Philosophy of Mathematics[1]

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For many philosophers of science, mathematics lies closer to logic than it does to the empirical sciences like physics, biology and economics. While this view may account for the relative neglect of the philosophy of mathematics by philosophers of science, it ignores at least two pressing questions about mathematics. First, do the similarities between mathematics and science support the view that mathematics is, after all, another science? Second, does the central role of mathematics in science shed any light on traditional philosophical debates about science like scientific realism, the nature of explanation or reduction? When faced with these kinds of questions many philosophers of science have little to say. Unfortunately, most philosophers of mathematics also fail to engage with questions about the relationship between mathematics and science and so a peculiar isolation has emerged between philosophy of science and philosophy of mathematics. In this introductory survey I aim to equip the interested philosopher of science with a roadmap that can guide her through the often intimidating terrain of contemporary philosophy of mathematics. I hope that such a survey will make clear how fruitful a more sustained interaction between philosophy of science and philosophy of mathematics could be.

I.

The late nineteenth century and early twentieth century saw a sharp rise in philosophical discussions of mathematics. This can be plausibly traced to the continuing interest in Kant's philosophy along with the dramatic changes taking place in mathematics itself in the nineteenth century. Kant had argued that our knowledge of mathematics was grounded in our special access to the features of our mind responsible for our perception of objects in space and time. This appeal to pure intuition, as Kant called this special kind of representation, was meant to explain both the a priori and synthetic character of mathematical knowledge. Such knowledge was a priori because it could be justified independently of any appeal to perceptual experience. At the same time, it extended our knowledge in a way that a mere analysis of our concepts did not, so such knowledge was synthetic. Kant presented mathematics as the clearest case of synthetic a priori knowledge, but also argued that synthetic a priori knowledge was at the basis of any scientific knowledge or indeed of any knowledge. Mathematics was crucial to defending Kant's system of transcendental idealism according to which our knowledge is restricted to appearances whose features are due in large part to the nature of our mind. Later interpreters of Kant as well as his critics took pains, then, to account for mathematical knowledge and its relationship to the rest of our knowledge.

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For our purposes the most influential critic of Kant is John Stuart Mill. In his *System of Logic* (1843) Mill conceded that mathematics is synthetic or, as Mill would prefer to put it, involves real propositions. But Mill argued that mathematics is justified via ordinary perception and induction and so it is empirical and not a priori. This empiricism about mathematics combined a claim about justification with a position on the subject-matter of mathematics. For Mill, a claim like "7+5=12" is about physical regularities such as the result of combining 7 ducks with 5 geese to get 12 birds. Similarly, a geometrical theorem such as "Every triangle has interior angles summing to 180 degrees" is about ordinary physical objects whose shape is approximately triangular.[2]

While these philosophical positions were being articulated and debated mathematics itself was undergoing significant changes.[3] One view of these developments is that they took mathematics in a more abstract direction, away from the traditional arithmetic and geometry at the heart of Kant's and Mill's discussion of mathematics. This abstraction countenanced new mathematical entities and techniques that extended mathematical knowledge in new directions. The most well-known examples of this trend are the development of calculus or analysis as well as non-Euclidean theories of geometry. The clarification of the central concepts of these new areas of mathematics such as function, derivative, limit and dimension occupied the attention of leading mathematicians like Weierstrass, Cauchy and Dedekind. Collectively referred to as the rigorization of analysis, this trend was incredibly successful in providing precise definitions of previously vague concepts and in pushing mathematical research in even more abstract directions.

Standing at the confluence of these two streams of philosophy and mathematics are Frege and Russell and their respective versions of logicism. In *Foundations of Arithmetic* (1884) Frege argued that arithmetic was analytic in the sense that all arithmetical concepts could be defined in purely logical terms and all arithmetical theorems could be proved by making only logical assumptions. For Frege this project went hand in hand with the development of a new approach to logic. His earlier *Concept-script* (1879) provided the first system of polyadic predicate logic so that it became possible to logically articulate the difference between, for example, the true claim that for every number there is a number greater than it and the false claim that there is a number greater than every number ($\forall x \exists y\ x<y$ vs. $\exists y \forall x\ x<y$). In *Foundations* Frege focused most of his discussion on the natural numbers and presented several devastating objections to Kant's and Mill's philosophies of arithmetic. This cleared the ground for his own logicist proposals so effectively that neither approach to mathematics has had many contemporary defenders.

Frege's proposed definition for the natural numbers can be best approached by first considering a definition that he deemed flawed. This is to define the numbers by saying that they obey the following principle, commonly known as Hume's Principle:

(HP) $NxFx = NxGx$ if and only if $F \approx G$

[2] See Shapiro 2000, ch. 4 for discussion of Kant and Mill.
[3] An accessible summary of this period in the history of mathematics is Kline 1973, esp. ch. 43.

