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Categories, Structures, and the Frege-Hilbert Controversy: The Status of Meta-mathematics†

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There is a parallel between the debate between Gottlob Frege and David Hilbert at the turn of the twentieth century and at least some aspects of the current controversy over whether category theory provides the proper framework for structuralism in the philosophy of mathematics. The main issue, I think, concerns the place and interpretation of meta-mathematics in an algebraic or structuralist approach to mathematics. Can meta-mathematics itself be understood in algebraic or structural terms? Or is it an exception to the slogan that mathematics is the science of structure?

The slogan of structuralism is that mathematics is the science of structure. Rather than focusing on the nature of individual mathematical objects, such as natural numbers, the structuralist contends that the subject matter of arithmetic, for example, is the structure of any collection of objects that has a designated, initial object and a successor relation that satisfies the induction principle. In the contemporary scene, Paul Benacerraf's classic 'What numbers could not be' [1965] provides the standard motivation for structuralism, arguing that numbers are not objects and that numerals are not singular terms. According to Benacerraf, elementary arithmetic is concerned with systems that share the common structure, and not with any particular ontology. As structuralism was gaining momentum in the philosophy of mathematics, Colin McLarty's 'Numbers can be just what they have to' [1993] put forward the thesis that category theory provides the proper, or at least an especially insightful and compelling, framework for it. He points out that objects (and arrows) in categories have only relational properties, which are just the features that the structuralist focuses on in

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systems that exemplify the natural-number structure. Recent issues of this journal explore the relationship between category theory and structuralism. Geoffrey Hellman [2003] challenges the foundational claims made on behalf of category theory, and McLarty [2004] and Steve Awodey [2004] reply. It is interesting that these two replies take the debate in competing and perhaps incompatible directions.

The title of Hellman [2003] is a question: 'Does category theory provide a framework for mathematical structuralism?', and Awodey's opening sentence provides an answer: 'yes, obviously'. Like just about everything else, it depends on what question is being asked. What sort of framework are we after? And what is mathematical structuralism? For that matter, what is category theory? I suspect that, to some extent at least, the various sides of this debate are at cross-purposes, focusing on different sets of issues and questions. Even if this is true, it does not follow that the debate is useless, uninteresting, or unimportant. It may be that the issues and questions of one side are more central to mutual concerns than the issues and questions of the other side.

In this note, I hope to shed a little light on the question, or questions, by relating the present debate to a clash that took place over a hundred years ago, between two intellectual giants, Gottlob Frege and David Hilbert. I propose to focus on the role and function of meta-mathematics, which, I suggest, does not fit smoothly into Hilbert's algebraic perspective at the time. The problem was directly remedied in the subsequent development of the Hilbert program some decades later, where it is explicit that the proper meta-mathematics is finitary arithmetic. But, the story goes, this resolution was undermined with the incompleteness theorems, thanks to Gödel. So there is some unfinished business in the original debate, at least from Hilbert's side of it.

The general issue concerning meta-mathematics provides some perspective to the current debate over category theory and structuralism. The category-theory folks, or at least some of them, are squarely on Hilbert's side of the Frege-Hilbert divide. This is no accident, of course. Saunders Mac Lane's roots go back to Göttingen. In part, Hellman's approach to structuralism [1989], [1996], as well as my own, also fall on Hilbert's side (see, for example, Shapiro [1997], Chapter 5, §3, or Shapiro [1996]). However, my own *ante rem* structuralism is an attempt to have it both ways. At least some of the questions answered in Shapiro [1997] derive from what may be called the Frege-Quine tradition, and they concern the aforementioned unfinished business of the Hilbert program—the proper role or place of meta-mathematics. In my book, I use the word 'structure' as a sortal, with quantifiers ranging over structures. I took this as a burden to say something about what a structure is, and I was led to traditional talk of universals and platonic forms. Fellow structuralists, such as Hellman and Michael Resnik [1997], accept the same problematic, and give different answers from mine, the former being a structuralism without structures.

The issue at hand is distinctly Fregean. We try to say what a structure is, and when two structures are identical or distinct. In short, we require a mathematical and/or a philosophical *theory* of structures or of systems-with-shared-structure. One of the burdens of McLarty [2004] is to show that the meta-mathematical matters can themselves be approached from the categorical perspective.

The debate between Frege and Hilbert concerned geometry. Alberto Coffa ([1986], pp. 8, 17) provides a delightful summary of the situation on the ground at the time:

> During the second half of the nineteenth century, through a process still awaiting explanation, the community of geometers reached the conclusion that all geometries were here to stay ... [T]his had all the appearance of being the first time that a community of scientists had agreed to accept in a not-merely-provisory way all the members of a set of mutually inconsistent theories about a certain domain ... It was now up to philosophers ... to make epistemological sense of the mathematicians' attitude toward geometry ... The challenge was a difficult test for philosophers, a test which (sad to say) they all failed ...
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> For decades professional philosophers had remained largely unmoved by the new developments, watching them from afar or not at all ... As the trend toward formalism became stronger and more definite, however, some philosophers concluded that the noble science of geometry was taking too harsh a beating from its practitioners. Perhaps it was time to take a stand on their behalf. In 1899, philosophy and geometry finally stood in eyeball-to-eyeball confrontation. The issue was to determine what, exactly, was going on in the new geometry.

