c(x))$ and such that $c(x){\cdot}s = s$ for all s with $b(s) = x$ and $t{\cdot}\ c(x) = t$ for all t with $a(t) = x$. Finally, there is a function, i, from B to B such that, for all s, $i(s){\cdot}s = c(a(s))$ and $s{\cdot}i(s) = c(b(s))$.
> (2) A groupoid is a small category in which every arrow is invertible.

Rather like translating a piece of French text into English, we end up with a much more concise definition, even taking into account the need to define what a category is.

The debate surrounding groupoids resembles one between three parties who when told about the coining of a certain term, one says it's like those philosophical chestnuts 'grue' or 'emeroses', or my

'cubeprime' – i.e., having no scientific purpose, another says it's like 'tree' or 'bush' – not a natural, intrinsic term, but one which has its uses, and the last says no – it's like copper – a real kind. You can see the point of advocates of the first position: if I kludge together the monster group (an object with 10^{52} arrows) and an equivalence relation representing all the people within 100 metres of you, partitioned according to the initial letter of their family name, then the resulting groupoid feels far from natural.

The middle position now chips in: Yes, groups are where it's at when it comes to symmetry measuring. However, there are advantages to working with groupoids. For example, maps from one group to another group don't form groups, whereas maps from a groupoid to another do form a groupoid. So it's worth working with (the category of) groupoids, even if one is only interested in groups.

The final party now unleashes the full force of its persuasive weaponry on you. Groupoids are found to lie at the intersection of a large number of schemes: Connes' noncommutative geometry (groupoids to help with bad spaces which resist classical topology/geometry); Grothendieck's notions of space;[4] Baez and Dolan's *n*-categories. They also tell you of the many independent discoveries, about their use in crystallography, energy levels of atomic electrons. (See chap. 9 of Corfield 2003 for the details.)

It is important to note that a presupposition made by all parties to these debates is the recognition of a distinction between gerrymandered concepts and those 'good for the life of mathematics', even if they disagree about where the boundary lies. Weil (1960) talks of sculpting from porphyry when working on analogies between function fields and number fields, and moulding with snow when giving axioms for uniform spaces. Philosophers cannot choose to ignore this difference. Following this lead they will encounter further questions: Is 'good for the life of mathematics' a mind-independent quality or connected to the ways in which our embodied minds work or have been trained to work? Are mathematicians engaged on a never-ending quest, or do we foresee the arrival at a terminus?

6. Tradition

4 See Cartier 2001 for ideas about how to unify these two visions.

I'm often asked what bearing my work has on "traditional" questions in the philosophy of mathematics. When given limited time to respond I feel pulled by two seemingly contradictory trains of thought: philosophers are allowed to ask new questions; these are not new questions. Allowing myself a little extra elbow room, I'll also suggest a synthesis.

(a) Questions change with the times

One defense against the charge that one is cooking up a philosophical problem out of nowhere is the thought that philosophical questioning must adapt itself to the state of development of a field of knowledge. It wasn't likely that the mathematician-philosophers of around 1900 would be talking about real kinds, the notion of function and number having been radically transformed through the nineteenth century (Gray 1992). They were just happy to make advances towards an all-embracing definition of these types of entity. After Hilbert's discovery of hidden assumptions in Euclidean geometry, a further point of concern was the security of extant mathematics. This stimulated a drive to construct a common language to embrace all of mathematics, which at the same time provided mathematicians with a sense of definitively establishing truth, is thus understandable.

Of course, there were questions then of organization, Hilbert himself playing a major part in these. But a hundred years down the track and many thousands of tons of mathematics papers later, the pendulum has swung very much towards concerns about organization rather than certainty, even with the advent of computer assistance.

Similarly in the natural sciences, philosophical questioning depends on the state of play. Locke's talk of real essences coincides with the rise of Boyle's corpuscularism: Is there a real essence to that yellowish, dense metal we call gold? According to Kornblith (1993), Locke gives two nominalist responses: there's no satisfactory empirical evidence for real essences, and in principle we could never know real essences. What happened in subsequent centuries has a bearing on this matter. For most, Locke is vulnerable to what we have found out about the world, that is, we do now have evidence for gold having a real essence. His '*a priori*' argument shows a lack of imagination rather than

contact with conceptual necessity. It is perhaps not accidental that natural kind talk crops up again in the 1960s when a good account of the periodic table had been made available by quantum mechanics.

As another example of historical parochialism, one case Locke makes against there being natural kinds in the animal kingdom is the lack of gaps or chasms between species. Some sea-birds dive, some fish fly. Even if one allowed that at the high point of set theoretic reductionism there was nothing to prevent interpolation between any two entities, this reveals itself as a parochialism. Interpolation is heavily constrained: groupoids have to fight for the right to be seen as properly interpolating between groups and equivalence relations. We have to look behind the scenes to justify kinds, just as we now have good reason to think of diving birds as very like sparrows, and flying fish as very like cod.

