Some objections to structuralism*

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By "structuralism" in what follows I mean the structuralist view of mathematical objects. Although it has a history going back to Dedekind, readers will naturally think of views presented by writers on the philosophy of mathematics in the period since about 1980. Different versions of the view have been presented by Michael Resnik, Stewart Shapiro, Geoffrey Hellman, Charles Chihara, and me. The basic idea of the view can be put as follows:

> … reference to mathematical objects is always in the context of some background structure, and that the objects involved have no more by way of a "nature" than is given by the basic relations of the structure.[1]

In my view the main dimension on which to classify such views is whether they purport to eliminate reference to mathematical objects, at least the objects with which a treatment is primarily concerned. Programs that undertake that I call eliminative structuralism, others noneliminative structuralism. Michael Dummett's terms "hard-headed" and "mystical" are used with the same

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[1] Charles Parsons, *Mathematical Thought and its Objects* (Cambridge University Press, 2008), p. 40. This work is referred to as MTO.

extension in application to contemporary views, but they are highly tendentious.[2] What Shapiro calls *ante rem* structuralism is a species of noneliminative; Hellman's "modal structuralism" is a species of eliminative.

Elsewhere I have argued that eliminative structuralism cannot achieve its aim in the case of higher set theory, even if one grants that the typical use of second-order logic is not in conflict with the eliminative aim.[3] In this talk I will be at most tangentially concerned with eliminative structuralism, but some objections canvassed are aimed at either type of structuralism. The version that I have advanced myself is of the noneliminative type.

One feature of my own version is not sufficiently emphasized in what I have published, even in the "definitive" presentation in §18 of MTO. That is that the view and its presentation are not tied to any particular theory that serves as a "framework" or "foundation" of mathematics, as set theory does in many writings on mathematics and its foundations, and as perhaps category theory does for other writers. However, I have to confess that I have not studied the category-

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[2] Dummett, *Frege: Philosophy of Mathematics* (Cambridge, Mass.: Harvard University Press, 1991), p. 296. Strictly, no contemporary noneliminative structuralist is a mystical structuralist in Dummett's sense, because none is committed to Dedekind's idea that mathematical objects are free creations of the human mind. Probably others who have adopted Dummett's term don't intend to attribute this view to the "mystical structuralist." But it was surely the reason why Dummett used the adjective "mystical." Thus I think the usage described in the text is to be deplored.

In the abstract of this talk (**Bulletin of Symbolic Logic** 15 (2009), 454), Dummett is mentioned as a writer whose objections to structuralism are relevant. However, I have found that I have nothing to add on that subject to what is said in §14 of MTO.

[3] MTO §17. It is a delicate question how far this criticism applies to Hellman's version, the most worked out form of eliminative structuralism. His basic interpretation of second-order logic in *Mathematics without Numbers* (Oxford: Clarendon Press, 1989), p. 20, has the second-order variables ranging over classes of individuals, which can be impredicatively defined. By my lights, that means he does not aspire completely to eliminate commitment to mathematical objects. He does, however, consider more nominalist ways of interpreting his formalism. About the application to higher set theory, I would then make the same comment as I have made about Putnam's ideas (see MTO pp. 97-98).

theoretic alternatives to a sufficient degree to determine how what I have presented would be affected by the existence of that option. From what I do know, I am inclined to say that the basic objects would be different, but otherwise the issues would be the same.

This feature distinguishes my version of structuralism from that of Stewart Shapiro, probably the contemporary structuralism most widely discussed among philosophers. Neutrality is compromised by Shapiro's procedure of proposing a theory of structures and maintaining that structures are prior to the "systems" that realize them [4]. The theory looks in many ways like set theory, but also has significant differences. In particular, it appears that isomorphic structures are to be identified, although the theory does not explicitly state this.

This does not mean that according to me structures are not part of the ontology of mathematics. That would be hard to defend, since mathematical literature abounds in references to groups, fields, rings, topological spaces, and more complicated structures. But they are mathematical objects among others, no more fundamental than the objects in them or than sets and the numbers of the various number systems. Structures do play an essential role in stating structuralist views, but in my view where a set-theoretic concept runs out we can use a metalinguistic concept that introduces no new ontology.[5]

What, then, makes the view structuralist? Let us first consider the simplified situation where our discourse is about one type of mathematical

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[4] *Philosophy of Mathematics: Structure and Ontology* (New York and Oxford: Oxford University Press, 1997), ch. 3.