In HP "Nx" is a variable-binding operator that turns a concept expression like "Fx" into a name for the number of Fs. The relation symbol "≈" stands for a relation between concepts that obtains just in case the objects that fall under the concepts can be correlated in a one-one and onto fashion. Let us call this the relation of being equinumerous. For example, suppose our concepts are *x is a knife on the table* and *x is a fork on the table*. Then HP says that the number of knives on the table is identical to the number of forks on the table just in case the knives can be exactly paired with the forks. Nobody would dispute the truth of HP for our pre-philosophical concept of number, but Frege offered clear reasons for thinking that it is not an acceptable definition of the concept of number. HP leaves open which objects the numbers are, so in particular it does not settle the question of whether or not Caesar is identical with some number. Frege tried to solve this Caesar problem, as it is sometimes called, by picking specific objects that could be identified with the individual numbers in line with HP. This explicit definition deployed extensions of concepts, but we will put our discussion in terms of the set of things that fall under a concept. With this adjustment Frege's proposed explicit definition is

(ED) NxFx = G^(G ≈ F)

In words, this is the claim that the number of Fs is identical to the set of concepts that are equinumerous to the concept F. Using our earlier example, the number of knives on the table would then be a set that has as its members all the concepts that are equinumerous to the concept *x is a knife on the table*. Supposing that there were 8 knives, then this set would include the concept *x is a year strictly between 2000 and 2009* and the concept *x is a planet in the Solar System*.

To vindicate his logicism for arithmetic Frege had to defend the claims that ED involves only logical concepts and that it was sufficient to prove all arithmetical theorems using only logical principles. This is what he attempted in his *Basic Laws of Arithmetic* (vol. 1, 1893; vol. 2, 1903). The project failed because of Frege's reliance on certain principles of set existence that, in context of his strong logical system, rendered his theory inconsistent. The general problem is usually referred to as Russell's Paradox for it was Russell who communicated the problem to Frege in 1902 and who also sought to resolve it in a way consistent with a logicist philosophy of mathematics. To avoid the contradictions that ruined Frege's approach Russell developed his theory of types which places strong restrictions on which sets exist. These restrictions, in turn, forced Russell to deploy debatable axioms for his logical system like the axiom of infinity. It states that there are infinitely many individuals, where an individual is an object that is not a set. Russell needed this assumption to prove many mathematical theorems, but it is hard to justify the view that the axiom of infinity is a logical truth.[4]

Russell's paradox and similar foundational puzzles, along with the perceived failure of logicism, prompted an intense period of reflection on the foundations of mathematics, especially in the 1920s and 1930s. Following Frege and Russell, the participants in this debate seemed to assume that it was possible to give a comprehensive description of the nature of mathematics and that it was crucial to this project to explain how mathematics related to logic. The two main alternatives to logicism developed in

[4] The best collection of Frege's writings in English is Beaney (ed.) 1997. A thorough discussion of Frege's philosophy of mathematics is Dummett 1991. A recent survey of logicism is given by Demopoulos and Clark in Shapiro (ed.) (2005).

this period are the intuitionism championed by Brouwer and the formalist program advanced by Hilbert. Brouwer's intuitionism involved a return to many aspects of Kant's philosophy of mathematics, especially the focus on non-perceptual intuition and the constructive aspects of mathematics. Brouwer traced the foundational crisis to the failure to observe appropriate restrictions on principles like the law of excluded middle. In a proof by contradiction, for example, a classical mathematician may prove p by showing that $\neg$p entails a contradiction with antecedently known theorems. The intuitionist insists, however, that this inference assumes that p or $\neg$p is true, while there may be claims which have no determinate truth-value. Similarly, the intuitionist does not accept a proof from the premise that $\neg\forall$xFx to the conclusion that $\exists$x $\neg$ Fx. This is because the proof of an existence claim requires the production of the entity that witnesses the truth of the existence claim. Brouwer grounded these restrictions in his positive conception of the nature of mathematics based on a basic intuition of what he called "two-ity". This is tied to our temporal experience. These aspects of Brouwer's intuitionism proved difficult to motivate and later philosophers of mathematics influenced by Brouwer have sought to ground restrictions on classical reasoning by other means. The most influential instance of this legacy is Michael Dummett's criticisms of the use of the law of excluded middle in mathematics based on his views on meaning.[5]

The second main alternative to logicism in the foundational period is Hilbert's formalism. Hilbert granted the intuitionist the worry that reasoning about certain objects like sets of infinitely many things could be the source of the contradictions. But he sought to address this worry in a way that would preserve classical mathematics and its logical principles. Hilbert's strategy began by separating off a core body of real mathematics that was taken as the valid starting point for any further foundational investigations. A classical mathematical theory, for example, the theory of functions on the real numbers, could then be approached as the result of adding various symbols for "ideal" mathematics to the original symbolism expressing the real mathematics. Hilbert argued that the meaning of these new symbols could be ignored, leaving only formal rules for their manipulation, if it was possible to give a proof that the new theory was consistent using only the sub-theory of real mathematics. This philosophical program yielded a rich body of mathematical results in the form of proof theory. Here the mathematical symbolism was treated as a subject for mathematical study in its own right. The ultimate goal of these investigations was to show that the theories of classical mathematics lacked a configuration of signs amongst its theorems which would yield a contradiction, e.g. "0=1".