(b) This is a traditional question

The second defence against charges of not being "traditional" is to show that 'real kind' talk has always been there. Listen to Frege:

> [Kant] seems to think of concepts as defined by giving a simple list of characteristics in no special order; but of all ways of forming concepts, that is one of the least fruitful. If we look through the definitions given in the course of this book, we shall scarcely find one that is of this description. The same is true of the really fruitful definitions in mathematics, such as that of the continuity of a function. What we find in these is not a simple list of characteristics; every element is intimately, I might almost say organically, connected with others. (Frege 1950: 100)

Had he been talking about science, these might have been words spoken by the realist philosopher of science Richard Boyd.

If 120 years is not enough and Frege's authority insufficient, we can also appeal to Plato, and by doing so restore the term 'Platonism' to something closer to the intricate cluster of ideas contained in his works. For Plato, it is not any old 'mathematical' notion we seek. Rather, through dialogue we question the first principles, including the definitions, so as to sharpen them and get them right. In *The Republic* the ideal education for the future philosopher-king is given as follows:

Non-systematic education to age 18: play, sport, music.
18-20 military training
20-30 mathematics
30-35 dialectical discussion
35-50 practical experience of military and administrative offices
50-55 dialectic.

During the 30-35 period, you discuss the definitions and hypotheses which geometers merely assume. This would lead, presumably, to something like Euclid's later definition of the line: "A straight line lies evenly on its points". In a sense, then, the 'true' Platonist of the 20th century was Lakatos:

> As far as naïve classification is concerned, nominalists are close to the truth when claiming that the only thing that polyhedra have in common is their name. But after a few centuries of proofs and refutations, as the theory of polyhedra develops, and theoretical classification replaces naïve classification, the balance changes in favour of the realist. (Lakatos 1976: 92n)

(c) A more honest answer

I could compose a more complete answer which recognises that both the above components, attaining epistemic security and getting concepts right, have been given philosophical attention since Plato, that even at times where one is given greater consideration, the other persists, that the perception of the relationship between the components alters and has been influenced by the state of mathematics, and that the time is now ripe for a change of emphasis. A happy composite answer, but it is not a totally honest one, as I shall explain.

The quotations from Lakatos surprise many philosophers when I show them. Anyone who knows Lakatos's work thinks of his emphasis on continuing growth of knowledge in the Popperian World 3. Most would imagine that for Lakatos the evolution of concepts never ceases. However, as chapter 7 of my book relates, Lakatos cannot see how concepts evolve once they have been defined precisely in a formal or quasi-formal language. When a proof is given in this language, we can

only find logical mistakes or that the language is inconsistent. I believe I have shown a way of getting around this obstacle in chapters 8 and 9. We should not be talking about arriving at a definite end, but about an ongoing elaboration of ideas of number, symmetry, space, etc. By contrast to Frege's claim that with the entry of imaginaries we reach the 'natural end of the domain of numbers', we are dealing here with a more open-ended sense of conceptual connectivity. Listen to Arnol'd's praise for Sylvester and his sense of the elaboration of ideas:

> Sylvester (1876) already described as an astonishing intellectual phenomenon, the fact that *general statements are simpler than their particular cases*. The antibourbakiste conclusion that he drew from this observation is even more striking. According to Sylvester, *a mathematical idea should not be petrified in a formalised axiomatic setting, but should be considered instead as flowing as a river*. One should always be ready to change the axioms, preserving the informal idea.
> Consider for instance the idea of a number. It is impossible to discover quaternions trying to generalise real, rational, complex, or algebraic number fields. (Arnol'd 1999a: 2)

This sounds very Lakatosian, but Arnold is not denying the need for a rigorous formal setting, just that one musn't forget the informal ideas animating them. So just because the informal notion of symmetry gets axiomatised as group theory, does not prevent us from continuing to elaborate the notion. Indeed, the emergence of the groupoid concept is a stage on the path of its elaboration. A separate line of elaboration leads us to the quantum group and Hopf algebra concepts. These paths converge in the notions of quantum groupoid and Hopf algebroid.[5]

Of course, along the way of an elaboration we may hit upon some stable concepts. Just because mathematicians are reaching out beyond groups does not mean that the group concept will become obsolete. As for the long run, although we might completely change our classification and decide to take another path than has been taken to date, it is worth considering the possibility of achieving a certain stability. In any case, at any given moment, the evolving concept offers us a collection of natural kinds, so we may attempt to make contact with the

5 Brown (2004: 11) presents a very intricate picture of the elaboration of a cluster of concepts including symmetry.

philosophy of science literature on this topic. Kinds are treated there in many ways, for example, in terms of *projectability*, *reference*, and *promiscuity*.