[5] See MTO, pp. 111-14.

object, which could be sets or natural numbers. Sets stand in a binary relation called membership. Leaving out, as is common in set theory, the complication of urelements, all that is specified about a set is what elements it has and what sets it is an element of. The axioms of set theory assert (typically conditionally) the existence of sets satisfying certain conditions, generally having as elements just the objects satisfying some condition. If urelements are ruled out, all of this is statable in a first-order language with 'x is an element of y' as sole predicate.[6] Writers reflecting on set theory often undertake to say something about what a set is, that it is formed from its elements, that it is a multiplicity that is a unity, perhaps that it is the extension of a concept or predicate, or the like. Although these ideas might play a role in explaining the axioms of set theory, perhaps even in persuading readers to accept them, they play no further role in proofs in set theory. These ideas compete with one another, but the axioms are noncommittal between them.

There is nothing in the theory that distinguishes between one system of sets and another isomorphic copy of it. Furthermore, the theory is silent about whether any sets are identical with objects given or described in some other way, in particular other mathematical objects such as numbers. It may be tempting to say that sets are *sui generis*, that no set is identical with any object given in some other way. How to put this point exactly may be a problem, because it is trivial to say that no set is identical to anything that is not a set. However, the idea can be realized by a typed language, in which there is a type of sets, or perhaps a

[6] The adequacy of the first-order language has been questioned on various grounds. I think it holds up very well, but to discuss the matter would take us too far afield.

hierarchy of such types, in which there may be other types. However, the *sui generis* view of a structure like one of sets, or the natural numbers, introduces a walling off of the structure from others or from other entities, which again is something additional to the structure.[7] It is thus dubiously compatible with the structuralist idea.

Where this consideration leads to potential controversy is when we think about set-theoretic constructions of number systems. In virtually every case there are alternatives. The case of different constructions of natural numbers by finite sets was dramatized by Paul Benacerraf in a famous paper.[8] He was led by his reflections to the conclusion that numbers are not objects at all. He hints at eliminative structuralism as a way to understand that. I will consider this question in the context of objections to structuralism.

II

I now turn to objections. A rather general objection is suggested in a late short paper by W. V. Quine. Although he describes his own ontological conception as a form of structuralism, extending beyond mathematics to reference to objects in general, at the end of the paper he writes:

> My global structuralism should not, therefore, be seen as a structuralist ontology. To see it thus would be to rise above naturalism and revert to the sin of transcendent metaphysics. My tentative ontology continues to consist of quarks and their compounds, also classes of such things, classes of such classes, and so

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[7] Such a walling off would be absurd if it ruled out maps of the domain of the structure into others, but it need not have that implication.

[8] "What numbers could not be," **Philosophical Review** 74 (1965), 47-73.

on, pending evidence to the contrary.[9]

By "structuralist ontology" Quine may mean an ontology of structures, such as plays a role in Shapiro's version. And indeed, Quine's mathematical objects are classes of physical things, classes of classes, and so on, so that they fall among ordinary mathematical objects even if they are a severely reduced version of them.

Like Quine, I take the usual mathematical language at face value, but unlike Quine (at least the Quine who does ontology) I am willing to take objects other than sets, such as natural and real numbers, as primitive in contexts where this is appropriate. It is not obvious that on this view structures will not sometimes be primitive, but in the sense relevant to Quine's remark I do not embrace an ontology of structures, because even if structures do arise as primitives, it will be in the context of some mathematical investigation in which they do not play the role of a universal ontology.

However, there's something else that Quine might have meant, and that is that the further gloss on discourse referring to mathematical objects that the structuralist offers is an objectionable form of metaphysics. Perhaps that could even be claimed of statements of the basic idea, such as my own statement quoted at the beginning of this paper.