What was going on, I believe, was that geometry was becoming less the science of space or space-time, and more the formal study of certain structures. Issues concerning the proper application of geometry to physics were being separated from the status of pure geometry, the branch of mathematics.[1] Hilbert's *Grundlagen der Geometrie* [1899] represents the culmination of this development, delivering a death blow to a role for intuition or perception in the practice of geometry. Although intuition or observation may be the source of axioms, it plays no role in the actual pursuit of the subject.

The early pages of Hilbert [1899] contain phrases like 'the axioms of this group define the idea expressed by the word 'between' ...' and 'the

[1] Coffa's claim that philosophers had ignored and then opposed the developments in geometry is a bit of an exaggeration. Husserl [1900] made effective use of the new perspective in developing his metaphysics and philosophy of science (see Chapter 11, especially §§70–71). Thanks to Per Martin-Löf for the reference. Coffa focuses on Frege and Russell.

axioms of this group define the notion of congruence or motion’. The idea is summed up as follows:

> We think of . . . points, straight lines, and planes as having certain mutual relations, which we indicate by means of such words as ‘are situated’, ‘between’, ‘parallel’, ‘congruent’, ‘continuous’, *etc.* The complete and exact description of these relations follows as a consequence of the *axioms of geometry*.

A crucial aspect of the axiomatization is that the system is what I call ‘free-standing’. Anything at all can play the role of the undefined primitives of points, lines, planes, *etc.*, so long as the axioms are satisfied. Hilbert was not out to capture the essence of a specific chunk of reality, be it space, the forms of intuition, or anything else. Otto Blumenthal reports that in a discussion in a Berlin train station in 1891, Hilbert said that in a proper axiomatization of geometry, ‘one must always be able to say, instead of “points, straight lines, and planes”, “tables, chairs, and beer mugs”.’[2]

In a retrospective encyclopedia article, Hilbert’s student and protégé Paul Bernays ([1967], p. 497) summed up the aims of the new geometry:

> A main feature of Hilbert’s axiomatization of geometry is that the axiomatic method is presented and practiced in the spirit of the abstract conception of mathematics that arose at the end of the nineteenth century and which has generally been adopted in modern mathematics. It consists in abstracting from the intuitive meaning of the terms . . . and in understanding the assertions (theorems) of the axiomatized theory in a hypothetical sense, that is, as holding true for any interpretation . . . for which the axioms are satisfied. Thus, an axiom system is regarded not as a system of statements about a subject matter but as a system of conditions for what might be called a relational structure . . . [On] this conception of axiomatics, . . . logical reasoning on the basis of the axioms is used not merely as a means of assisting intuition in the study of spatial figures; rather logical dependencies are considered for their own sake, and it is insisted that in reasoning we should rely only on those properties of a figure that either are explicitly assumed or follow logically from the assumptions and axioms.

At first, Frege had trouble with this orientation to mathematics. In a letter dated December 27, 1899, he lectured Hilbert on the nature of definitions and axioms.[3] According to Frege, axioms should express truths;

[2] ‘Lebensgeschichte’ in Hilbert [1935], pp. 388–429; the story is related on p. 403.

[3] The correspondence between Frege and Hilbert is published in Frege [1976] and translated in Frege [1980]. See Blanchette [1996] for an insightful discussion of Frege’s notion of logical consequence.

definitions should give the meanings and fix the denotations of terms. These are fundamentally different enterprises, and should never be confused. Moreover, with a Hilbert-style implicit definition, *neither* job is accomplished. Frege complained that Hilbert [1899] does not provide a definition of, say, 'between' since the axiomatization 'does not give a characteristic mark by which one could recognize whether the relation Between obtains':

> . . . the meanings of the words 'point', 'line', 'between' are not given, but are assumed to be known in advance . . . [I]t is also left unclear what you call a point. One first thinks of points in the sense of Euclidean geometry, . . . But afterwards you think of a pair of numbers as a point . . . Here the axioms are made to carry a burden that belongs to definitions . . . [B]eside the old meaning of the word 'axiom', . . . there emerges another meaning but one which I cannot grasp.

According to Frege, definitions are in sharp contrast with axioms and theorems. The latter

> . . . must not contain a word or sign whose sense and meaning, or whose contribution to the expression of a thought, was not already completely laid down, so that there is no doubt about the sense of the proposition and the thought it expresses. The only question can be whether this thought is true and what its truth rests on. Thus axioms and theorems can never try to lay down the meaning of a sign or word that occurs in them, but it must already be laid down.

Frege's point is a simple one. If the terms in the proposed 'axioms' do not have meaning beforehand, then the statements cannot be true (or false), and thus they cannot be axioms. If they do have meaning beforehand, then the 'axioms' cannot be definitions.