7. Kind Properties

(a) Projectability

> (…) the properties we take to be essential to a kind are those we project. Indeed, the function of the essence placeholder in our cognitive economy just is to drive inductive inference: we conceptualize kinds in such a way in order to separate the properties of the members of a kind which are projectable from those that are not. We are aided in this task by our ability to detect clustered covariation. (Kornblith 1993: 106).

If a zoologist wandering through the Amazonian rainforest chances upon an animal of a species previously unknown to science, which is suckling its young, they know without further ado an immense amount about what to expect about its bone structure, internal organs, bodily functions, genetic constitution. What of a mathematician stumbling across something behaving rather like something he already knew? This is what happened to Euler when he encountered the function sin x/x and reckoned it should behave much like a complex polynomial. Like complex polynomials, but unlike some other functions, such as sin $(1/x)$ it has non-accumulating zeros, unlike others, such as tan x/x, it has no poles, and unlike others, such as exp x, it has symmetric behaviour at plus/minus infinity. On the other hand, the function tends to zero at plus or minus infinity unlike a complex polynomial. Is this difference significant?

Now, complex polynomials have roots to match their degree. Decomposing them into a product of $(1 - x/\text{root})$ times a constant is possible. What is it about them that allows this? I can construct a proof which shows me that any nonconstant polynomial has a root. Then given this first root, I divide by $(1 - x/\text{root})$ and continue the process. Have we got at the 'reason' for this decomposition phenomenon? Might the properties 'responsible' for this behaviour be found in other situations, perhaps with the function sin x/x. Would we know when to expect the

phenomenon to occur in anything other than polynomials?

In chapter 5 of my book, I discuss Polya's 'Bayesian' conception of mathematical plausibility. That sin x/x should decompose like a complex polynomial is plausible because of their shared properties. But there is more. What we have here is a 'hope for a common ground' to account for this decomposition, an explanation for its occurrence. This emerged in the result: all entire functions are of the form $A\, z^m \exp(h(z))\Pi (1 - z/\text{root}) \exp ((z/\text{root}) + (z/\text{root})^2/2 + \ldots + (z/\text{root})^n/n)$, the product being taken over all roots, for entire $h(z)$. From this story, being an entire function seems to be the relevant natural kind, just as the class of mammals was for the zoologist.

We shall also want to place our new species within the current hierarchical classification. We have just spoken of entire functions within the ring of analytic functions in one complex variable. These in turn are an instance of a more general notion which includes collections of algebraic integers, e.g., Z[i], factorisation in which we touched upon in our discussion of $13 = 2^2 + 3^2$. Behind this lies the grand analogy I treat in chapter 4 of (Corfield 2003).

Being something in my pocket is not a natural property. All the coins there may be silver, but this doesn't mean that for any coin were it in my pocket it would be silver. These properties do not 'gel' in some sense. Similarly, our zoologist noticing that the first twenty examples of the new mammal he has spotted have a bald patch in their fur, does not infer that the whole species will have this property, but rather postulates a local skin infection. In mathematics, all the hundreds of millions of non-trivial Riemann zeta zeros found to date have imaginary part ½, but it need not be the case that all of them have this property. The property of 'having been found to date' does not gel with being a zero, nor does being less than a certain size. This 'gelling' is something philosophers should work on.

(b) Making referential contact

Kinds are also used to account for our ability to establish reference without getting it quite right, so that we can say of J.J. Thomson's electron that it is the same as our electron, even if he mistook some of its properties. In his (1983) Hacking had argued for an 'entity realism' rather than a 'theoretical realism'. We are to identify entities by our

ability to manipulate them. Some of our ways of producing streams of electrons are derived from Thomson's techniques. However, as several philosophers have pointed out there must be some kind of connection between our earlier and later theoretical grasp of these entities, otherwise how would you know you were manipulating the same sort of thing. On the other hand, merely counting shared properties is not enough. For Aronson, Harré and Way (1994) achieving verisimilitude is not about getting more properties right than wrong, but about not straying too far from the correct kind in your hierarchical classification of what there is. At a party, have I successfully referred to the man over there holding a gin, if I point and say "See that woman with the vodka"? They would be close to each other on most people's ontological hierarchy of kinds. With "See that gorilla over there with the gun", the situation is not clear-cut. Both are primates holding artefactual objects. But if I say "see that car-crash taking place", unless my audience is highly adept at thinking metaphorically, perhaps the man's life is being ruined by alcoholism, they'll wonder what I'm on about.

