# *Dynamics of Mathematical Reason*

Abstract
Michael Friedman's rich account of the way the mathematical sciences ideally are transformed in his *Dynamics of Reason* affords mathematics a more influential role than is common in the philosophy of science. In this paper I assess Friedman's position and argue that we can improve on it by pursuing further the parallels between mathematics and science. We find a richness to the organisation of mathematics similar to that Friedman finds in physics.

Keywords: Mathematics, mathematical sciences, Michael Friedman, Dynamics of Reason

It is always interesting to inspect new philosophical conceptions of the mathematical sciences to ascertain the space their authors have accorded to the purely mathematical components. All too often philosophers of science implicitly buy into the logical empiricist stance that mathematics is a branch of logic, broadly speaking, and thus a transparent language whose involvement in scientific theories in no sense frames or mediates our understanding of the world. Even those more sophisticated philosophers who have left behind a naïve empiricism to examine the mediating effects of our instruments and models have little to say to us on the subject of mathematics. On the other hand, when the logical empiricist attitude to mathematics is rejected and the use of mathematics is taken to involve something more than the use of a logical language, this largely amounts to a kind of literalism which worries about our being committed to the sorts of abstract entities physicalists take not to exist. Philosophies which find in the application of mathematics something of significance other than a troublesome problem are fairly rare, and experience shows that most of these owe considerable allegiance to Kantianism.

In his book *Dynamics of Reason*, Michael Friedman (2001), philosopher of science and Kant enthusiast, has provided us with a rich, synthetic picture of how science should proceed, which accordingly pays mathematics far more respect. To fully appreciate this picture, there is of course no substitute for reading the book. Within the limited space of an article the quickest way to indicate what is involved is to sketch his account of the arrival and bedding down of Einstein's General Theory of Relativity, the 'exemplar', we might say, of Friedman's scheme. Starting out from Newtonian gravitation, which was written in the mathematical language of the calculus within the setting of Euclidean geometry, and which had received its meta-scientific grounding in Kant's philosophical system, through the nineteenth century mathematicians devised new forms of geometry and corresponding forms of calculus. With the resources of this language made available, Einstein could give new constitutive laws for the physics of the cosmos, in such a way that Newtonian cosmology could be reinterpreted as approximating one of a broader set of empirical possibilities. Although this work of reinterpreting the earlier theory is an essential part of scientific work, Friedman is enough of a Kuhnian to maintain that a pre-revolutionary advocate might find the theory quite incomprehensible when presented in these novel terms.

What has been achieved by this re-interpretation, Friedman terms 'retrospective communicative rationality'. But this does not satisfy him. What is especially novel in his account is his depiction of the means by which not just *retrospective* communicative rationality is achieved, but also a *prospective* version. In the case of general relativity, the associated meta-scientific work was carried out by Helmholtz, Mach, and Poincaré, stretching the Kantian schematism in light of the transformations of geometry by Riemann, Lie and Klein, and in Helmholtz' case his own psychophysical research. Poincaré's meta-scientific work was conducted in the context of his conventionalist philosophy. After the Einsteinian revolution, further meta-scientific work went into bedding the theory down, thereby sparking off a novel philosophy. In this case the role was played by Schlick, Reichenbach and Carnap.

For Friedman, when all of these component parts pull together, science is working at its best. It is a truly optimistic picture, which gives you a glow of pride for ploughing the noble furrow of philosophy, a discipline that could make a contribution to one of mankind's greatest intellectual achievements. Before we get too carried away, however, an obvious concern presents itself here that there have not been too many of these successes, especially in recent decades. We need to see, then, how other 'revolutions' fare. It is, we might say, a constitutive principle of Friedman's scheme that only the mathematical sciences will feature, although he does find scope to bring Darwinian evolution into the story. In the case of the original Newtonian revolution, we have: the invention of the calculus and its later development by Euler *et al.*; the meta-scientific spadework being done by Galileo, Descartes, Leibniz and Newton himself. Then, there is Kant, of course, at the other end of the revolution giving a philosophical shape to Newtonianism and separating philosophy from natural science in the process.

But what of more recent pieces of physics? What of quantum mechanics, surely by any account one of our most successful pieces of natural science?

