Part XIII

IS FOUNDATIONAL WORK IN MATHEMATICS RELEVANT TO THE PHILOSOPHY OF SCIENCE?

How Foundational Work in Mathematics Can be Relevant to Philosophy of Science

John P. Burgess

Princeton University

1. What "Foundational" Work in Mathematics Is

A common metaphor compares mathematics to a building: The axioms are its foundation, the lemmas and theorems and corollaries are its stones and bricks, logical deduction is the mortar or cement that holds them together. Once it is agreed, as it came to be in the nineteenth century, that mathematics is to be organized as a deductive science, with new results logically deduced from old, and ultimately from axioms, certain questions, not themselves straightforwardly mathematical, about the choice and status of the axioms to be adopted then arise. These are metaphorically called "foundational" questions by philosophers.

Now in one respect the metaphor of a building may have outlived its usefulness and become misleading. In a literal building, the foundations ground, support, uphold the whole. It was once the consensus view that in mathematics axioms should have a kind of bedrock certainty. This traditional view is now often called "foundationalism". It has not for a long time been anything like a consensus view. To acknowledge the interest and importance of "foundational" questions is *not* to imply an endorsement of "foundationalism". A critique of "foundationalism" however penetrating does not by itself suffice to show that no interest or importance attaches to "foundational" questions. In other respects, the metaphor retains its vitality, and in particular it is useful for indicating in what "new direction" philosophers of mathematics such as Tymoczko (1985) who are *not* interested in "foundational" questions would have us turn.

One of these new directions has actually been called "architecture". Note that in a literal building, the foundations form the least conspicuous part of the architecture, being mainly subsoil. In the case of mathematics, the above-ground architecture has some curious features. On the ground floor, the geometric and numerical wings seem almost wholly disconnected. A little higher up, the method of coordinates establishes a bridge between the two. Higher still, a basic division of the edifice (or of its core, for there are also some semi-detached outbuildings on the periphery) into algebraic, analytic, and geometric wings, each with a highly reticulated system of subdivisions, is evident; but so, too, is a complex network of bridges, passageways, and links among these many divisions and their various subsections. Through these links, by

PSA 1992, Volume 2, pp. 433-441

which, say, the motif of "group", originating in the algebraic wing, spreads to geometry, or the motif of "topology", originating in the geometric wing, spreads to analysis, mathematics maintains a material unity which is something very different from the formal unity of common set-theoretic foundations.

Another new direction attends less to the finished structure than to the process of its construction. "A cathedral is not a cathedral," Gauss reputedly said, "until the last scaffold is taken down." Today, far more so than in Gauss's own time, his precept is observed. A new chamber of the edifice is not unveiled for inspection by the guild of masons until the mortar has set and the scaffolding been carefully packed away; rigorous proofs, not heuristic arguments, are generally what is published. But of late many philosophers have become interested in what goes on under wraps before the unveiling. They have been more interested in the wooden scaffolding of *heuristics* than in the finished masonry of proof. And indeed the construction process often cannot be guessed from examination of the finished product alone (emphatically not in the work of Gauss, for instance). For the construction of a proof is not like that of a wall, where the bottom course of bricks is laid first, and then the next, and then the next, and so on. It is more like the construction of an arch or a dome, where the topmost piece may be first held in place, as a conjecture, by intuitive or inductive considerations, and then various intermediate supports are found and installed. In the opinion of many philosophical observers, the process is indeed rather like the kind of construction that goes on in empirical science.

Interest in heuristics has branched off in two other new directions. Some philosophers have become intrigued by a change in building methods now going on, whereby the scaffolding is in many cases no longer made of natural materials, human intuition, but rather of synthetics, of computer simulations, explorations, and inductive verifications. Some enthusiasts predict that in the future much of mathematics will be built of computer plastic rather than rigorous brick. Other philosophers have turned their attention in the opposite direction, towards the past, when standards of rigor and proof were lower, and the kind of heuristic, quasi-inductive thinking that now goes on mainly in the private "context of discovery" rather than the public "context of evaluation" was more out in the open. Large parts of the lower stories of present-day mathematics are, indeed, merely Victorian or Edwardian brick reconstructions of much older wooden structures. In the algebra and geometry wings, the masonry reproduces the form of the original almost without alteration. Eighteenth century analysis, however, was a bit rickety and rotten in spots, and unable to support higher developments, and so had to be redone according to a rather different blueprint.