The viability of Hilbert-style formalism was called into question by Gödel's incompleteness theorems. The first incompleteness theorem applies to axiomatizable theories which include a certain minimal arithmetic, i.e. a set of sentences T that are derivable from of a set of axioms that include some minimal arithmetical claims Q. The theorem claims that if such a theory T is consistent, then it will be incomplete in the sense that there will be a sentence G in the language where neither G nor $\neg$G are derivable from the axioms of the theory. Gödel's second incompleteness theorem extended this result to show that for such theories, subject to some further fairly weak conditions, the sentences missing from T include an arithmetical claim Con(T) that is materially equivalent to the claim that the theory T is

[5] See Posy's and McCarty's contributions to Shapiro (ed.) 2005 for more discussion of intuitionism.

consistent. This convinced many that Hilbert's quest for a proof of the consistency of classical mathematics that would persuade the intuitionist to abandon her restrictions was impossible to fulfill. This is because a theory that was stronger than T was necessary to prove the consistency of T, and so any part of T restricted to real mathematics would not have the resources to prove the consistency of T. The case is not completely closed, however, and some philosophers of mathematics continue to champion more modest versions of Hilbert's formalism as an adequate philosophy of mathematics.[6]

II.

The central role of sets in the foundations of mathematics prompted some mathematicians to develop an axiomatic theory for sets that seemed strong enough to unify all of mathematics in a single framework. The most commonly discussed theory of this kind is ZFC (Zermelo-Fraenkel set theory with the axiom of choice). On this approach certain truths about sets are taken for granted and other mathematical entities like the natural numbers are identified with particular sets. If these axioms are adopted as the basis for mathematics, then it is quite tempting to view mathematics as the study of non-physical, abstract objects that exist outside of space and time. This is because there does not seem to be any physical interpretation of the axioms which renders them true. This proposal is often referred to as "set-theoretic platonism", although the associations with Plato's actual views are far from clear. In particular, few philosophers have been tempted to combine the view that the subject-matter of mathematics is a domain of abstract objects with the epistemic claim that our knowledge of the axioms of mathematics is based on a special faculty of reason or intuition. A notable exception here is Gödel's philosophy of mathematics as presented in "What is Cantor's Continuum Problem?". The Continuum Hypothesis (CH) is a claim involving sets that is easy to formulate, but which resisted proof or disproof using the axioms of ZFC. Eventually it was shown that CH is logically independent of ZFC. Contemplating this possibility, Gödel insisted that there was nevertheless a fact of the matter concerning the truth of CH and that mathematicians should deploy various methods to try to determine whether or not CH was true. These methods include further reflection on the concept of set itself: "the very concept of set on which [the axioms of set theory] are based suggests their extension by new axioms which assert the existence of still further iterations of the operation 'set of' " (Benacerraf and Putnam (ed.) 1983, p. 476).

Quine is the most influential proponent of a version of platonism for mathematics that tries to justify our mathematical knowledge empirically. This combination of views can be seen as a consequence of Quine's naturalism. Quinean naturalism is the view that all knowledge is scientific knowledge and so the only form of justification of any purportedly non-scientific belief is its contribution to the overall success of science. For mathematics, this led Quine to offer what has come to be called an indispensability argument for platonism. This argument proceeds by noting the crucial role of mathematical theories in our best scientific theories. As these mathematical theories, when suitably regimented in the language of first-order logic, entail sentences like "(∃x)(x is a number)", then our best science gives us empirical evidence for the existence of numbers. Quine rejects Mill's view that the

[6] See Detlefsen's contribution to Shapiro (ed.) (2005) for an in-depth discussion of varieties of formalism. See Smith 2007 for more on Gödel's theorems.

subject-matter of mathematics is physical regularities. He insists that the best interpretation of mathematical language is in terms of sets and that sets are abstract objects (Quine 1981).