Something similar occurs in mathematics:

> (…) three areas of mathematics and physics, usually regarded as separate, are intimately connected. The analogy is tentative and tantalizing, but nevertheless fruitful. The three areas are eigenvalue asymptotics in wave (and particular quantum) physics, dynamic chaos, and prime number theory. At the heart of the analogy is a speculation concerning the zeros of the Riemann zeta function (an infinite sequence of numbers encoding the primes): the Riemann zeros are related to the eigenvalues (vibration frequencies, or quantum energies) of some wave system, underlying which is a dynamical system whose rays of trajectories are chaotic.
> Identification of this dynamical system would lead directly to a proof of the celebrated Riemann hypothesis. We do not know what the system is, but we do know many of its properties, and this knowledge has brought insights in both directions (...) (Berry & Keating 1999: 236)

If Berry and Keating find their system, even if it does not possess all the properties they expected, so long it is in the right kind of place in the hierarchy of mathematical entities, we might still say that they had already made reference to it and knew how to manipulate it. A

fascinating case to study here would be when it could be said that the monster finite simple group had been referred to.

(c) Promiscuity

We saw several organisational schemes in mathematics from Arnold, Atiyah, and Baez and Dolan. Are they pointing to some ultimate classification, or are there a vast range of schemes shaped by our interests. For instance, computational complexity classes may be seen as not intrinsic characteristics, but as part of a classification due to an interest in what we can compute with our present kinds of technology. In the philosophy of science, John Dupré is a representative of this way of thinking. He is a realist – science aims at mind-independent features of the world - but he is a *promiscuous* realist – there are many different classification schemes. E.g., regarding the animal kingdom we categorise according to agricultural, culinary, zoological interests. All classifications pick up real features, but these features are laden with our interests. Is this a realism worth having? Don't we want a *relatively chaste* realism?

In his *Natural Kinds*, Wilkerson (1995) argues for privileged sorts of classification, ones which are not human-interest related. He does so by invoking Boyd's conception of homeostatic cluster of properties: animals have systems to maintain body temperature, cell walls work to maintain pressure from inside the wall in equilibrium with pressure outside. Properties work together to maintain themselves. Causality is in air, and yet recall Frege's comment about fruitful mathematical concepts quoted above, "What we find in these is not a simple list of characteristics; every element is intimately, I might almost say organically, connected with others."

In mathematics, we often hear that such-and-such a construction has been taken to be an example of an *X*, but that we get to its essence, how it is *in itself*, if we think of it as a *Y*. John Baez (2004) tells us that although mathematicians have taken the spectrum of a ring as a set, it is properly viewed as a groupoid. But what is the right way of talking here? Why not just say there are two different ways of thinking of a spectrum, one of which may interest a mathematician more than another? The point is that so often one way just turns out to fit much more satisfyingly and to lead to powerful new ways of taking research

further.

Likewise, Wilkerson wants to stress that the biologists' classifications are privileged ones, which cut far more deeply into nature than do the chef's. The promiscuous realist could respond that there is still human interest at play in our concern with those parts of the living world where speciation operates. The vast majority of organisms are bacterial, and so-called 'lateral gene transfer' between unicellular bacteria of different genealogical descent is common place. Gould claims that unicellular bacteria as "the true dominators of earth and rulers of life" (2002: 301), so why devote so much time to large mammals? Surely, it's because they are like us that we are so interested in them. Then what of our predilection for visible over dark matter?

Similarly, in mathematics, Robert Solomon points out that, despite the fact that the majority of finite groups are nilpotent of nilpotence class at most 2, "experience shows that most of the finite groups which occur "in nature" – in the broad sense not simply of chemistry and physics, but of number theory, topology, combinatorics, etc. – are "close" either to simple groups or to groups such as dihedral groups, Heisenberg groups, etc. which arise naturally in the study of simple groups." (Solomon 2001, 347). What interests determine how we distribute our attention over the collection of all groups?

8. Conclusion

What I hope I have managed to show in this paper is that ongoing debates in the philosophy of science could find parallels in philosophy of mathematics every bit as nuanced. For the foreseeable future, the greatest benefit will come from philosophers of mathematics dipping into the philosophy of science literature, but the issues in mathematics are often crisper, and one should expect a rapid pay back. Opportunities have already been missed. The philosophy of science could have saved itself much effort if it had thought to read the work of George Polyà on encoding plausible reasoning in a probabilistic calculus (see chapter 5 of Corfield 2003).

EMPIRICAL INFERENCE GROUP
MAX PLANCK INSTITUTE, TÜBINGEN

DCORFIELD@TUEBINGEN.MPG.DE

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