A different "new direction" of inquiry attends less to the building and its builders, the mathematicians, than to its users or inhabitants, the scientists who *apply* mathematics. Originally, of course, the building was mostly being done by people who were planning to live in the finished structure. Today, there is a division of labor. The result is that a number of wings of the building, labyrinthine in complexity and baroque in ornamentation, are unoccupied and perhaps uninhabitable, while many looking for housing are confined to peripheral out-buildings, or encamped on the porches or under the awnings of the core structure. On the other hand, it does not seldom happen that scientists, coming upon some beautiful pavilion or complex long uninhabited, find that it is just what they have been looking for, and move in at once with all their goods and chattels.

The various new directions I have been indicating with this Homeric simile mostly belong to "cognitive studies" in a broad sense, but do not yet constitute a "cognitive science", because they are not yet ripe for strictly scientific treatment. Thus they belong to "philosophy" in the traditional sense of pre-scientific but not undisciplined speculation. The sustained pursuit of any of them, however, requires an acquaintance with the content and methods of mathematics which few professional philosophers possess, and indeed many of the most interesting explorations have been the work of professional mathematicians who are *amateurs* (in the original, favorable sense) of philosophy.

Interesting as these explorations often are, and useful too as a corrective to a too exclusive concentration on issues about the choice and status of axioms and the nature of logical proof and rigorous deduction, I have some reservations about the recent swing in the pendulum of philosophical fashion that would make them the main topic of philosophizing about mathematics. The "renaissance of empiricism" in philosophy of mathematics could easily go (perhaps in a few cases already has gone) too far in the direction of assimilating mathematics to other sciences, so that the gross, palpable difference in format between papers in mathematics journals and papers in chemistry journals, for instance, which results from mathematics having a unique methodology, gets ignored.

Neglect of axiomatics, logic, rigor, and so forth is especially unfortunate because its these matters, and these almost alone, that can at present be investigated in a fully scientific matter. When "foundational" questions of proof and provability and unprovability are at issue, we have in mathematical logic excellent idealized models of the phenomenon under investigation (almost equal to the best models in mathematical physics, and superior perhaps to those of mathematical economics or linguistics). One of the peripheral out-buildings, mathematical logic, is a kind of observation tower, from which one can get a good view of the core of the building. In mathematical usage, work in "foundations" means work in mathematical logic. (The section of the *Mathematical Reviews* covering mathematical logic, for instance, is labeled "Foundations".) It would be extremely unfortunate if mathematical work in "foundations", which is extremely active just at present, escaped the notice of philosophers because their attention was too exclusively focused in other directions.

2. One Question Such Work Can Help Answer

Not everyone was pleased by the reconstruction of mathematics that took place at an increasing rate during the nineteenth century, and was completed in the opening decades of the twentieth. Opposition among mathematicians to the direction their discipline was taking, which led to what philosophers now call "classical" mathematics, took various forms. By hindsight we can disentangle two distinct though interwoven strands of criticism within the mathematical community: the *utilitarian*, best represented perhaps by Heaviside, electrical engineer and inventor of the vector notation still in use among physicists; and the *constructivist*, most famously represented by Brouwer, topologist and mystic. (Illustrative quotations from both can be found in the standard history, Kline (1972).)