The idea appears to be that the structuralist tries to say too much; saying what mathematical objects there are and making basic points about them (e.g. that they are abstract, typically pure abstract objects) is ontology enough. This idea may underlie some other literature critical of structuralism. But it is

[9] "Structure and nature," **The Journal of Philosophy** 89 (1992), 5-9.

impossible to evaluate without some specific instance of the structuralist's stepping beyond legitimate bounds. So I will put it aside. The reader will have to decide for himself whether I am guilty of the charge.

I want to start with a problem with the first part of the structuralist thesis, that the existence of mathematical objects is in the context of a structure. This was emphasized by Paul Bernays in an essay published some years before the English-language discussion of structuralism started.[10] He used the term *bezogene Existenz*, relative existence. If we think of the structure in the usual way, as involving definite relations, functions, and distinguished objects, then it is evident that it may not be unique, even in a simple case like the structure of the natural numbers. In other cases the ontology might not be uniquely determined by what we think of informally as the structure. For example, in Euclidean geometry we typically have points, lines, and planes, but it is possible to get by with merely points.

Bernays apparently has such situations in mind when he qualifies his own observation. The conceptual framework or "thought-system" within which some part of mathematical practice operates intends "a certain domain of mathematical reality" that is at least to a certain degree independent of the "particular configuration" of the framework.[11] The term translated "reality" is *Tatsächlichkeit*; one might also say mathematical facts. He seems to recognize that the idea of

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[10] "Mathematische Existenz und Widerspruchsfreiheit," first published 1950, reprinted in *Abhandlungen zur Philosophie der Mathematik* (Darmstadt: Wissenschafliche Buchgesellschaft, 1976), pp. 92-106.

[11] Ibid., p. 102. In these brief quotations I have used the translation of the paper by the Bernays Project at Carnegie-Mellon University.

relative existence loses some of its sharpness by being viewed in this way.[12]

I don't know whether Bernays thought of this point as an objection to the first thesis. I think it amounts to just pressing a little further the point that the structure to which a mathematical object belongs might not be uniquely determined, and sometimes it has the consequence that what at first sight appear to be quite definite objects really are not. The choice of a structure in the precise sense may belong to regimentation of mathematical discourse for one or another foundational purpose and not necessarily to informal mathematical discourse.

Another point regarding the first thesis is that we should not think of it as expressing a kind of ontological or metaphysical dependence of the objects on the structure. That could not even be stated without an ontology of structures, and the nonuniqueness of the structure could give rise to a number of difficulties. Some difficulties of this kind were raised some years ago by Hellman, and the matter is pursued further in a recent paper by Øystein Linnebo, which I will refer to later in another context.[13]

III

We might look once again at the rather hackneyed question of the identity, or lack thereof, of numbers and sets. The background of Benacerraf's paper and its impact indicate that the question arises independently of

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[12] Ibid., p. 104.

[13] Hellman, "Three varieties of mathematical structuralism," **Philosophia Mathematica (III)** 9 (2001), 184-211; Linnebo, "Structuralism and the notion of dependence," **Philosophical Quarterly** 58 (2008), 59-79.

Possibly more troubling, but concerned with the second thesis, is Linnebo's suggestion that a natural number depends metaphysically on the earlier numbers and that a set depends on its elements. This problem is commented on in section VI below.

structuralist views. The issue is really a special case of a more general problem: There are too many construals of the natural numbers, which in general propose different and often incompatible answers as to "what the numbers are." Frege and Russell defined the numbers so that their cardinal role is intrinsic to them, while Dedekind rejected such a procedure. Different versions that make them the order types of finite sequences are possible. Hilbert and Bernays, in their expositions of the finitary method, treat the numbers as strings of signs.[14] Zermelo and von Neumann made different proposals for construing the numbers as finite sets. Church defined the numbers by λ-terms. Even the introduction of the natural numbers by "Dedekind abstraction," which models neatly the basic structuralist idea, has something optional because it treats them as a syntactically distinct type.[15]

I don't know of any serious argument for the view that one of these construals represents the truth about the natural numbers, so that the others are false. The view that they are all false or meaningless, so that the numbers are *sui generis*, does have some appeal. It would be harder to argue for this on behalf of all the number systems: the integers, the rationals, the reals, and the complex numbers. Even if that can be done, some account is needed of what appear to be statements of identity between numbers and sets or other objects described otherwise. It seems to me that the best answer the opponent of structuralism can give is that all have an element of fiction. There are different possibilities as to

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[14] It doesn't follow that their view was that the natural numbers *are* strings of signs, as some writers have said.