Hilbert replied on December 29, rejecting Frege's suggestion that the meanings of the words 'point', 'line', and 'plane' are 'not given, but are assumed to be known in advance':

> I do not want to assume anything as known in advance. I regard my explanation . . . as the definition of the concepts point, line, plane . . . If one is looking for other definitions of a 'point', *e.g.* through paraphrase in terms of extensionless, *etc.*, then I must indeed oppose such attempts in the most decisive way; one is looking for something one can never find because there is nothing there; and everything gets lost and becomes vague and tangled and degenerates into a game of hide and seek.

This talk of paraphrase is an allusion to 'definitions' like Euclid's 'a point is that which has no parts'. Such 'definitions' play no role in the mathematical development, and are thus irrelevant. Later in the same letter, when responding to the complaint that his notion of 'point' is not 'unequivocally fixed', Hilbert wrote:

> . . . it is surely obvious that every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and that the basic elements can be thought of in any way one likes. If in speaking of my points, I think of some system of things, *e.g.*, the system love, law, chimney-sweep . . . and then assume all my axioms as relations between these things, then my propositions, *e.g.*, Pythagoras' theorem, are also valid for these things . . . [A]ny theory can always be applied to infinitely many systems of basic elements. One only needs to apply a reversible one-one transformation and lay it down that the axioms shall be correspondingly the same for the transformed things. This circumstance is in fact frequently made use of, *e.g.*, in the principle of duality . . . [This] . . . can never be a defect in a theory, and it is in any case unavoidable.

Note the similarity to the remark in the train station. Hilbert repeated the role of what is now called 'implicit definition' (or, in philosophical circles, 'functional definition') noting that it is impossible to give a definition of 'point' in a few lines since 'only the whole structure of axioms yields a complete definition'. He noted the now familiar point that isomorphic structures are equivalent.

Frege did not get it, or did not want to. On the following September 16, he wrote that he could not reconcile the claim that axioms are definitions with Hilbert's view that axioms contain a precise and complete statement of the relations among the elementary concepts of a field of study. For Frege, 'there can be talk about relations between concepts . . . only after these concepts have been given sharp limits, but not while they are being defined'. On September 22, an exasperated Hilbert replied:

> . . . a concept can be fixed logically only by its relations to other concepts. These relations, formulated in certain statements I call axioms, thus arriving at the view that axioms . . . are the definitions of the concepts. I did not think up this view because I had nothing better to do, but I found myself forced into it by the requirements of strictness in logical inference and in the logical construction of a theory. I have become convinced that the more subtle parts of mathematics . . . can be treated with certainty only in this way; otherwise one is only going around in a circle.

Hilbert's claim that a concept can be fixed only by its relations to other concepts is a standard motivation for structuralism.[4]

Nowadays we have a rough and ready distinction which we can apply here. The algebraist says that a group is anything that satisfies the axioms of group theory; a ring is anything that satisfies the ring axioms, *etc.* There is no such thing as '*the* group' or 'the ring'. Hilbert says the same thing about geometry, and, by extension, arithmetic, real analysis, and so forth. At the time, it seems, Hilbert took *every* branch of mathematics to be algebraic: any given branch is 'about' any system that satisfies its axioms. In opposition to this, Frege insisted that arithmetic and geometry each have a *specific* subject matter, space in the one case and the realm of natural numbers in the other. And the axioms express (presumably self-evident) truths about this subject matter. Following a suggestion of Hellman's, let us say that for Frege, the axioms of arithmetic and geometry are *assertory*; and for Hilbert, they are *algebraic*. Sentences that are assertory are meant to express propositions with fixed truth values. Algebraic sentences are schematic, applying to any system of objects that meets certain given conditions.[5]

For what it is worth, my *ante rem* structuralism proposes to bridge the gap between the algebraic and assertory approach to theories like Euclidean geometry, arithmetic, real analysis and complex analysis. From one perspective, called 'places-are-offices', the theories are algebraic, applying to whatever systems satisfy them. However, if the axioms of a branch of mathematics are satisfiable and categorical, then they characterize a (single) structure, and the axioms are true *of it*. I call this the 'places-are-objects' perspective. The idea is that places in a structure are *bona fide* objects, and we can have quantifiers ranging over them. The structure itself is a chunk of reality, and the theory is about it. So the same axioms are algebraic from one perspective, and assertory from another (Shapiro [1997], Chapter 3).[6]

[4] The exegetical and historical issues are complex, and it would take us too far afield to go much deeper. Did Hilbert intend his remark to be limited to mathematical, or perhaps just geometrical concepts? Consider, moreover, Frege's ([1884], Introduction) own context principle that one can 'never ask for the meaning of a word in isolation, but only in the context of a proposition'. This can be, and has been, interpreted to entail that a concept can only be fixed by its relations to other concepts (see, for example, the neo-logicists Wright [1983], Hale [1987]).