For Heaviside, what was going wrong was that mathematicians were devoting too much attention to proof and rigor, to the neglect of the needs of applications: The rigorists were damping interest in useful techniques, such as manipulation of divergent series; they were devoting excessive attention to useless subtleties, such as pathological counterexample functions. By hindsight such criticisms may seem deeply misguided. How could we ever have arrived at the kinds of mathematics used in twentieth century physics, if the nineteenth century had not advanced beyond the very lackadaisical attitude towards rigor of the eighteenth? Even if we set aside theoretical physics and consider only commercial applications, the most innovative and profitable technology of our era derives in an important measure through the genealogy Gödel-Turing-von Neumann from the "useless subtleties" of the rigorists. But I am not concerned here with the cogency of applicationist criticism, but rather with its difference in motivation from constructivist criticism.

For odd as it may sound to associate the word "rigor" with the name of Brouwer, what for him had gone wrong was that mathematicians were accepting as proofs what he felt ought not to be so accepted. And thus a higher standard of proof, of rigor, was in effect what he was demanding. Certainly the needs of applications were the farthest thing from his thoughts. To make the value of mathematics depend on its applications, especially technological applications, is to make the value of mathematics hostage to the decisions of political, economic, and social powers as to what kinds of applications do and don't get made. It is a commonplace that the practical military and industrial applications of mathematics have often been humanly and environmentally destructive. Brouwer held a globally negative evaluation of practical applications, and he even seems to have regarded purely theoretical applications to purely theoretical physics as involving a kind of sinful hubris.

Thus there arises the question of the status of a mathematics like that of Brouwer from the perspective of a radically different system of values, one more like Heaviside's. This question seems to have become the unofficial topic of our symposium. And it is a question on which "foundational" work in mathematics can provide much information. For one thing one can get from the observation tower of mathematical logic is a good a *comparative* view of the cathedral of orthodox, established mathematics and the chapels of various dissident sects: intuitionists, predicativists, and the like. One need not wait to see how high the heretics manage to build on the bases they adopt, but will to a considerable extent be able to predict this in advance, from results of mathematical logic.

The question whether constructive mathematics suffices for applications, the unofficial topic of the present symposium, was the official topic at another symposium at the Ninth International Congress of Logic, Methodology, and Philosophy of Science at Uppsala in the summer of 1991, where I had the good fortune to be moderator rather than a speaker. On that occasion a good deal was made of the ambiguities in the formulation of the question: What is "constructivism", what are "applications", what is "sufficiency"? Some of these issues have arisen again on the present occasion, and brief comment on them may be appropriate.

Constructivism is not a single position but a range of positions, whose relations to each other and to classical mathematics and subsystems thereof Professor Friedman's work and that of his collaborators has done much to illuminate. (Of course, there are some sectarians who will claim to constitute the one true constructivist faith and deny the "constructivist" label to all others.) On the panel in Sweden the liberal side of constructivism was represented by Per Martin-Löf, though his variety is less liberal than several that have been so intensively studied by Professor Feferman and his students. The strict side was represented by Edward Nelson, and his is an even stricter variety than would have been defended by Professor Bridges had he been able to join us in Chicago as originally planned.

Applications, too, may be understood in a narrower and a broader sense, as restricted to applications to engineering technology, or as including also applications to highly theoretical science. Even accepting the opinion that 95% of applications can fairly easily be handled by a moderately liberal constructivist mathematics, if one is actually going to adopt the militant attitude of the early constructivists like Brouwer, and call for the outright abolition of classical of mathematics, one has a responsibility to look at the more problematic 5% remainder. And the most problematic cases seem to be results connected with highly theoretical science. Professor Hellman cited one such example in Uppsala, connected with quantum mechanics; Roger Penrose cited another from his own research, related to general relativity. Had I been a participant rather than the moderator, I would probably have said something about measurable selection theory, a cluster of results on the theoretical fringes of subjects whose cores are applied: optimization and control theory, probability and statistics, mathematical economics, operator theory.