[15] W. W. Tait, "Truth and proof," **Synthese** 69 (1986), 341-370, p. 369 n. 12; cf. the comment in MTO, pp. 104-05.

where the fiction might lie. One might say that the identities are not really asserted. If the definitions in a system of set theory are treated as abbreviations, then it would be natural to say what is a fiction is that the numerals thus introduced designate numbers; actually they designate sets that are surrogates for numbers in a certain model.

More congenial to noneliminative structuralism is the view that such statements are context-dependent. It has as a consequence, however, that the reference of numerals is context-dependent. Thus in a Zermelo-numbers development one derives $2 = \{\{\emptyset\}\}$; in the von Neumann development one derives $2 = \{\emptyset, \{\emptyset\}\}$, but surely in both contexts the set terms designate different sets. The point is even more evident if one considers an interpretation in set theory and one in the λ--calculus.

It follows that numbers are not "definite objects," in the Fregean phrase. That is probably a concession to Benacerraf's view, but one doesn't have to go all the way with him: There is still reference to objects when numerals are used. But if the numbers are treated as a stand-alone background structure, there isn't a background of more fundamental objects. The claim that this is somehow the canonical or genuinely correct way to talk about numbers, with all the others involving either some fiction or pretense or the segregation of a type of natural numbers, is not plausible when one looks at the variety of ways in which natural numbers are talked about.

IV

I want now to turn to an objection that has been around for some time, to which I have already replied in print.[16] This is the point raised by John Burgess and Jukka Keränen, and pressed at some length by the latter, that in the case where a structure has non-trivial automorphisms, the relations of a structure are insufficient to individuate the objects in it, and there is nothing else the noneliminative structuralist can appeal to. As Burgess put the point:

> The situation changes, however, when we come to the complex numbers. There we have two roots of the equation $z^2 + 1 = 0$, which are additive inverses of each other, so that if we call them i and j we have $j = -i$ and $i = -j$. But the two are not distinguished from each other by any algebraic properties, since there is a *symmetry* or *automorphism* of the field of complex numbers … which switches i and j. On Shapiro's view he two are distinct, though there seems to be *nothing* to distinguish them.[17]

Homogeneous structures such as Euclidean space are even worse off.

One might reply to this objection in a dismissive way, by asking why a structure cannot have distinct places with *nothing* to distinguish them. In the case of small structures like simple finite graphs, even in homogeneous cases it seems evident that we have a coherent conception of a structure. This idea could be implemented by treating identity as one of the basic relations.

The reply I have offered is much more conciliatory. It distinguishes between basic and constructed structures. Basic structures are what is assumed

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[16] "Structuralism and metaphysics," **Philosophical Quarterly** 54 (2004), 56-77, sections III-IV; MTO pp. 107-09. Shapiro replies to this objection in "Structure and identity," in Fraser MacBride (ed.), *Identity and Modality* (Oxford: Clarendon Press, 2006), pp. 109-145, and in "Identity, indiscernibility, and *ante rem* structuralism: the tale of *i* and *-i*," **Philosophia Mathematica (III)** 16 (2008), 285-309.

[17] Review of Shapiro, *Philosophy of Mathematics*, **Notre Dame Journal of Formal Logic** 40 (1999), 283-291, pp. 287-88.

as building blocks of other mathematical objects. The natural numbers and the well-founded sets are obvious candidates. The real numbers are a natural candidate. These structures are all rigid. But the complex numbers can well be treated as a constructed structure, as are most of the structures that arise in developed mathematical research. "Constructed" may not have been the best term; "derived" or "defined" might be better.[18] But I will stick to the one I am used to.