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It is a commonly held view that all is not well with the interpretation of quantum mechanics, but where does the fault for this lie? Well, the mathematics was certainly quickly in reasonable shape. Indeed, the speed with which Weyl and von Neumann intervened is staggering. And had this revolution been philosophically achieved, that is, had it been another exemplary case of Friedman's scheme, we would have heard from him a rich tale of the development of mathematics, about how work on integral equations, spectral theory, group representation theory, Fourier analysis and its massive generalisation in the shape of harmonic analysis, Wedderburn structure theorems, von Neumann algebras, C*-algebras, and so on, provided a constitutive language in which the physical laws of quantum mechanics could be expressed.

Along with the new powerful mathematical framework came the ability to see how the earlier classical theories succeeded as well as they did in certain regimes. So, one cannot fault quantum mechanics for its retrospective communicative rationality. No, what seems to be missing in the quantum case relates to the forward movement. In other words, quantum mechanics has been let down by the lack of a meta-scientific framework. For Friedman, while quantum mechanics has been empirically successful, philosophical contributions have not been 'timely' (2001: 120-121). Now, there has to be something right about this, but I want to leave that to one side for the moment and return to the mathematics used in quantum theory to wonder what Friedman is to make of it. The mathematical path he has outlined for us in most detail, the one relating to the Einsteinian revolution (Riemannian geometry, Klein's Erlanger Programme, Hilbert's foundations of geometry) seems to lead inexorably to the logical empiricist view of mathematics as a part of logic plus the odd principle. We can afford to be generous and take set theory of

1930. The question arises, then, as to how the mathematics of quantum mechanics must go beyond this set theoretic framework for it to count as part of a Friedmanian alteration in the constitutive language.

Does anything about the development of the mathematics used in quantum theory mentioned above constitute a sufficiently transformation ? For instance, does the fact that von Neumann constructs a very different style of mathematical analysis, one which appeared so strange to G. H. Hardy in the mid-1930s that he could wonder aloud whether it was really mathematics, does this fact constitute an augmentation of the constitutive capacity of mathematics, even if in principle it can all be done in set theory? If not, I cannot see that there is enough of capacity for the kind of changes in mathematics that Friedman needs.

Let me try to apply more pressure on this point by carrying on the story of the quantum revolution to quantum field theory. Now, Freedman Dyson famously maintained that mathematics and physics divorced in the 1930s over the problems of dealing with the infinities that plague quantum field theory. Where mathematicians had previously had the resources in stock to deal with the problems posed them by physics, or at least they were not far from hand, quantum field theory had them stumped. Disliking the cavalier attitude of the physicists, they turned inwards, leaving the physicists to get on with things as best they could. Reconciliation came in the late 1970s with a realisation that both parties had interesting things to tell each other. We shall not subject this story to too much scrutiny here, save to point out that Soviet mathematicians might be said to have kept communication alive, but instead pass on to see how the couple are faring today. Well, here things are looking quite encouraging. Regarding the mathematically suspect

Feynman diagram calculations, we can see several proposals on the table as to how to bring them safely within the mathematical fold. Rather promising is Kreimer and Connes' attack on the renormalization group and the discovery of a Hopf algebra closely resembling one Connes had earlier found in his noncommutative geometry programme.

Perhaps, noncommutative geometry will be part of something larger, the Holy Grail of a quantum gravity. Certainly, connections have been made to string theory. String theorists avoid some of the notorious infinities by passing from the Y-shaped particle interactions of the Feynman diagram to the smoothness of an upside-down 'pair of pants' cobordism. Now, while string theory and more broadly conformal field theory are using plenty of category theory, this is also the case for the other leading contender in the race to a consistent quantum gravity, which uses, if anything, more of the stuff, and, partly inspired by Grothendieck, higher-dimensional versions at that. I am referring to work in the loop quantum gravity paradigm. Here, Feynman diagrams are 'categorified' into spin foams. Space-time emerges from a weave of representations (in the mathematical sense). Now string theorists are glancing over and taking note of 2-category theory in an attempt to get at a non-abelian string theory.[1] There is an extraordinary ferment of new ideas.

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Let us now get to the crux of the matter. I have mentioned these developments to pose the following question of Friedman's scheme:

[1] See Baez and Schreiber (forthcoming) to see how advocates of loop quantum gravity and string theory are combining their efforts to produce the right mathematical language for theories of quantum gravity.

> If things work out and we end up with as good a quantum field theory, or even quantum gravity, as we could hope for, should we be led to take it as a confirmation of his scheme by saying that it has been achieved by mathematics broadening its constitutive principles?