[5] The word 'algebraic' might be a bit misleading. There is a three-fold distinction that can be made here. Say that a theory is 'Fregean' if it is intended to be about a specific subject matter, and that a theory is 'Hilbertian' if it consists of taking the logical consequences of an axiomatization regarded as an implicit definition of a type of structure. Contemporary group theory and ring theory are not pursued, for more than a few minutes, in this Hilbertian manner. Rather, the group theorist studies all groups, developing relationships between them and with other structures. This study is made in a background framework, perhaps naïve set theory, and one can take either a Fregean or a Hilbertian approach to this background.

[6] It was thus potentially misleading for me to refer to theories like arithmetic and real analysis as 'non-algebraic' in the motivating sections of Shapiro [1997] (*e.g.*, pp, 40–41).

One can take any algebraic sentence and interpret it directly as a proposition about all systems of a certain sort. Consider, for example, the Euclidean sentence that there is a point that lies between any two distinct points. From the algebraic perspective, this comes to something like this:

(*) In any (possible) Euclidean system S, for any two distinct objects a, b in S that are 'points-in-S', there is a third object c that is also a point-in-S, and c lies between-in-S a and b.

This is the route of eliminative structuralism *à la* Benacerraf and modal structuralism *à la* Hellman, the latter supplying the 'translation' schemes explicitly, with full mathematical rigor. The above passage from Bernays contains a sentence in this form, and such statements are at least implicit in Hilbert's motivating remarks and in the correspondence with Frege.

What is the status of statements like (*)? It would seem that for the algebraist, such sentences must themselves be assertory. This is just to insist that a philosopher or mathematician assert something when stating the algebraic position. Moreover, it would run counter to the spirit of Hilbert's approach to think of the opening quantifier in (*) as itself restricted to a particular system. A system of what? A system of systems? At least *prima facie*, then, if an algebraist insists that all (legitimate) mathematical statements are algebraic, then (*)-type assertions are not mathematical. But this seems *ad hoc*. In typical cases, the (*)-type assertions contain no non-logical terminology, and so for the algebraist, there is nothing to reinterpret. In an attempt to recapture Hilbert's perspective, Frege [1971] himself showed how to make statements like (*) in his own logical system. And, of course, for Frege such statements, like all others in mathematics, are assertory.

Hilbert's thoroughgoing algebraic perspective is reminiscent of the oft-heard claim that a category is anything that satisfies the axioms of category theory, what Awodey calls the top-down approach. Contrary to what I once wrote (Shapiro [1997], p. 193), the 'arrows' of a category do not have to be functions (as those notions are understood in set theory—not to quibble over terminology). The category theorist claims that her account, in terms of the axioms of category theory, is a more fruitful way to define and organize (algebraic) mathematics than Hilbert's method of implicit definition in higher-order languages (which is closer to the techniques of Shapiro [1997]). I do not have the expertise to shed light on that matter, and it does not strike me as particularly philosophical. I see how many of the philosophical claims in my book can be formulated in terms of category theory,

I took the distinction to be implicit in mathematical practice, although this begs the question against a thoroughgoing algebraist like Hilbert of 1900. The correct idea, I think, is that theories like arithmetic and real analysis can be treated as assertory from one perspective, that of places-are-objects. This does not preclude them from also being treated algebraically.

rather than the quasi-model-theoretic perspective I took at the time. I will not speak for my fellow structuralists.

Some of the central points in Hellman [2003] have roots in a closely related matter on which Hilbert and Frege never saw eye to eye: the role of consistency and the nature of mathematical existence. Hilbert's *Grundlagen* provided consistency and independence proofs by finding interpretations that satisfy various sets of axioms. Typically, he would interpret the axioms of a theory in terms of constructions on real numbers. This approach, now as common as anything in mathematics, runs roughshod over Euclid's definition of a 'point' as 'that which has no parts'. When we interpret a 'point' as an ordered pair of real numbers, we see that points can indeed have parts. This free reinterpretation of axioms is a main strength of contemporary mathematical logic, and a mainstay of mathematics generally. It drives the structuralist, algebraic, perspective on mathematics. And it runs counter to the Fregean perspective. In the first letter Frege complained that Hilbert takes 'a pair of numbers as a point', contrasting this with 'points in the sense of Euclidean geometry'.

What is the Hilbertian to make of the statements of consistency themselves? Are they algebraic or assertory, or both at once? In the 1899 letter, Frege said that there is never a serious question of consistency. From the truth of axioms, concerning their intended subject matter, 'it follows that they do not contradict one another'. Since Hilbert did 'not want to assume anything as known in advance', he rejected Frege's claim that we can reason from truth to consistency. He wrote:

> As long as I have been thinking, writing and lecturing on these things, I have been saying the exact reverse: if the arbitrarily given axioms do not contradict each other with all their consequences, then they are true and the things defined by them exist. This is for me the criterion of truth and existence.