There are, however, some structures one would like to treat as basic that are not rigid. The Euclidean plane and three-space are primary examples. One could not give a structuralist interpretation of pre-nineteenth century mathematics without taking them in this way. Historians might regard that as no great loss: the structuralist idea arose with characteristically modern developments, for example the rise of abstract algebra, the "arithmetization of analysis," and the developments in geometry.

Hannes Leitgeb and James Ladyman discuss examples from graph theory in connection with this issue and intimate that graph theorists treat certain simple graphs as basic structures, even when they are homogeneous.[19] In this case, it is far from clear that the basic character is needed. But one might view the problem as that of establishing the coherence of the description of the structure. In these simple cases, this could be taken care of in a number of ways, for example by exhibiting an instance. I believe that the tradition had what could be described as a way of making out the coherence of the theory of Euclidean

[18] The latter term was suggested by D. A. Martin.

[19] "Criteria of identity and structuralist ontology," **Philosophia Mathematica (III)** 16 (2008), 388-396.

space, but I cannot pursue that matter here.

Even in that case, however, it is not necessary for modern mathematics that the structure be treated as basic. But a problem arises for constructed structures, illustrated by the following remark of Shapiro:

> One option is to interpret complex analysis in another, rigid structure, or, perhaps better, to *replace* complex analysis with a rigid structure. For example, if one thinks of the complex numbers as pairs of real numbers, then our problem is solved. One stipulates that *i* is the pair (0, 1), in which case -*i* is the pair (0, -1). Those pairs are distinguishable from one another in $\mathbf{R}^2$. Given how pervasive non-rigid structures are, however, I would take this to be a last resort, only to be invoked if we cannot do better. In line with faithfulness, I take it that, other things equal, it is better to take the languages of mathematics at face value.[20]

Although Shapiro's own replies to the objection have been on different lines from mine, I think the problem he expresses here is not with regarding the complex numbers as a constructed rather than a basic structure. This is shown by the proposal he makes a couple of pages later. That is, roughly, to introduce the term *i* as a parameter, on the analogy with the introduction of parameters in connection with existential quantifier elimination or (in different formulations) existential instantiation in natural deduction. Existential instantation says that if we have derived $\exists x A(x)$, one can introduce a new parameter b and infer $A(b)$. b can be interpreted as designating any object satisfying $A(x)$. If this idea is to model the introduction of a term like *i*, then unlike parameters in natural deduction the parameter then becomes a permanent part of mathematical

[20] "Identity, indiscernibility, and *ante rem* structuralism," p. 295.

language. John Burgess, one of those who has developed this theme, calls them permanent parameters.

The point relevant to objections to structuralism is that in many cases where a structure has been constructed, in going on to reason about the structure the fact that it has been constructed in one way rather than another is not only irrelevant to the further development but is often better forgotten. As it stands, even the term for the structure may originally refer to the construction (at least in a particular development of the subject), so that, as was pointed out by Richard Pettigrew, that too should become a parameter.

This logical idea has been used by Pettigrew and by Burgess to describe a lot of mathematical usage in a way that is in the general spirit of structuralism.[21] Burgess puts it into a wider context by applying it generally to cases of what he calls indifference to identification, which include kinds of examples beyond those that figure in the discussion of structuralism. Prima facie, however, it is applicable only to the case of constructed structures, and Burgess makes this limitation explicit. I am happy to regard it as a friendly amendment to my own reply to the individuation objection.

V

It has been very common to reject structuralism in application to set theory. A number of grounds have been offered. Some, including the one of my own mentioned above, touch only eliminative structuralism, and I will pass them over. A natural question to ask, before one goes into the details of formulating

[21] Richard Pettigrew, "Platonism and Aristotelianism in mathematics," **Philosophia Mathematica (III)** 16 (2008), 310-332; John P. Burgess, "Putting structuralism in its place," unpublished text of a lecture to a conference at New York University, April 2009.

the structuralist view, is whether the universe of sets is to be viewed as a structure. The question whether our understanding of set theory determines a unique structure of all sets is a contested one; the intuition that it does (evidently shared by Gödel) is far from universal. The structuralist view should not presuppose that. By taking the language of set theory as it stands, what the structuralist needs to do is to avoid further moves that signify a stand on this contentious question.