What Friedman says concerning mathematics inclines me to think that he might answer 'No' to this question. Rather than any of the glorious mathematics mentioned above, Friedman makes what he admits to be a speculative suggestion that the quantum logic of von Neumann and Birkhoff may prove to be the breakthrough. So in his book, the only pieces of mathematics we hear about are Euclidean geometry, the calculus, Riemannian geometry, Hilbert's Foundations of Geometry, then the possibility of quantum logic. It seems as though for Friedman a change in our mathematical principles following the 'foundational' period must impact on either the set theoretic framework or the classical logic used with it. I think not. Even if quantum logic gets up and running, it is just going to be seen as one way of viewing the structure of orthomodular lattices, a piece of mathematics perfectly capable of being formulated in terms of set theory and good old classical logic. Nobody is going to start systematically ignoring the distributive law in the meta-language. Nobody will say "I have an orthomodular lattice which is boolean and I know that any orthomodular lattice is either finite or infinite, but I cannot say that my orthomodular lattice is either finite and boolean or infinite and boolean". No, if that line of von Neumann should prove to be a fruitful way to view quantum mechanics, it must be taken at an intra-mathematical conceptual level, for example, as being part of noncommutative geometry, or perhaps the category theorists will tell us it points to the right setting for quantum mechanics in some variety of monoidal category.[2] It is already at this level that we shall have to be able to speak of radical overhaul, just as while the discovery that the internal logic of a topos is constructive does not mean than Errett Bishop triumphed, one can claim with some justification that the very arrival of the topos notion marked such an overhaul. So, if Friedman cannot see this and other aspects of what has taken place in mathematics in the post-1930 era as already involving a radical overhaul, then I suspect his scheme is in trouble.

What this comes down to can be equated crudely with the question whether there are 'revolutions' in mathematics in the post-1930 era. It is perfectly convincing to talk of a century long revolution from 1800 onwards (see Gray 1992), but can we have revolutions post-Zermelo-Fraenkel, even if we do not exceed its bounds? Yes, I would say. I am glad to see that van Fraassen thinks it possible too (2002: 239n8), although significantly here too he alludes to classical logic being at stake when he invokes an article where a supposed revolutionary new logic - intuitionistic this time - failed. But we do not have to remain with the *potential* for revolutions, because we are going through one right now. My revolution like Gray's is a long drawn out and defuse affair. If after the revolutionary event the lines appear reasonably sharp, when you are in the thick of things it is more like being in a cloud.

Pierre Cartier, once a Bourbaki member, tells us:

> When I began in mathematics the main task of a mathematician was to bring order and make a synthesis of existing material, to create what Thomas Kuhn *called normal science*. Mathematics, in the forties and fifties, was undergoing

[2] See Baez *forthcoming* about this possibility. He claims there that "...quantum theory will make more sense when regarded as part of a theory of spacetime" and that "...we can only see this from a category-theoretic perspective - in particular, one that de-emphasizes the primary role of the category of sets and functions."

what Kuhn calls a solidification period. In a given science there are times when you have to take all the existing material and create a unified terminology, unified standards, and train people in a unified style. The purpose of mathematics, in the fifties and sixties, was that, to create a new era of normal science. Now we are again at the beginning of a new revolution. Mathematics is undergoing major changes. We don't know exactly where it will go. It is not yet time to make a synthesis of all these things - maybe in twenty or thirty years it will be time for a new Bourbaki. I consider myself very fortunate to have had two lives, a life of normal science and a life of scientific revolution. (Senechal 1998)

The best article to consult concerning what he includes in this revolution is his 'Mad Day's Work' paper (Cartier 2001), which treats, and attempt to reconcile, Alexandre Grothendieck's and Alain Connes' work. In a similar vein, Yuri Manin's very interesting 'George Cantor and his heritage' (Manin 2002) points to higher categories as the new 'foundations', the term taken to mean "...the historically variable conglomerate of rules and principles used to organize the already existing and always being created anew body of mathematical knowledge of the relevant epoch." (p. 6).

One might have expected that philosophers would be crawling all over category theory. It is not everything, but as Peter May claims:

A great deal of modern mathematics, by no means just algebraic topology, would quite literally be unthinkable without the language of categories, functors, and natural transformations introduced by Eilenberg and MacLane in their 1945 paper. It was perhaps inevitable that some such language would have appeared

> eventually. It was certainly not inevitable that such an early systematization would have proven so remarkably durable and appropriate; it is hard to imagine that this language will ever be supplanted. It's introduction heralded the present golden age of mathematics. (May 2000:11)[3]

In Friedman's terms, May is presenting category theory as a constitutive language, but constitutive for other part of mathematics, rather than science.