On the algebraic, top-down approach, we characterize a structure and thereby study systems that exemplify that structure. Clearly, if the characterization is not coherent, then one has not characterized a possible system, and the enterprise has misfired. The more controversial claim is the converse: if the given characterization is coherent, then all has gone well. There is no further mathematical or metaphysical burden to discharge. The question of coherence is all that remains of the traditional issue of existence, at least for mathematics.

But what *of* this notion of coherence? Is it a *mathematical* question, and, if so, how do we negotiate it? For Hilbert, coherence is consistency, and by this he surely meant deductive, proof-theoretic consistency. If it is not possible to derive a contradiction from a collection of axioms, then 'the things defined by' the axioms exist, and the axioms 'are true' of those things.

It seems clear that for Hilbert and just about anyone else, consistency is itself a mathematical matter. His methodology indicates that in order for us to be assured that certain mathematical objects exist, we have to establish the consistency of an axiomatization. In the *Grundlagen*, Hilbert discharged this burden, at least in part, by providing relative consistency *proofs*. For example, in showing how to interpret the axioms of a non-Euclidean geometry in the real numbers, he established that the non-Euclidean theory is consistent if real analysis is.

We now enter the realm of meta-mathematics. Given the way this matter is handled in Hilbert's *Grundlagen*, it is clear that meta-mathematics is itself mathematics. What are we to make of it? What is the status of *statements* (and proofs) of consistency? Are they assertory or algebraic?

This matter is not treated explicitly in Hilbert's *Grundlagen*, and it is hard to be definitive on what his view was, or should have been, but I suggest that the meta-theory—the mathematical theory in which the consistency of an axiomatization is established—is not to be understood algebraically, not as another theory of whatever satisfies *its* axioms. Instead, the statement that a given theory, such as Euclidean geometry, is consistent is itself assertory. The notion of consistency is a contentful property of theories, and is not to be understood as defined implicitly by the axioms of the meta-theory. For one thing, the meta-theory is not axiomatized in the *Grundlagen*, and so there is no implicit definition of the meta-theoretic notions. This, of course, is not decisive. It would be a routine exercise for a graduate student in mathematical logic to axiomatize the meta-theory of the *Grundlagen*. Given the structural analogy between natural numbers and strings, the meta-theory would resemble elementary arithmetic. However, if a Hilbertian algebraist did think of the axiomatized meta-theory as algebraic, then she would have to worry about *its* consistency. How would we establish that? The ensuing regress is vicious to the epistemological goals of the *Grundlagen*.[7]

In the later Hilbert program (*e.g.*, [1925]) relative consistency gives way to absolute consistency. There, the meta-theory is finitary proof theory, focused directly on formal languages themselves. It is explicit that finitary proof theory is not just the study of another structure, on a par with geometry and real analysis. Finitary proof theory has its own unique subject matter, related to natural numbers and formal syntax, and it is ultimately founded on something in the neighborhood of Kantian intuition. Hilbert said that finitary proof theory is contentful. In present terms, the theorems of finitary proof theory are assertory, not algebraic.

Of course, thanks to Gödel, finitary proof theory proved to be all but useless for establishing consistency. We have come to live without absolute consistency proofs. The crisis in set theory passed, and we now fly

[7] I am indebted to an anonymous referee for this point.

without a Hilbert-style safety net. But, of course, the powerful algebraic orientation to mathematics continues to thrive today, as well it should. The orientation is championed by, among others, the advocates of category theory. But, in practice, how do we satisfy ourselves that a given characterization—whether it is a traditional axiomatization or a type of category—is coherent, and thus characterizes a structure or a possible system? This is the unfinished business from the Hilbert program that I mentioned earlier.

Here is the question: Do we still need some sort of meta-mathematics to answer legitimate foundational questions? If we do need meta-mathematics, can it be understood algebraically, or structurally, on a par with (pure) geometry, and, for the Hilbert-style algebraist, infinitary arithmetic and real analysis? Or is meta-mathematics an exception to the slogan that mathematics concerns structure?

As noted above, in the debate with Frege, Hilbert said that (deductive) consistency is sufficient for 'existence', or, better, that consistency is all that remains of the traditional, metaphysical matter of existence. This much continued into the Hilbert program. If we restrict ourselves to first-order axiomatizations, then Gödel's completeness theorem does assure us that consistency implies existence. The theorem is that if a first-order axiomatization is consistent, then it has a model: there is a system that makes the axioms true. So perhaps Hilbert's claim about consistency foreshadowed the completeness theorem. But then what is the status of the completeness theorem itself? If the algebraist wants to use it to bolster the claim that consistency is all that remains of the traditional question of existence, then she must think of the completeness theorem as itself assertory. Indeed, she might *assert* it in defense of her algebraic claim. To play the foundational role, the meta-theory in which the completeness theorem is proved cannot be algebraic. It must be contentful.