The metalinguistic conception of structure is of help here. An ontology of structures would make a structure for set theory, if not a set, then something closely analogous to a set. But the metalinguistic conception means that the generalization involved in talking of sets as a structure is just semantic ascent. Nonuniqueness will imply that there is some ambiguity in the quantifiers of the language of set theory when used "straight," without reference to a specific model. Since what is assumed in normal investigation are definite axioms, nothing is asserted that does not follow from these axioms.

The fact that set theory plays the role of a general framework for mathematics implies that nonuniqueness, if it obtains, differs from what arises in the more algebraic concepts of structure, where we have "axioms" that from the beginning are intended to characterize a *type* of structure. By contrast, in doing set theory we often talk as if our quantifiers have a single definite range, even if on reflection we would question this.[22]

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[22] The question of the uniqueness of the universe of sets recalls the question of the uniqueness of the natural numbers. In the latter case we have Dedekind's categoricity theorem; in the former we have Zermelo's quasi-categoricity theorem. In both cases the question arises whether the theorem can be taken at face value. The natural number case is treated extensively in MTO §48-49. But the case of sets has difficulties that do not arise in the number case, in addition

There is an objection to a structuralist view of set theory that I discussed in MTO, following an earlier paper. The objection claims that in order to motivate and give some kind of justification to the axioms of set theory, we need an ontologically richer conception of set. One such conception is that of a collection, an object that consists of or is constituted by its elements.[23] It has to be the case that the elements are not fused into the collection; otherwise a collection would collapse into a mereological sum. Two alternatives are that of extension, derived from Frege's conception, and that of plurality, derived from plural constructions. Taking the latter as a possible conception of set involves regimenting the plural by the singular, contrary to the practice of plural logicians.

My response to this objection is that none of them is adequate to motivate the axioms of set theory, and that different axioms get some degree of evidence from different conceptions, as well as from other ideas of a more global character, such as that of "limitation of size." Considerations of an a posteriori character, because they rest on logical relations, are neutral between conceptions of what a set is, and therefore have no bearing one way or the other on how much weight ontological conceptions of set can have.

I have only stated the claim; the argument involves examining individual axioms. I refer the reader to my previous publication on the subject.[24] Although I have not discussed axioms beyond ZFC individually, I don't know of any

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to the evident fact that Zermelo's theorem allows standard (second-order) models to differ with respect to the length of the sequence of ordinals.

[23] This use of the term "collection" should be distinguished from that in which it is a generic term for any entity that can play the role of a set or class.

[24] See MTO ch. 4, which incorporates "Structuralism and the concept of set," in Walter Sinnott-Armstrong (ed.), *Modality, Morality, and Belief: Essays in honor of Ruth Barcan Marcus* (Cambridge University Press, 1995), pp. 74-92.

reason to think the considerations would be different if they are taken into account. I know of only one discussion in the literature that seems to take issue with mine, in the paper by Linnebo cited in note 13.[25] Linnebo argues that a particular ontological conception, that of collection, is an adequate foundation for a part of set theory, the theory of hereditarily finite sets.

I don't think my argument is Linnebo's immediate target. But whatever his intention, the conclusion is something I can readily grant, at least modulo reservations one can have about the clarity of the notion of collection. It is not my view, nor should it be any structuralist's view, that we can't have "nonstructuralist" understandings of limited domains of mathematical objects. In my writings there is emphasis on a particular kind of case, what I call quasi-concrete objects. But I also suggest a somewhat phenomenological conception of the hereditarily finite sets and offer genetic stories about numbers, where there is certainly talk of numbers that is not structuralist.[26] If that were not possible, we would not be able to understand what either Frege or the neo-Fregeans are talking about, still less what mathematicians before the nineteenth century were talking about.

The structuralist view of mathematical objects is a thesis about what the central talk of objects in developed modern mathematics amounts to. It is not and should not be a thesis about any possible or sensible reference to mathematical objects. In defending it, one needs to put it in its place.

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[25] "Structuralism and the notion of dependence," pp. 73-74.

[26] MTO, §§32-34.