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There seems to be an obstacle that prevents philosophers from coming to terms with post-1930s mathematics, and it is one even Lakatos never quite surmounted. He could never see where the dialectical excitement would come from once you are locked into a dominant formal system. It seems to me that Friedman is caught in a similar bind. We can see this by looking more closely at where Friedman takes the principal difference between mathematics and science to lie. One of the most important features of his scheme is the occurrence during a revolution of empirical laws becoming constitutive principles and similarly former constitutive principles becoming (approximately true) empirical facts. For example, what is a contingent fact of the Newtonian universe, that the inertial mass and the gravitational mass should be the same, becomes a constitutive principle of the Einsteinian picture. On the other hand, the constitutive lack of curvature of the Newtonian universe becomes an approximately true, but in places false, description of this universe. This, he claims, is where mathematics differs principally from the natural sciences (2001: 98).

[3] Using Google, one finds that the search 'revolution' + 'mathematics' often leads to Grothendieck's algebraic topology (where toposes originated). It also leads to Eilenberg and Steenrod's algebraic topology.

But we can find instances to parallel shifts in the status of laws between being empirical and being constitutive, even in post-30s mathematics.[4] By the mid-1940s, there were many ways of going from space to group: Cech cohomology, singular cohomology, simplicial cohomology, etc., and corresponding homology theories. On some spaces they gave the same answers, but on others they differed. With the axiomatisation of homology and cohomology theory by Eilenberg and Steenrod, which, as May suggests, could not have been written down without the language of category theory, what were contingent features of a bunch of ways of extracting algebraic information from a topological space became constitutive of what it is to be a homology or cohomology theory. Cech homology was found to not to possess one of these features, codified as the exactness axiom, and got 'improved' as Steenrod homology, although it was later revived as an example of a partially exact homology theory.

Let us look more closely at what happened to homology theories. Prior to the change topological spaces were defined as collections of points satisfying various axioms, although for the most part mathematicians dealt with subsets of $n$-dimensional Euclidean space. Again groups could be defined abstractly as sets satisfying certain axioms, but for the most part were realised as products of copies of the integers and its quotients, or perhaps as vector spaces over the rationals or reals. Processes called homology theories were defined to extract groups indexed by natural numbers with mappings between them. Comparisons were made between these theories as to how they acted on specific spaces, many coincidences being found.

[4] For an account drawing parallels between mathematics and science as regards laws and happenstantial facts see author's paper.

After Eilenberg-Steenrod, homology theories were natural number indexed families of functors satisfying certain axioms. This axiomatisation would not have been written without the new language of category theory. Something previously named a homology theory now had to pass a test to see whether it was *bona fide*. One of the consequences of the axiomatisation was that all *bona fide* homology theories would agree on certain basic spaces. Previously 'empirical', or 'quasiempirical' if you prefer, facts became either axioms or consequences of the axiomatisation. The old fact that there existed a homology theory behaving in a certain atypical way became false, Cech 'homology' was simply not a homology theory. Debates then ensued about what was the 'right' category of topological spaces to allow this new powerful functorial algebraic topology to function well.

Now this might be thought of as just a case of the rise of a new definition. Indeed, can we not see this episode as a case of what Ian Hacking (2000) takes Lakatos to be highlighting in his role as a 'deflator'? The empirical, or rather to use Lakatos's term, the 'quasi-empirical', has been made analytic by sufficient work on the definition of the concepts involved. But that is not the whole story on Lakatos. He is also someone who believed a mathematician, at least one who "...has talent, spark, genius, communicates with, feels the sweep of, and obeys [the] dialectic of ideas" (Lakatos 1976: 146), will contribute to getting things 'right':

> 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)

We are driving here at the notion of the proper organisation of concepts, in this case the right notion of mathematical space. In a given conceptual organisation, there are dependency relations which resemble very closely those Friedman detects in physics. These organisations can be overturned in revolutions.[5]

But still you might say that there is a difference. Going along with Friedman, Newtonian Gravitation can be recast retrospectively in the language of Riemannian manifolds, where it can now be compared with General Theory of Relativity. Evidence - say, observations on Mercury's advancing perihelion - is checked against them both, and the former is found wanting. When mathematics conducts itself analogously and recasts the past in a new framework, you do not tend to throw things away. Singular, Cech and simplicial cohomology all make fine cohomology theories, no single one is the right cohomology theory, and as I mentioned earlier even the aberrant Cech homology is still studied. But does this difference amount to much? Are we not seeing a shift in physics in any case to this mathematical way with the treatment of universality classes of models and of toy models?