Quine’s views on mathematics set the agenda for much of the philosophy of mathematics in the 1960s and 1970s. The central issue for the discipline became Quine’s claim that a platonistic interpretation of mathematical language in terms of sets was required or was in some way superior to a non-platonistic interpretation. Alternatives to Gödel’s and Quine’s respective versions of set-theoretic platonism received new life from two influential papers by Benacerraf. In “What Numbers Could Not Be” (1965, reprinted in Benacerraf and Putnam (eds.) 1983) Benacerraf questions the motivation for choosing to identify things like numbers with sets in this or that particular way. For example, the natural numbers 0, 1, 2, 3, … could either be identified with the series of sets $\emptyset$, $\{\emptyset\}$, $\{\{\emptyset\}\}$, $\{\{\{\emptyset\}\}\}$, … or with the series $\emptyset$, $\{\emptyset\}$, $\{\emptyset, \{\emptyset\}\}$, $\{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$, … (Here “$\emptyset$” refers to the empty set). Benacerraf argues that is was no mathematical reason to prefer one identification over another. This complicates the set-theoretic reduction of numbers to sets, at least if it is conceived of as revealing what the natural numbers were all along. Benacerraf concludes from these problems that not only is there no fact of the matter whether or not numbers are identical with this or that series of sets, but also that numbers are not objects at all: “in giving the properties (that is, necessary and sufficient) of numbers you merely characterize an *abstract structure* – and the distinction lies in the fact that the ‘elements’ of the structure have no properties other than those relating them to other ‘elements’ of the same structure” (Benacerraf and Putnam (eds.) 1983, p. 291). This enigmatic formulation inspired a variety of structuralist interpretations of mathematics. The most ambitious version of structuralism is eliminative structuralism. It aims to eliminate all reference to abstract objects from the interpretation of mathematical language by conceiving a mathematical claim C as a tacit conditional of the form

$$(\forall x)(\forall y)(\ldots)(A^{(x, y, \ldots)} \rightarrow C^{(x, y, \ldots)})$$

Here the axioms for the domain in question “A” and the original claim “C” have been amended so that, as the superscripts indicate, all the constants and predicate symbols are replaced by variables x, y, … . The resulting generally quantified conditional claim says that for any domain satisfying the axioms, the claim will hold for that domain. The eliminative structuralist tries to recast all ordinary mathematical claims in this way so that their truth does not require the existence of any abstract objects. This undercuts set-theoretical platonism, but only at the cost of ascribing a non-standard logical form to the statements of mathematics.

A further objection to eliminative structuralism motivates the platonistic or ante rem structuralism of Resnik and Shapiro (Resnik 1997, Shapiro 1997). Consider a purported domain that has no concrete instances such as Zermelo-Frankel set theory. Here the only non-logical symbol is the sign for the membership relation “$\in$”. So, the eliminative structuralist suggests interpreting the result R of ZFC as $(\forall x)(ZFC^{(x)} \rightarrow R^{(x)})$. But given that there are no concrete domains that satisfy $ZFC^{(x)}$ it follows that this conditional is vacuously true and so $(\forall x)(ZFC^{(x)} \rightarrow \neg R^{(x)})$ will come out to be true as well. This deprives the eliminative structuralist of an adequate interpretation of mathematics as it fails to preserve the truth-value of the original mathematical claims. The ante rem structuralist responds by positing abstract structures that will satisfy the axioms of the traditional theories of mathematics like number theory and set theory. This does not mark a return to set-theoretic platonism, though, because these structures are conceived of on the model of structured universals which are prior to the positions which play the role of traditional mathematical objects.

Ante rem structuralism is similar to traditional platonism to the extent that it faces a serious epistemic objection. In another influential paper "Mathematical Truth" (1973, reprinted in Benacerraf and Putnam (eds.) 1983) Benacerraf argued that a literal or standard interpretation of mathematics is strongly supported by the grammatical role of terms for mathematical objects like "five" and "seven". For example, the claim "There are at least three perfect numbers greater than 17" seems to be of the same logical form as "There are at least three large cities older than New York". But then "17" is a name for an object just like "New York". However, Benacerraf continued, if we adopt this standard interpretation, then we have difficulty explaining how we can refer to such objects and how we can know any truths about them or even that they exist. Benacerraf originally pressed this point using the then fashionable causal theory of reference and causal theory of knowledge. But the worry remains in force for many pictures of reference and justification. As Field summarized the worry, the epistemic challenge "is to provide an account of the mechanisms that explain how our beliefs about these remote entities can so well reflect the facts about them … if it appears in principle impossible to explain this, then that tends to undermine the belief in mathematical entities, despite whatever reason we might have for believing in them" (Field 1991, p. 26).

The theme set by "Mathematical Truth" was played out in many ways in subsequent philosophy of mathematics with philosophers finding ingenious strategies either to account for our knowledge of abstract objects or else to recast the logical form of mathematical statements so that mathematical knowledge could be assimilated to knowledge of logic and ordinary scientific knowledge. The most vigorous strand of the former strategy is neo-Fregeanism as developed by Hale and Wright among others.[7] Hale and Wright argue that Frege's HP definition was adequate after all and that Frege was mistaken in thinking that the further ED definition was needed. Neo-Fregeanism received significant support from a result known as Frege's Theorem. This is the fact that the theory of arithmetic given in second-order logic with HP as its only non-logical axiom, called Frege Arithmetic (FA), is sufficient to derive Peano Arithmetic (PA) (Boolos 1999). PA is typically thought to represent our knowledge of the natural numbers. Furthermore, FA can be shown to be consistent just in case PA is consistent. The main epistemic advantage of FA over PA is that HP is treated as a definition and so is supposed to be easier to justify than alternative axiomatizations of arithmetic. The success of this approach for the natural numbers has led to a sustained investigation of other principles like HP for other areas of mathematics such as the real numbers and set theory (Fine 2002, Burgess 2006).