VI

Linnebo's paper raises a more challenging question in proposing that a given natural number is ontologically dependent on numbers earlier in the sequence, and a well-founded set is similarly dependent on the sets in its transitive closure. The priority of the elements of a set to the set is quite naturally interpreted in that sense. And speaking for the same view about natural numbers is the thought that, in whatever sense in which we can speak of the possible nonexistence of a number, it is necessary that if a given number, say 5, exists, then the preceding numbers exist. One can put it by saying that if 5 exists, 4 must exist; if 4 exists, 3 must exist, and so on. In the case of sets, it is tempting to infer from extensionality that what a set is is to be the set that has just the elements it has. At least for well-founded sets, we then have reason to think that a set depends on its elements. And I don't think structures of non-well-founded sets, such as arise from theories incorporating Aczel's anti-foundation axiom, are very plausible candidates to be basic structures.

The problem these intuitions pose for structuralism is that such dependence, if it exists, is something nonstructural of a different nature from either what I call external relations or metaproperties such as being abstract.[27] In fact, the claim about sets is a new consideration favoring the objection I address in chapter 4 of MTO, that understanding set theory and making it plausible require understanding an ontologically richer notion of set than the structuralist view allows.

One might reply to the dependence objection about sets that what does

[27] On external relations see MTO §14, on metaproperties p. 107.

mathematical work is simply the priority ordering, more accurately the well-founded character of the membership relation. I don't think this reply is adequate because it may be that we may still need a structure of sets with the dependence in question to convince ourselves of the coherence of set theory. But I will leave the matter there for the moment and turn to the simpler case of natural numbers.

What seems a minimal necessary condition for the existence of the number 5 is that five objects exist. (This may be too minimal for the taste of most of us; after all it is hard to see that there is any real mathematics without the full sequence of natural numbers, at least potentially infinite.) But we can easily conceive that the condition is satisfied by objects none of which depends on any other in any plausible sense. They might for example be marks on a paper or blackboard, not distinguishable from one another by their perceptible qualities although distinguishable by their positions.

Proper subsets of this set would satisfy the minimal condition for the existence of 1, 2, 3, and 4.[28] But no proper subset is distinguished in this way. I would infer from this that although the existence of 5 implies that of 4, we are not obliged to say that 4 enters into the constitution of 5.

It seems easy to conceive of the whole structure of natural numbers as witnessed by a sequence of objects that are ontologically on a par, in the sense that none depends on any other. A possible example would be points of space or

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[28] I would like to abstract from set theory and therefore not admit the argument that the empty set will satisfy the condition for 0. It is noteworthy that some of the more profound writers on the natural numbers thought of them as beginning with 1, Dedekind being a prime example. Frege disagreed, mainly, it seems, because he thought of the numbers as essentially cardinals.

space-time. Even a more "potentialist" way of witnessing the structure, such as that in my own writings on intuition (in turn inspired by Hilbert and Bernays) can have the property that finite segments don't exhibit such dependence. Moreover, it is clear that properties of any such witnessing sequence need not be taken on board as properties of *numbers*.

The case about sets is more difficult because the idea that a set depends on its elements is so natural. In what may be the deepest study of the relation of the concept of set and modal notions, Kit Fine proposes as an axiom that if a set exists, its elements exist, i.e.

$$Ea \wedge x \in a \rightarrow Ex.$$

(The context is a free-logical formulation of modal logic.[29])

I mention that only to illustrate the force of the intuition; I don't want to continue in the modal-logical setting. My original argument can be extended to incorporate this intuition, since although it is satisfied by the notion of collection and plausibly by that of plurality, it is not at all evident that it is satisfied by the notion of extension. Quine regarded his New Foundations system as based on a formal trick, but certainly a notion of extension was an underlying idea, but the system (assuming it is consistent) cannot have a model in which $\in$ is well-founded, since a universal set exists. Other such theories exist that, unlike NF, are known to be consistent relative to standard theories, such as the New V of

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[29] Kit Fine, "First-order modal theories I: Sets," **Noûs** 15 (1981), 177-205; see also my *Mathematics in Philosophy* (Ithaca, NY: Cornell University Press, 1983), Essay 11, section I.