Inextricably intertwined with the question of what constitute "applications" is the question of what constitutes being "needed" for applications: If it would be in principle possible but in practice inconvenient to do without a certain result, should that result be called "unneeded"? Considerable attention was given in this connection at Uppsala to the status of *negative* results. It is a commonplace that in order to establish a positive result to the effect that something can be done, it suffices to do it; whereas, in order to establish a negative result to the effect that something cannot be done, it is necessary to articulate more formally what would count as doing it. A richer system of concepts may be needed for the statement of a negative result, and a richer system of axioms for its proof.

Now there is an obvious sense in which a negative result is inapplicable. But there is an equally obvious sense in which negative results are constantly being applied: For such results warn us not to waste time and other scarce resources on attempts to do the impossible, and thus have a definite utility in guiding research, even if that utility is obviously derivative from the utility of whatever else one does instead with the time saved. An example of such negative utility is provided by results in complexity theory to the effect that such-and-such a problem is NP-complete. Such a result provides a valuable suggestion about what it is reasonable to look for in an algorithm for solving the problem.

Actually, making use of NP-completeness in this way presupposes the negative proposition that $P \neq NP$. But this proposition is a conjecture, rather than a theorem. So long as it remains so, the question whether even *classical* mathematics is sufficient for applications remains in a peculiar but genuine sense open. This should remind us that potential future applications as well as actual present-day applications need to be considered: For were it never so firmly established that 100% of present-day applications can be accommodated by some version of constructivism, this would still leave us wondering whether this is because non-constructive mathematics is somehow inherently inapplicable, or rather because we have not yet been clever enough to figure out how to apply it.

But I do not want to pursue the question whether this, that, or the other form of mathematics suffices for applications any further here. That is to say, I do not wish to pursue an attempt to *answer* our unofficial question any further here. What I would like to do instead is to say something by way of indicating the connection of this question, to which the "foundational work in mathematics" of logicians among whom the leading contributors are surely (in alphabetical order) Professors Feferman and Friedman, is so relevant, with larger issues in metaphysics, epistemology, and general philosophy of science. In this way I hope to be able to indicate, however sketchily, one important ground for answering our *official* question in the affirmative.

3. How This Question Can Be Relevant to Philosophy of Science

I have urged philosophers not to ignore the fact that, among sciences, mathematics is, owing to its distinctive methodology of deductive proof, a special case. There is a need for a philosophy of mathematics specifically, in addition to general philosophy of science. At the same time, since mathematics is itself an important science and has important applications to other sciences, general philosophy of science cannot ignore or set aside the case of mathematics as special. A philosophical account of science that succeeds only insofar as mathematics is not involved does not succeed at all.

More than this, I believe that there are several large, important features that are *not* unique to mathematics but that are more *conspicuous* in the case of mathematics than in that of some other sciences, so that in connection with several large, important issues, general philosophy of science would do well to pay more attention than is often done to the mathematical case. Perhaps the largest question in general philosophy of science is that of *realism* versus *idealism*. The term "realism" has, however, been so overused as to have become almost useless for purposes of conveying a distinct idea to the mind, while the term "idealism" has almost ceased to be used, as if the contemporary idealists were ashamed to answer to their traditional name. I will therefore avoid both traditional terms, and in contrasting what properly should be called extreme realism with extreme idealism, I will call them *theologism* and *sociologism*.

"We are impelled," writes a contemporary philosopher of mathematics (Dummett 1987, p. 254) "by a drive to discover how things really are in themselves, that is to say, independently of the way they present themselves to us, with our particular sensory and intellectual faculties and our particular spatial and temporal perspective. I doubt whether it is possible to represent this notion of reality as it is in itself as even coherent, save by equating how things are in themselves with how they are apprehended by God: without that identification, there is only the description of the world as it appears to us and as how we may usefully represent it to ourselves for the purpose of making its workings and regularities surveyable."