The alternative strategy that retains its popularity is nominalism. Strictly speaking, nominalism for mathematics is just the claim that mathematics does not motivate us to accept the existence of any abstract objects. One version of nominalism achieves this result by adopting a non-standard interpretation of the logical form of mathematical statements. For example, Chihara treats mathematical statements in terms of the constructability of certain linguistic items (Chihara 1990), while

[7] See Hale and Wright (2001) and Hale and Wright's contribution to Shapiro (ed.) 2005.

Lewis offered an interpretation of these statements using the mereological apparatus of part and whole (Lewis 1993). Building on the eliminative structuralist proposal, Hellman considered taking mathematical claims to involve only the possibility of a certain kind of structure (Hellman 1989). All three stop short of countenancing abstract objects, but aim to assign objective truth-values to the right mathematical statements. They thus reject both Benacerraf's claim that mathematical statements should be given a logical form that matches ordinary statements and Quine's argument that the role of mathematics in our scientific theories requires a metaphysics of abstract objects. A second variety of nominalism argues that mathematics is not after all essential to the success of our best scientific theories. This kind of nominalist presents non-mathematical versions of these best theories and uses these versions to determine what we should believe exists. Here Field's proposal is the most developed, but we will defer a discussion of it until section IV. Finally, a more recent version of nominalism is typically dubbed "fictionalism". This is the view that even though mathematical statements have a literal content that accords with their standard logical form, they also have a non-literal or fictional content that we can use to fix our ontological commitments. Different fictionalists obtain their fictional contents in different ways. For example, Balaguer focuses on the causal isolation of abstract objects from the physical world (Balaguer 1998), while Yablo speaks more metaphorically of "the real-world condition that makes it sayable that S" (Yablo 2002, p. 229).

Despite these innovations, it is fair to say that many philosophers of mathematics would agree with Burgess' verdict in "Why I Am Not a Nominalist". Combining Quine's test for ontological commitment with Benacerraf's standard interpretation of the logical form of mathematical statements, Burgess ridicules philosophers who would impose philosophical standards to adjust the successful practice of scientists and mathematicians (Burgess 1983, p. 98). While the platonist-nominalist debate has reached a sort of standoff, new innovations continue to inject new life into these traditional positions. The appearance of Parsons' *Mathematical Thought and Its Objects* is perhaps the best example of this in some time (Parsons 2008). Parsons combines aspects of structuralism with a novel account of our intuitive access to what he calls quasi-concrete objects to try to overcome the persistent problems with a platonist interpretation of mathematical language.

III.

The debates between platonism and nominalism continued through the 1980s and 1990s, but by this time the philosophy of mathematics had come to seem to many to be too disconnected from the mathematics studied in most mathematics courses and pursued by most mathematicians in their research. Unlike the time of the original foundational crisis, these critics argued, philosophy of mathematics had drifted away from mathematics and risked losing track of ongoing developments within mathematics itself. A major strand of this "maverick" tradition appealed to the range of topics that were being pursued in the philosophy of science as an inspiration or even a model that could be imitated by the philosophy of mathematics. Lakatos' *Proofs and Refutations* remains one of the earliest and best examples of this trend (Lakatos 1976). Written in a lively dialogue style, Lakatos offered a kind of rational reconstruction of developments from the history of mathematics centered around the theorem that for any polyhedron, the number of vertices minus the number of edges plus the number of faces is identical to two. A central lesson of the dialogue, though, is that the different proofs offered for this theorem serve as much to clarify the central notions like polyhedron as they do to place some determinate theorem beyond doubt. Also, counterexamples to previous versions of the theorem make a positive contribution to the development of new theorems. In the philosophy of science Lakatos aimed to reconstruct scientific knowledge using his theory of progressive research programs and the similarities between the mathematical and scientific cases are significant. In both cases, our knowledge is the product of a historical process of scrutiny and innovation.

Lakatos' call to focus on mathematical practice and the history of mathematics was taken up with enthusiasm by Philip Kitcher in his *The Nature of Mathematical Knowledge* (1984) and the collection co-edited with Aspray *History and Philosophy of Modern Mathematics* (1988). On the one hand, Kitcher engaged with the foundational tradition by presenting a thoroughly empiricist interpretation of mathematics inspired by Mill. On this view, mathematics is the study of the operations of an ideal agent stripped of many of the limitations of ordinary human agents. Different mathematical theories correspond, then, to different conceptions of the abilities of this ideal agent. On the other hand, Kitcher unleashed a polemic against the methods typically employed by most philosophers working in the foundational tradition. Writing with Aspray, Kitcher identifies a "minority tradition" of philosophers, including Lakatos, who "share the view that philosophy of mathematics ought to concern itself with the kinds of issues that occupy those who study other branches of human knowledge (most obviously the natural sciences)" (Aspray and Kitcher (eds.) 1988, p. 17). This historical turn proved of more lasting significance than Kitcher's empiricist interpretation of mathematics. For while this interpretation seemed to be just one more attempt to give a non-standard nominalist spin on mathematics, the methodological turn that Kitcher argued for linked the philosophy of mathematics to the history of mathematics and so re-opened many philosophical questions of mathematics that had been overlooked for some time. A similar call for change can be found in the collection edited by Tymoczko, *New Directions in the Philosophy of Mathematics* (1986/1998). Here a range of different approaches to philosophy of mathematics are presented, each of which places an emphasis on the similarity between scientific methods of investigation and more recent developments in mathematics, as with the use of computers to prove the four-color theorem.