Recall also that Hilbert's original axiomatizations are not first-order. Indeed, when first-order logic was originally separated out for special study, it was called the *restricted* functional calculus. The completeness theorem fails in higher-order systems (see Shapiro [1991], Chapter 4): deductive consistency does not entail coherence. Consider, for example, the theory I shall call PA-weird, consisting of the second-order Peano axioms together with the formal statement that second-order PA is *in*consistent. PA-weird is consistent, but it has no models and, arguably, it does not describe a coherent structure. In my structuralism book, I propose that coherence is to be modeled by satisfiability. But satisfiability seems to require an assertory theory of sets or structures.

This is not just an obscure matter for philosophers of mathematics. The practice of mathematics occasionally runs into serious questions concerning coherence, and, thus for the Hilbert-style algebraist or the structuralist, the existence of certain structures. Mark Wilson ([1993], §III) illustrates the historical development and acceptance of a space-time with an 'affine' structure on the temporal slices:

> . . . the acceptance of . . . non-traditional structures poses a delicate problem for philosophy of mathematics, *viz*., how can the novel structures be brought under the umbrella of *safe* mathematics? Certainly, we rightly feel, after sufficient doodles have been deposited on coffee shop napkins, that we understand the intended structure . . . But it is hard to find a fully satisfactory way that permits a smooth integration of non-standard structures into mathematics . . . We would hope that 'any coherent structure we can dream up is worthy of mathematical study . . .' The rub comes when we try to determine whether a proposed structure is 'coherent' or not. Raw 'intuition' cannot always be trusted; even the great Riemann accepted structures as coherent that later turned out to be impossible. *Existence principles* beyond 'it seems okay to me' are needed to decide whether a proposed novel structure is genuinely coherent . . . [L]ate nineteenth century mathematicians recognized that . . . existence principles . . . need to piggyback eventually upon some accepted range of more traditional mathematical structure, such as the ontological frames of arithmetic or Euclidean geometry. In . . . our century, set theory has become the canonical backdrop to which questions of structural existence are referred. (pp. 208–209)

Within the community of professional mathematicians, if not philosophers, a set-theoretic proof of satisfiability resolves any legitimate questions of existence.

But we now have an especially sharp version of the previous question concerning meta-theory. What are we to make of the set-theoretic model theory used to resolve questions of coherence in practice? Set theory, of course, is far more substantial than finitary proof theory, but it plays a similar role in adjudicating matters of 'existence'. There are two theoretical options concerning the meta-mathematical background, what Wilson calls the 'canonical backdrop'. One is to argue that for set theory to play the foundational role, it is *not* to be understood algebraically. On this view, set theory has a subject matter, the iterative hierarchy V. It is an *assertory* theory about how various structures relate to, and interact with, each other. Our first option, then, is to think of the background meta-mathematics—model theory—in the same way that Frege thought of arithmetic and geometry, and the same way that Hilbert understood finitary proof theory and, arguably, the assertions of relative consistency in his *Grundlagen der Geometrie*.

This orientation toward the meta-theory supports Hellman's [2003] claim that the category theorist requires an 'external' theory of relations, functions, and the like. This 'home address' issue concerns the nature of the fundamental terminology of category theory. Hellman's point is that the talk of relations and functions (or functors) in the informal language of category theory must be assertory, and that set theory is a natural background for such assertions. It is not that one thinks of the iterative hierarchy as literally containing all structures, or all categories. Rather, we think of the iterative hierarchy as containing an isomorphism type for each structure. My own structure theory (Shapiro [1997], Chapter 3, §4) was meant to play the same assertory, foundational, role as set theory, and, indeed, structure theory is a notational variant of set theory. Hellman's [1989] modal set theory does the same work, without presupposing the (actual) existence of any abstract objects. In present terms, the point here is that the meta-theory, whatever it is, must itself be assertory, and thus an exception to the slogan that mathematics is the science of structure.

A category theorist who goes for this first theoretical option concerning the meta-theory is not without resources. McLarty [2004] argues that a set theory formulated in categorical terms, such as ETCS or CCAF, will work even better as a canonical backdrop than the more standard Zermelo-Fraenkel set theory. To be sure, if a category-based theory is to play this role, then *its* axioms must be assertory. The canonical backdrop, whatever it may be, is 'external' to the algebraic perspective. But there is nothing to prevent the category-theorist from understanding a theory as assertory. This is not to say that McLarty himself takes the axioms of some category-based set theory this way. His point is that if one needs or wants a set theory to serve as canonical backdrop for questions of existence, as on our first option, then a category-based set theory will do the job as well as, or better than, the more standard Zermelo-Fraenkel set theory. I must plead ignorance, or at least incompetence, concerning the proper mathematical theoretical framework for such meta-mathematical questions, once it is agreed that such questions are legitimate, and assertory. The various category theories and set theories are inter-translatable, and the debate sometimes turns on which is more natural.

In any case, our first theoretical option is not quite the bottom-up approach alluded to in Awodey [2004]. The algebraic structuralist does not *construct* the structures of mathematics within his or her favored set theory. Set theory does not supply the ultimate subject matter for any branch of mathematics. Rather, we use set theory to establish that a given theory is coherent.