The thought that our scientific theories represent, or are coming increasingly closer to representing, how reality is apprehended by God; that they are, or are coming increasingly closer to being, images in the human mind of the creative blueprint in the mind of the Great Architect, is what I will, avoiding the R-word, call *theologism*. An eloquent description of this attitude, which he found in Kepler, Galileo, and other early modern philosopher-scientists, has been given by an author who did not share it, William James, in a passage I have quoted elsewhere and will forbear to quote here. It is a characteristic seventeenth or eighteenth century attitude, and an icon of it is provided by the water color of William Blake, depicting the Ancient of Days bent over and inscribing a perfect Euclidean circle with a cosmic compass.

The attitude seems today a bit old-fashioned and hard to subscribe to, especially when one reflects on the kinds of *mathematics* involved in present-day science: The conception of the Creator designing the cosmos by fiddling with the parameters of a gauge field somehow lacks the requisite dignity. It seems suitable to inspire not Blake but Gary Larson and a *Far Side* cartoon. This is, so far, only an emotional reaction, a suspicion or feeling that there is too much artificiality, too much of *us*, in contemporary mathematics for it to be inviting to suppose that an extraterrestrial creature, let alone a celestial Creator, would have exactly the same form of mathematics, and mathematically-informed science, that we have managed to develop for our practical purposes and intellectual satisfaction. (How this suspicion might be substantiated I will be suggesting later.)

A prominent (to me as an outsider it seems predominant) tendency in recent history of science, increasingly influential also in philosophy of science, leaps at once from theologism to the opposite extreme, which avoiding the I-word I will call *soci-*

ologism. Sociologism adopts towards natural and mathematical science the same approach that secular historians from Gibbon onwards have adopted to the history of religious belief. As Gibbon attempts to explain the evolution of dogmas about a supernatural world without himself invoking supernatural intervention, so many contemporary historians of science attempt to explain the history of beliefs about stars and their physical relations, numbers and their arithmetic relations, and so forth, without any reference to celestial or numerical objects and facts, entirely in terms of social relations among scientists and between scientists and the ambient culture.

One prominent representative of this sociologist tendency has called for a moratorium on "cognitive" approaches by historians of science, whose attention, he thus suggests should be exclusively fastened on social networks. This influential writer has argued that "since the settlement of a controversy is *the cause* of Nature's representation, not the consequence, we *can never use the outcome–Nature–to explain how and why a controversy has been settled*" (Latour 1987, p.99). Note the immediate leap from the rejection of theologism in the premise that scientific theory, the *representation of* Nature, is a human creation, to sociologism, the conclusion that the objects and relations that scientific theory *is about*, Nature *itself*, is a human creation.

This kind of inference from "there is no ready-made *theory of* the world" to "there is no ready-made world" is characteristic of an increasing influential school of thought. Of course, *some* people repeat the slogan "reality is socially constructed" only as a kind of figure of speech, without intending to contradict accepted scientific estimates of the comparative ages of human societies and of natural objects like the stars. But someone who bases a call for a "moratorium" on the slogan must be taking it literally. I believe attention to the case of mathematics can help one to see there is something dubious about the conclusion that everything is to be explained sociologically.

For when we consider, to begin with, rudimentary arithmetic, we find that the most diverse societies from Mesopotamia to Mesoamerica have returned the same answers whenever they have asked the same questions. Assyrian and Aztec cultures had comparatively little in common, yet they certainly agreed on how much two plus two makes. And the trend of recent archæological and ethnological research has been to widen the range of cultures credited with independent mathematical discoveries, and to deepen the scope of such the discoveries recognized to have been made thus independently well beyond rudimentary arithmetic.

I should immediately add that the mathematical case does not provide the only difficulty for sociologism. For there is also the matter of applications. Observation is not so theory-dependent that millions of people could be persuaded by social influences to perceive music as coming out of CD-players if such audio equipment was in fact silent or only emitted white noise. Nor would bottom-line-oriented corporate executives pay for engineers expensively educated in, say, Feynman's account of electromagnetism, if that scientific theory were as irrelevant to designing equipment that produces the required sounds as is, say, Reich's orgone energy theory.