Mathematical physicists are now happy with the idea that they need to study collections of models. The Ising model is surely inaccurate about the features of our world, false then, but it is the most computational tractable of a universality class, which includes models we would like to know about. Elsewhere, plenty of time is devoted to studying 2 + 1 dimensional models, in the search for insight into 3 + 1 dimensional ones.

[5] If one wishes to describe Lakatos as exploring the change of status from quasi-empirical to analytic, then the best way to my mind of understanding such 'analyticity' is through the lens of Alasdair MacIntyre's Thomistic-Aristotelianism, see especially (1998: 184-185).

And what of investigations into spaces of conformal field *theories* (the distinction between theories and models seems to be becoming blurred), and the dualities relating them, or of the idea of ensembles of universes? “Oh, that’s just mathematics dressed up as physics”, some say. Write it off if you will, but plenty of physicists work in this way.

All this is, of course, not to suggest that there are no philosophically significant differences between mathematics and physics. But one must be careful not to look to impose differences too early. If you were appointing a researcher to work on a whale physiology project, you would surely prefer someone who had made a thorough study of elephant physiology, but who had never seen the sea, to an oceanographer.

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To round out the picture, what prospects are there for meta-scientific philosophical activity to intervene in timely fashion to allow for quantum field theory, or quantum gravity, and belatedly quantum mechanics, to be properly achieved? Remember the life-line Friedman has provided for philosophers distressed by the thought that they might be drowning in a sea of ineffectiveness: Philosophy has played an indispensable role in the formulation of that crowning human achievement - the General Theory of Relativity - and it should expect to be able to make crucial contributions in the future. Looking at the meta-scientific level, whose resources have proved so shockingly wanting in the case of quantum mechanics, what do we have on the cards? For Friedman, after Carnap comes Kuhn , author of "our most sophisticated historiography". Perhaps, historians would find more conducive Peter Galison's interesting Peircean response to incommensurability in the final chapter of *Image and Logic* (Galison 1997)? Another way out would be to take up Kuhn's hint:

> the early models of the sort of history that has so influenced me and my *historical* colleagues is the product of a post-Kantian European tradition which I and my *philosophical* colleagues continue to find opaque. Increasingly, I suspect that anyone who believes history may have a deep philosophical import will have to learn to bridge the longstanding divide between the Continental and English-language philosophical traditions. (Kuhn 1977: xv)

This must surely be music to Friedman's ears, for who does Kuhn mean? Alexandre Koyré must feature, and beyond him lies Ernst Cassirer. Now, Cassirer is the one we are encouraged to return to at the end of Friedman's book *The Parting of the Ways* if we wish to glean from the Neo-Kantian tradition more than the pale shadow of it which was projected Westwards by the Vienna Circle Diaspora. Friedman in this book sees the origin of what Kuhn refers to as the "longstanding divide" in the differences between Carnap and Heidegger.

Of the philosophy which emerged from those who stayed in Europe we have Habermas's notion of communicative rationality, borrowed by Friedman. Elsewhere, van Fraassen (2002) appeals to Sartre's existentialist philosophy of the emotions to understand how we adopt revolutionary change. These are strange, but interesting times we live in, where philosophers of science themselves are experiencing Satrean emotions and so are reaching out further than they have for many decades. For my part, I think we could have saved a lot of time if we had only listened to Collingwood in, for example, his *The Idea of Nature*. Indeed, it seems to me that what he argues there is not so far from what Friedman is seeking.

But will any of these philosophies help us smooth the passage of the next revolution in mathematical physics? For my part, the only way they might is if they can help us revive the philosophy of mathematics. While I do not want to go so far as to claim that all hands should be set to the mathematical pump, I propose as an indication of the health of our philosophising we see whether mathematics is being taken seriously. For the past few decades we have been failing. The questions a philosophy of X should ask are not to be completely determined by the state of development of X, but there should be some genuine connection. For too long philosophical work on mathematics has let us down. Philosophers of mathematics are at last beginning to progress from alluding briefly in a footnote to category theory as another structuralist approach, to a more serious form of engagement, but the signs are, however, that it will be a long time before we come close to matching the meta-scientific work of a Poincare, a Mach, or a Helmholtz. I fear that all but a handful are at least fifty years behind, and the exceptions receive scant encouragement. We seem all too happy to keep philosophy of mathematics tightly bound to philosophical logic, philosophy of language, and analytic metaphysics. Our best hope, I believe, is to forge links to philosophers of science with the kind of vision displayed by Michael Friedman in *Dynamics of Reason*.

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