In the account of *ante rem* structuralism in Shapiro [1997], I said that a structure *consists* of places and relations. So far, this is only a structural claim, analogous to the set-theoretical thesis that a set consists of its members. However, I provided a mathematical theory of structures and their places, and I suppose that I was thinking of the subject matter of that theory as the universe of all of mathematics. In that sense, my account is bottom-up, within what we may call the Frege-Quine tradition. I presume that if ordinary model theory is understood as a semantics for mathematical languages, it too is bottom-up in the same sense, since it aims to provide a theoretical account of the interpretations of various theories. In any case, there is little need to quibble over labels.

To sum up our first theoretical option, standard set theory, the category-based set theory suggested by McLarty, my own structure theory, or Hellman's modal set theory are themselves assertory theories of sets, structures, the possible existence of systems, *etc.* As such, each of them is not just another mathematical theory, providing an implicit definition of some structures, or isomorphism types. The reason for this is that set theory, structure theory, *etc.*, has a foundational role to play concerning the coherence of definitions. And this last is an assertory matter.

Anyway, this is one way to look at the meta-mathematical background. A second theoretical option, more in line with Awodey [2004], is to kick away the foundational ladder altogether, and take the meta-mathematical set theory, structure theory, or whatever, itself to be an algebraic theory. On this view, set theory does not directly serve as a court of appeal for matters of coherence and thus existence, neither in the sense of supplying the ultimate ontology for mathematics, on the bottom-up approach, nor in the attenuated sense of supplying isomorphism types of everything in mathematics. The axioms of set theory are just implicit definitions that, if coherent, characterize a structure or a class of structures. The same goes for structure theory, modal set theory, and the various topos theories.

On this view, everything in mathematics is algebraic. So if there is to be an assertory canonical backdrop—a non-algebraic theory of coherence, consistency, mathematical existence, whatever—it will be relegated outside of mathematics, perhaps to philosophy. But this seems an *ad hoc* way to draw boundaries between disciplines. As we have seen, at least some questions of existence have been settled, by mathematicians, via rigorous proof. So this is a tough pill to swallow.

On our second theoretical option, set theory may still help when serious questions of coherence arise in practice for theories other than set theory. We go back to the plan executed in Hilbert's *Grundlagen*, and settle for the analogue of relative consistency proofs. By finding a model of a given theory T in set theory, or in 'the' iterative hierarchy, one shows that T is coherent, and defines a structure, *if set theory does*. Such a proof is effective whether the background set theory is formulated in traditional terms or in the idiom of category theory, as suggested in McLarty [2004].

Notice that we have no formal assurance that our set theory is itself coherent, and thus characterizes a structure or possible system. But perhaps we don't need such assurance. On the first theoretical option, where the meta-theory is assertory, we likewise have no theoretical assurance that set theory is *true*. Again, we have no safety net, and do not really need one.

From the present, thoroughly algebraic perspective, we are still left with the notion of relative consistency. And our nagging question returns. What are we to make of a statement of relative consistency—a proposition that a given theory T is consistent if set theory is? Since, on the view in question, all of mathematics is algebraic, there is no room for any assertory matters in mathematics. So if we insist on the letter of our second theoretical option, and if statements of relative consistency are assertory, as seems obvious, then they are not mathematical. But it seems equally obvious that relative consistency is a mathematical matter.

Perhaps at least one of these 'obvious' theses can be resisted. On our second theoretical option, to speak *mathematically* about consistency, coherence, or the like, is just to speak within an algebraic theory. The statements of relative consistency hold in any interpretation or reinterpretation of the non-logical terminology in the background set theory, which includes things like the membership symbol and a sign for satisfaction. Just as (pure) geometry is not *about* any particular things, be they space points or space-time points, (pure) set theory, proof theory, model theory, formal semantics, and the like, are not about any particular things, be they theories, consistency, deductions, interpretations, models, or the like. To be sure, we do make assertory statements about physical space—in physics itself for example. Such statements are part of the standard *application* of geometry. In doing physical geometry, we interpret the non-logical terminology of pure geometry accordingly, and the axioms, so interpreted, may or may not be true. Similarly, assertory statements about interpretations, deductions, relative consistency, and the like, are an application of the background meta-theory, perhaps the standard application. Just as mathematics became liberated from intuitions concerning physical space and space-time in the nineteenth century, contemporary mathematical logic is similarly liberated from theories, interpretations, deductions, and consistency. As a structuralist, our theorist can hold—in assertory philosophy—that satisfiability, consistency, or coherence implies existence, but she cannot maintain that any of these notions are mathematical matters. There simply are no distinctly mathematical objects, and so theories, deductions, and interpretations are not mathematical. But perhaps we should not quibble over labels.[8]

[8] On one occasion when I presented an ancestor of this paper, a mathematician in the audience endorsed the thoroughgoing algebraic perspective, calling it a 'Copernican revolution'. Just as the pioneers of modern physics and astronomy showed that we do not have to think of the earth as unmovable, or even unmoving, so the pioneers of modern mathematics showed that we do not have to see talk of numbers and the like as grounded in a solid subject matter. It is an intriguing analogy, but I am not sure what was meant. If the idea is that the modern mathematician does not need absolute certainty that her axioms are consistent, then it applies just as well to the less than thoroughgoing perspective of our first option. It is just the lack of a safety net, noted above. However, if the mathematician's comment was

This is not the place to decide which of the theoretical options are best for structuralism, or for category theory, or for mathematics generally. In this note, I will rest content if I have managed to sharpen the battle lines a little.