More recently, Maddy has pushed this turn to practice in new and interesting directions. She has developed her views under the aegis of a revised form of Quinean naturalism, beginning in *Naturalism in Mathematics* (1997) and continuing with *Second Philosophy: A Naturalistic Method* (2007). Maddy retains the traditional focus on set theory, but she criticizes Quine's view that scientific knowledge is to be the standard against which other areas of knowledge should be judged. Instead, she presents a more thoroughgoing naturalism which accords mathematical practice a default valid status. Maddy links this shift to a change in the sorts of questions that the philosopher of mathematics should be asking. Rather than trying to incorporate mathematics into our overall metaphysics and epistemology, philosophers should instead aim to articulate and clarify the standards internal to mathematical practice. For example, on the issue of which axioms should be added to ZFC to resolve CH (see section II), Maddy argues that philosophers should focus on the principles that set theorists themselves seem to be employing when they debate this issue. As a result, Maddy aims to reform philosophy of mathematics so that it is more descriptive in orientation.[8]

The most thorough presentation of this new approach to the philosophy of mathematics is Mancosu's edited collection *The Philosophy of Mathematical Practice* (2008). As he makes clear in his introduction, Mancosu aims to enrich the philosophy of mathematics by combining "local studies" on specific issues with a more global attempt to integrate these case studies into an epistemology and metaphysics for mathematics as a whole (Mancosu (ed.) 2008, p. 19). The focus on practice can be used to serve the same aims as the traditional foundational approaches, but the tools used to achieve these aims are completely different. Unlike the top-down foundational approach which focuses almost exclusively on set theory and its interpretation, and which seems to assume that all of mathematics works in the same way, the new bottom-up practice approach takes the variety of mathematics across sub-disciplines as a starting point and uses this to construct a more adequate metaphysics and epistemology for mathematics. The parallels with earlier developments in the philosophy of science are striking. For while mid-20th century philosophy of science had typically assumed that philosophy should study how science works in general, contemporary philosophy of science has developed into a series of different sub-disciplines focused more on the specific sciences like physics, biology, economics, etc. than on any overarching general lessons for how science as a whole functions. But few argue that this should displace traditional topics like realism, reduction or explanation. Instead, one would hope that these topics would now be pursued in a more enlightened and informed way.

The turn to practice indicated by Mancosu's volume has similar ambitions. This comes through clearly in Manders' discussion of the role of diagrams in Euclidean geometry. His aim is "to account for the justificatory success of diagram-based geometry" (Mancosu (ed.) 2008, p. 67) on its own terms. Manders' reconstruction of the value of diagrams tries to explain how the Euclidean geometrical tradition could be so stable for so long. The broader implications of this reconstruction are not drawn in his contribution to the volume, but it would provide a valuable input into any attempt to understand why mathematicians might have abandoned Euclid's methods or what epistemic shifts might arise across these different mathematical traditions. Similarly, in his own contribution with Hafner, Mancosu explores the status of explanation within mathematics. Mathematicians commonly distinguish proofs that are explanatory from those that are not, but it remains difficult to characterize the mathematical value of the pursuit of explanatory proofs. Hafner and Mancosu draw on a case study of the mathematician Brumfiel's work on real closed fields. Brumfiel explicitly indicates that he adopts a certain method of proof even though another method is available that would provide a more unified method of proof. This case is then used to undermine Kitcher's proposal that unification is the key to explanation not only in science, but also in mathematics. While this shows that unification is not always the source of explanatory power in mathematics, Hafner and Mancosu remain open to the possibility that more than one account of explanation may be necessary to explain how mathematicians do mathematics.

[8] See also Corfield 2003.

This rapprochement between the topics and priorities of philosophy of mathematics and philosophy of science naturally raises the more general question of how mathematics is both like and unlike a science. As we have already seen, there are general proposals like Mill's, Quine's and Kitcher's that align mathematics very closely with empirical science either in its subject-matter, methods of justification or both. What all these views have in common, though, is a conviction that mathematics and science as a whole can be characterized in uniform terms so that it makes sense to ask how the two disciplines are alike. By contrast, the turn to practice in the philosophy of mathematics and the philosophy of science suggests that we look for more local similarities and differences between sub-disciplines, or even more narrowly to particular historical episodes or contemporary methodological debates. This approach promises to deliver a more realistic picture of the links between this or that part of mathematics and science and should help us to improve our understanding of the remarkable success of both fields.