George Boolos.[30]

One might still not be persuaded by this reply. A more radical reply was suggested by Daniel Isaacson.[31] That was to say that classical pieces of structuralist mathematics, in particular Hilbert's *Foundations of Geometry* and Zermelo's paper of 1930, the treatment of Euclidean space (in Hilbert's case) and models of second-order set theory (in Zermelo's) as structures, where anything further that is said about the objects is irrelevant and the axioms *define* what the objects are, do not rely on any more or less intuitive conception of a structure satisfying the axioms. That suggests that in the discussion of the dependence objection the conversation should stop with my observation that it is only the well-foundedness of the membership relation that does mathematical work in set theory.

In addition to rejecting questions about the possible dependence of some objects in a structure on others, this view also coheres well with the dismissive attitude toward the issues about individuation discussed in section IV.

That picture describes accurately what proofs depend on and have depended on, in set theory at least since the work done in the inter-war period refining and extending Zermelo's axioms of 1908. But the context has been one in which set theory is a going concern, as geometry certainly was at the time of

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[30] "Saving Frege from contradiction," **Proceedings of the Aristotelian Society** N. S. 87 (1986-87), 137-151, reprinted in *Logic, Logic, and Logic* (Cambridge, Mass.: Harvard University Press, 1998). The point may be more intuitive if one accepts my reading according to which his V is a set. See my "Wright on abstraction and set theory," in Richard G. Heck, Jr, (ed.), *Language, Mind, and Logic: Essays in honor of Michael Dummett* (Oxford University Press, 1997), pp. 263-271.

[31] In discussion of an earlier version of this paper, University of Oxford, 11 March 2010. See also his paper "The reality of mathematics and the case of set theory," in Zsolt Novák and András Simonyi (eds.), *Truth, Reference, and Realism* (Budapest: Central European University Press, 2010), pp. 1-75. In places in this paper Isaacson's language seems to reject the whole idea of mathematical objects. But other remarks make me doubt that that is what he really intends.

Hilbert's work. So the question has to be asked: Whence these axioms, and not others? That is unavoidably a long story, which in the case of geometry stretches back into ancient times. The considerations offered to motivate the axioms of set theory that have been the focus of some of my own writing are directed at a more narrowly focused question.

Roughly, the question is why we should not regard set theory as we know it as empty, a theory that is not a theory of anything, or at least not of anything resembling what set theorists think they are talking about. Hilbert posed this question just after the *Foundations of Geometry* when he asked for a proof of the consistency of mathematical theories. In the famous program that he inaugurated after World War I, the ultimate aspiration was very likely to prove the consistency of set theory, but he did not offer any approach to a possible proof that would extend beyond second-order arithmetic. In the post-war period proof theorists have come to tackle some set theories, but they are weaker than second-order arithmetic, and it has been questioned whether a constructive proof of the consistency of second-order arithmetic is even possible in principle.

An obvious answer to the question why we should not regard the theory of a structure as empty is: mathematical experience. Arithmetic and geometry have been pursued for centuries, and with sufficient work at conceptual clarification there is harmony in the results. The same is true of analysis, in spite of the philosophical questions about it that were raised early in the twentieth century. This sort of answer was not absent from the writings of the Hilbert school. Hilbert's program can be regarded as a quest for something more absolute, which has turned out to be unattainable.

I believe that the answer offered by Isaacson in the paper cited in note 31 is along these lines,[32] but he usefully singles out certain aspects. Proofs of categoricity, where attainable, assure us that we are dealing with a definite structure. But prior to such proofs is the conceptual analysis and other work involved in rigorous axiomatization, such as was accomplished by Dedekind for arithmetic, by Pasch and Hilbert for Euclidean geometry, and by Zermelo and then Fraenkel and Skolem for basic set theory. It is there that Isaacson sees the main role of "informal rigor" in the sense of G. Kreisel.[33]

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[32] In particular see p. 41.

[33] "Informal rigor and completeness proofs," in Imre Lakatos (ed.), *Problems in the Philosophy of Mathematics* (Amsterdam: North-Holland, 1967), pp. 138-171.