References

AWODEY, S. [2004]: ‘An answer to Hellman’s question: “Does category theory provide a framework for mathematical structuralism?” ’, *Philosophia Mathematica* (3) **12**, 54–64.

BENACERRAF, P. [1965]: ‘What numbers could not be’, *Philosophical Review* **74**, 47–73; reprinted in P. Benacerraf and H. Putnam [1983], pp. 272–294.

BENACERRAF, P., and H. PUTNAM, eds. [1983]: *Philosophy of Mathematics.* 2nd ed. Cambridge: Cambridge University Press.

BERNAYS, P. [1967]: ‘Hilbert, David’ in *The encyclopedia of philosophy, Volume 3*, edited by P. Edwards. New York: Macmillan Publishing Company and The Free Press, pp. 496–504.

BLANCHETTE, P. [1996]: ‘Frege and Hilbert on consistency’, *Journal of Philosophy* **93**, 317–336.

COFFA, A. [1986]: ‘From Geometry to tolerance: sources of conventionalism in nineteenth-century geometry’ in Robert Colodny, ed. *From quarks to quasars: Philosophical problems of modern physics*. University of Pittsburgh Series, Volume 7. Pittsburgh: Pittsburgh University Press, pp. 3–70.

FREGE, G. [1884]: *Die Grundlagen der Arithmetik*. Breslau; Koebner; *The foundations of arithmetic*, translated by J. Austin, second edition. New York: Harper, 1960.

—— [1976]: *Wissenschaftlicher Briefwechsel*, edited by G. Gabriel, H. Hermes, F. Kambartel, and C. Thiel. Hamburg: Felix Meiner.

—— [1980]: *Philosophical and mathematical correspondence*. Hans Kaal, trans. Brian McGuinness, ed. Oxford: Basil Blackwell.

HALE, BOB [1987]: *Abstract objects*. Oxford: Basil Blackwell.

HELLMAN, G. [1989]: *Mathematics without numbers*. Oxford: Oxford University Press.

—— [1996]: ‘Structuralism without structures’, *Philosophia Mathematica* (3) **4**, 100–123.

—— [2003]: ‘Does category theory provide a framework for mathematical structuralism?’, *Philosophia Mathematica* (3) **11**, 129–157.

HILBERT, D. [1899]: *Grundlagen der Geometrie*, Leipzig, Teubner; *Foundations of geometry*, translated by E. Townsend, La Salle, Illinois, Open Court, 1959.

—— [1925]: ‘Über das Unendliche’, *Mathematische Annalen* **95**, 161–190; translated as ‘On the infinite’ by Stefan Bauer-Mengelberg in van Heijenoort

meant to say that (pure) mathematics and meta-mathematics can get by without making any assertions at all, then it is indeed our second option. For what it is worth, a number of other mathematicians and philosophers in the audience expressed bafflement at the second option.

[1967], pp. 369–392, and by Erna Putnam and Gerald J. Massey in Benacerraf and Putnam [1983], pp. 183–201.

—— [1935]: *Gesammelte Abhandlungen, Dritter Band*. Berlin: Julius Springer.

HUSSERL, E. [1900]: *Logische Untersuchen 1*. Halle a. d. S.: M. Niemeyer, translated as *Logical Investigations*, by J. N. Findlay. Amherst, New York: Humanity Books, 2000.

MCLARTY, C. [1993]: 'Numbers can be just what they have to', *Nous* **27**, 487–498.

—— [2004]: 'Exploring categorical structuralism', *Philosophia Mathematica* (3) **12**, 37–53.

RESNIK, M. [1997]: *Mathematics as a science of patterns*. Oxford: Oxford University Press.

SHAPIRO, S. [1991]: *Foundations without foundationalism: A case for second-order logic*. Oxford: Oxford University Press.

—— [1996]: 'Space, number, and structure: A tale of two debates', *Philosophia Mathematica* (3) **4** (1996), 148–173.

—— [1997]: *Philosophy of mathematics: structure and ontology*. New York: Oxford University Press.

VAN HEIJENOORT, J. [1967]: *From Frege to Gödel*. Cambridge, Massachusetts: Harvard University Press.

WILSON, M. [1993]: 'There's a hole and a bucket, dear Leibniz', *Midwest Studies in Philosophy* **18**. Minneapolis: University of Minnesota Press, pp. 202–241.

WRIGHT, C. [1983]: *Frege's conception of numbers as objects*. Aberdeen: Aberdeen University Press.