IV.

There is an additional point of contact between the philosophy of mathematics and the philosophy of science having to do with the role of mathematics in science. The contribution that mathematics makes to science is difficult to summarize and one suspects that mathematics might contribute in different ways to different parts of the scientific enterprise. Setting this complexity aside, both philosophers of mathematics and philosophers of science have sought to draw substantial conclusions from the central place of mathematics in nearly all contemporary successful science. After reviewing the respective debates on the mathematics and science sides, I will conclude by suggesting an additional, hopefully fruitful approach that would involve the expertise of both philosophical communities.

On the philosophy of mathematics side, most discussion has been concerned with Quine's indispensability argument for platonism. This argument received an influential formulation by Putnam in his *Philosophy of Logic* (Putnam 1979), but the most careful presentation is by Colyvan in his *The Indispensability of Mathematics* (2001). As Colyvan presents the argument, it has only two premises:

> 1. We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories;
> 2. Mathematical entities are indispensable to our best scientific theories.
> Therefore: 3. We ought to have ontological commitment to mathematical entities (Colyvan 2001, p. 11).

The first premise is supported by Quinean naturalism and another claim that Quine argued for, namely confirmational holism. This is the view that the success of a scientific theory leads to the confirmation of all of its sentences. Holism of this sort rules out any kind of selective confirmation that would assign differential confirmation to the mathematical and non-mathematical parts of the theory. This point has been questioned by Azzouni who argues that "posits" in science are treated differently depending on our epistemic access to them (Azzouni 2004). While other aspects of Quine's naturalism have been disputed (Maddy 1997), by far the most effort has been expended on the second premise. Notice that many nominalists can concede that mathematical language is indispensable to the presentation of our best scientific theories while still rejecting premise 2's claim that mathematical entities are themselves required. This is the response suggested by Chihara's, Lewis' and Hellman's different nominalist interpretations of mathematical language. Quine's response to these sorts of proposals is that they make use of non-logical resources like an appeal to modality that is illegitimate in the context of a proper regimentation of the language of science. This strategy is not very attractive for contemporary philosophers who reject many of Quine's views and Colyvan has at times conceded the point that the conclusion of the indispensability argument does not require a platonist interpretation of mathematics (Colyvan 2001, § 7.1, Pincock 2004).

A more aggressive reply to premise 2 is offered by Hartry Field's *Science Without Numbers* (1980). Field argues that mathematical language is only pragmatically indispensable to reasoning with our best scientific theories, but that there are non-mathematical formulations of these theories which we can use to determine our ontological commitments. Field's attempt to develop a non-mathematical version of Newtonian gravitational physics provoked an extended debate on the adequacy of such a formulation and the border between the mathematical and non-mathematical. He proceeded by presenting an axiomatic theory whose intended domain included only space-time points and regions. Scalar magnitudes like temperature or gravitational potential were then assigned to these points using physical relations like being greater in magnitude. Given certain axioms for these relations and the background space-time, Field then proved what is known as a representation theorem to the effect that these physical properties and relations can be accurately represented by assignments of real numbers to space-time points and regions. Field further claimed that he could prove that the original mathematical theory M+T was a conservative extension of his non-mathematical theory T. That is, for any sentence A that is entailed by the original theory M+T and that is formulated entirely in non-mathematical terms, A will also be entailed by T. This led Field to present T as a non-mathematical version of M+T.

Field's program was criticized in a number of directions (MacBride 1999). Some of the most important objections concerned the extension of Field's strategy to other sorts of scientific theories (Malament 1982), the acceptability of the logical resources deployed by Field (Shapiro 1983) and the nominalistic credentials of Field's background space-time (Resnik 1985). More recently, Pincock has argued that the physical assumptions of these non-mathematical theories are unwarranted by the evidence that is adequate to support the original mathematical theories (Pincock 2007). Still, Field's program prompted extended investigations into the prospects for non-mathematical scientific theories and offered indirect illumination of the positive role that mathematics has in science. The most thorough investigation of the possibilities for nominalizing science can be found in Burgess and Rosen 1998.

In his own response to Field Colyvan emphasizes the scientific benefits that traditional mathematical scientific theories tend to have over their purportedly non-mathematical rivals. These include the power to unify scientific theories (§ 4.4) as well as an increase in explanatory power (§ 3.3). The examples in the book prompted an exchange with Melia which has since expanded into an extended discussion.[9] The platonists in this debate seek to provide a positive explanatory argument for premise 2

[9] See Melia 2000, Colyvan 2002, Baker 2005, Bangu 2008 and Baker 2009.

in the original indispensability argument which parallels in many ways the explanatory "no miracles" argument championed by scientific realists like Psillos (Psillos 1999). For example, Baker has highlighted the scientific explanation of the primeness of the length in years of the life-cycle of some species of cicada to support such an explanatory indispensability argument. But as with anti-realists about science, anti-platonists have several responses. Bangu, for one, has complained that such an argument begs the question in favor of platonism because the explanandum is already given in a mathematical form. The issue has recently been taken up by Psillos himself who exploits the role of mixed abstract-concrete objects like the Equator and models like the linear harmonic oscillator to conclude that scientific realists should also be platonists to some degree (Psillos unpublished). Given that the realist needs some abstract objects, it is hard to see why mathematical objects are so objectionable.