The mathematical case is even, in one way, not the best case to cite. For if we wish to illustrate the way in which scientific theory is, though not a passive reflection of external reality, nonetheless sensitive to constraints imposed by non-sociological facts, the obvious way to try to substantiate our claim would be by suggesting what scientific theory might have been like if the facts had been different. Now in the case of electromagnetic facts, this seems easiest to do by citing empirical constants. If the fine-structure constant had been different in the fifth decimal place, so presumably would have been our estimates for that constant (unless, indeed, the difference had somehow made human life and intelligence and science impossible). But the mathematical facts *couldn't* have been different, and so can't be cited in this way.

It is, rather, in attempting to substantiate suspicions about the opposite extreme view, theologism, that mathematics becomes most relevant. An obvious difficulty confronts any attempt to show that our current scientific theories are the way they are in part because we and our intellectual equipment and historical conditioning are the way they are. If the notion of a theory that simply "reflects reality" is an illusion, we cannot show that our current scientific theories do not simply "reflect reality" by comparing them to some other theory that does. The comparison, rather, must be with some other theory that also does not simply "reflect reality" but that might have been ours instead of our current theory had we, but not the world apart from us, been different.

In other words, what is wanted is, for a given current scientific theory T, an alternative theory T* that could in principle be used as we use T, to begin with at least in applications. Note that one would not be *advocating* T* as a replacement for T. And hence it would not matter if, in practice, it would be quite inconvenient for us to use T* in place of T. For what is convenient for us surely depends on how we have been intellectually equipped and historically conditioned, and the question under consideration is what our theories might have been like if we had been differently equipped and conditioned.

The problem, then is to find a T* that in principle would be usable in roughly the same range of applications as T, presumably by having roughly the same relevant empirical consequences as T. It seems very unlikely that we could make up such a T* starting all over from scratch and ignoring T; rather, the obvious strategy would be to try to produce such a T* by suitably transforming T. Moreover, once a candidate T* has been produced, it seems very unlikely that we could convince ourselves that it bears the required relation of empirical equivalence to T by purely inductive means, by working out the consequences or predictions of the two theories in this, that, and the other case, and comparing. Rather, general considerations pertaining to the transformation that took us from T to T* could be expected to be invoked.

But to speak of "general considerations" and "general transformations" and so forth in connection with a sophisticated scientific theory strongly suggests that what would be at issue would in fact be *mathematical* considerations and *mathematical* transformations. The obvious strategy would be to look for an alternative to current theory differing in its mathematical form, or in the mathematics that informs it. Or more simply, the obvious strategy would be to look for an alternative to our current mathematics that would be equally usable, for applications. And since some alternatives to current mathematics have already been proposed, the obvious strategy would be to look whether those alternatives to our current mathematics would in principle be equally usable for applications.

If it can be shown that some version of constructivism, for instance, would (even if only in principle) be sufficient for applications, then that is one fairly concrete way of showing that the mathematics we have currently arrived at is not one we were, literally or metaphorically, divinely foreordained to arrive at. I believe the most productive approach to the traditional issue of realism *versus* idealism is not to debate in the abstract over a false dichotomy between extreme theological and extreme sociological views, but rather to develop as many concrete examples as we can that can plausibly be suggested to show how our theories of the world might have been different if the world had been different, and how they might have been different if we had been different. Mathematical logic is highly relevant to such an enterprise, and this is one way (*only* one way, but the only way I have time for on the present occasion) in which foundational work in mathematics is relevant to philosophy of science.

References

Dummett, M. (1987), "The Intelligibility of Eucharistic Doctrine", *The Rationality of Religious Belief*, W.J. Abraham and W.W. Holtzer (eds.). Oxford: Clarendon Press, pp. 231-261.

Kline, M. (1972), *Mathematical Thought from Ancient to Modern Times*. New York: Oxford University Press.

Latour, B. (1987), *Science in Action*. Cambridge: Harvard University Press.

Tymoczko, T. (ed.) (1985), *New Directions in the Philosophy of Mathematics*. Boston: Birkhäuser.