Most philosophers of science have shown little interest in the indispensability argument for platonism, but ongoing debates about modeling and idealization have placed the role of mathematics in science on the philosophy of science agenda. A central issue in discussions of models is the relationship between models and theories and the metaphysical nature of models themselves. The semantic view of theories offered clear answers to these questions by identifying theories with collections of models that satisfied a certain range of descriptions and by identifying models with sets of the sort used in logic to interpret formal languages. On this approach a model represents a situation when there is a certain kind of structural correspondence between the model and the situation. The traditional advocates of the semantic view seemed to downplay the distinctively mathematical nature of the descriptions that were used to pick out their models. Here they followed much of the foundational tradition in the philosophy of mathematics in assuming that first-order logic and set theory were sufficient to characterize scientific models and their representational relationship to the world.

Many of the assumptions of the semantic view of theories were challenged by an alternative tradition which advocates for the need for "mediating models" which would stand in between the abstract theories picked out by mathematical descriptions and the concrete physical situations which these theories purport to represent. The most influential contributor to this approach is Nancy Cartwright. Many of her assumptions about mathematics are reflected in the early paper "Fitting Facts to Equations". As the title suggests, Cartwright conceives concrete situations in a thoroughly non-mathematical way. It is only through a considerable distortion and selective focus that we can achieve any kind of structural correspondence between mathematical descriptions and actual situations. As Cartwright puts it, her "basic view is that fundamental equations do not govern objects in reality; they govern only objects in models" (Cartwright 1983, p. 129). The objects in models are artificially devised through idealization and abstraction so that they can mediate between the abstract mathematics and the concrete physical world. This has far reaching implications for Cartwright for the proper understanding of what scientific knowledge tells us about the world, especially concerning the limited scope of the fundamental laws of our best science.[10]

An important response to Cartwright was offered by McMullin in his "Galilean Idealization" (1985). McMullin argued that since Galileo the scientific realist has had a clear strategy for reconciling

[10] See also Morgan and Morrison 1999.

the sort of gap between abstract, mathematical scientific theories and the concrete, messy physical world. This is to posit a series of corrections between the highly idealized mathematical representation and a fully realistic description (McMullin 1985, p. 261).[11] While no doubt a productive response to Cartwright's pessimism about bridging the gap between the mathematical domain and the concrete systems of the physical world, McMullin's realist correction strategy left several open questions that have been pursued in more recent work on modeling and idealization. To start, McMullin gave no general assurances that this sort of correction was always possible and left little for the realist to say in those cases where it seems de-idealization is impossible (Batterman 2009). A deeper issue concerns the need to distinguish between the semantic question of whether or not a mathematical representation is about a target system from the epistemic question of the respects in which the mathematical representation is an accurate representation of the target system. McMullin gave the realist a means to address the epistemic worry, but had little to say against Cartwright's and others semantic worries (Suárez 2003).

A more recent proposal by Bueno and Colyvan aims to address these semantic worries (Bueno and Colyvan forthcoming).[12] They argue that the attempt to understand representation using a single stage of structural correspondence is too rigid to account for the inferences that scientists carry out using their scientific models. Instead, they offer a three-stage process of immersion, derivation and interpretation. In the immersion step, an aspect of the target system is associated with some mathematical structure. Then in the derivation step certain consequences of this association are drawn. Finally, in the interpretation step the resulting mathematical claims are related back to predicted features of the target system. The new flexibility offered by Bueno and Colyvan's framework is sure to provoke continuing refinements from advocates of a broadly semantic view of theories and a new round of objections by their critics from the mediating models tradition.

We have seen how philosophers of mathematics are most concerned with questions about the metaphysical interpretation of mathematics, while philosophers of science tend to focus on the status of scientific models, representation and the realism debate. A new way forward is suggested by the success of the local studies of the success of science that are the shared data for the positions of the semantic theory tradition, the mediating models tradition as well as both realists and anti-realists. What is needed is a survey of the different ways in which mathematics has contributed to the success of science. These contributions may turn on issues of representation and structural correspondence or they may vindicate Cartwright's conception of the gap between the mathematical world and the physical world. Whatever the results, it seems clear that this sort of investigation would have broad implications for all of the debates covered in this survey. Carrying it out successfully would require the skills and background of both philosophers of mathematics and philosophers of science and might provide the motivation for further collaboration and philosophical innovation.

[11] See also Laymon 1995. A recent classification of approaches to idealization is offered by Weisberg 2007.
[12] See also da Costa and French 2003.

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