PARSONS AND THE STRUCTURALIST VIEW

ABSTRACT. Parsons' characterization of structuralism makes it, roughly, the view that (i) mathematical objects come in structures, and (ii) the only properties we may attribute to mathematical objects are those pertaining to their places in their structures. The chief motivation for (ii) appears to be the observation that in the case of those mathematical objects that most clearly come in structures, mathematical practice generally attributes to them no properties other than those pertaining to their places in structures. I argue that in mathematical practice there are exceptions to (i), though how many depends on how strictly one takes (i), and that there is an alternative interpretation available for the facts about mathematical practice motivating (ii).

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1.   OVERVIEW

The involvement of Charles Parsons with structuralism goes back to the first appearance of the issue in the literature of contemporary analytic philosophy of mathematics, in the mid-1960s, before the 'structuralist' label had been attached to it, and when still 'structuralism' referred only to intellectual trends then in fashion in Paris. For while the origin of the issue of structuralism in the sense under discussion here is generally traced to the seminal paper of Paul Benacerraf [1965], which is wholly devoted to the issue, Parsons made some of the same observations, and advanced some of the same conclusions, in the same year, in the course of his very wide-ranging critical study of Frege's theory of numbers [Parsons 1965, §III].

Both Benacerraf and Parsons observe that multiple reductions of the natural numbers are available, with Benacerraf discussing mainly set-theoretic reductions, and Parsons logicist reductions. Both suggest that nothing crucial to mathematical practice will be affected by which reduction one chooses, and conclude that there is no basis for claiming that the natural numbers literally *are* what set-theory or logicism would reduce them to. A difference is that Benacerraf goes on, as Parsons does not, to end his paper

by suggesting that there are no such things as the natural numbers, though he expresses the thesis in a paradoxical way. Much as Santayana's view was, according to Robert Lowell, that there is no God and Mary is His mother, so Benacerraf's view seems to be that there are no numbers, and infinitely many of them are prime.

A difference between Benacerraf and Parsons is that the former is concerned with mathematicians, Zermelo and Von Neumann, who in offering their reductions of natural numbers never claimed to be revealing what natural numbers really had been all along; whereas Frege, the author with whom Parsons is concerned, seems to be committed to some claim in such a direction. Frege in this respect contrasts with his fellow logicist Russell, who while proposing an identification very similar to that put forward by Frege, one that makes out the number two, for instance, to be the class of all two-membered classes, repudiated any claim to be discovering what was there all along, writing apologetically as follows:

> So far we have not suggested anything in the slightest degree paradoxical. But when we come to the actual definition of numbers we cannot avoid what must at first sight seem a paradox, though this impression will soon wear off. We naturally think that the class of couples (for example) is something different from the number 2. But there is no doubt about the class of couples; it is indubitable and not difficult to define, whereas the number 2, in any other sense, is a metaphysical entity about which we can

never feel sure that it exists or that we have tracked it down. It is therefore more prudent to content ourselves with the class of couples, which we are sure of, than to hunt for a problematical number 2 which must always remain elusive. [Russell 1919, p.18]

Benacerraf's position would in Parsons' terminology be called *eliminative* structuralism, and opposition to eliminativism has been an enduring feature of Parsons' writing on structuralist views. This rejection of eliminativism puts him in opposition to the 'hard-headed' or '*in re*' varieties of structuralism found in the work of the nominalists Geoffrey Hellman [1989] and Charles Chihara [2004], and in a sense, in the work also of the anti-nominalist David Lewis [1991, 1993]. Parsons' differences with the 'mystical' or '*ante rem*' approaches of Michael Resnik [1997] and Stewart Shapiro [1997] are more subtle, and to me more elusive.

Parsons returned in the 1980s to the matter of multiple reductions in another wide-ranging critical study, this time of Quine on the philosophy of mathematics [Parsons 1982, §IV]. Both the Frege and the Quine papers are reprinted in the volume of selected papers [Parsons 1983], where the Introduction (end of §I) adds some further remarks. But the first full-scale paper devoted *wholly* to the topic is [Parsons 1990].

This paper opens with a characterization of structuralism intended to cover all versions, both those with which Parsons sympathizes and those

with which he does not, that continues to be used in Parsons' later work:

(A)   Reference to mathematical objects is always in the context of some
        background structure, and the objects involved have no more to them than
        can be expressed in terms of the basic relations of the structure.

This formulation is followed by an apparently approving quotation from

Resnik of a passage asserting that mathematical objects are 'structureless

points or positions in structures'.

Note that while discussion of structuralism began with the case of the

natural numbers, the claim in (A) is perfectly general, and nowadays one is

not perhaps considered a full-fledged structuralist unless one is willing to

uphold a thesis like (A) pretty much across the board. By this criterion

Quine is not a structuralist, despite favoring something like structuralism for

all mathematical objects except sets, precisely because he does make an

exception for sets. Nor is Parsons a full-fledged structuralist, since he makes

an exception for *expression types*, of which more later.

The paper from which (A) comes was reprinted in [Hart 1996], and

followed by two substantial sequels, one on structuralism and the concept of

set, originally appearing in an obscure venue in 1995, but more readily

available in the reprint [Parsons 1997], and another on structuralism and

metaphysics [Parsons 2004].

In the latter Parsons makes a distinction between 'basic' and 'constructed' or derived structures, which among other things lets him avoid having to claim that each mathematical object has a unique home structure: If the natural numbers form a basic structure, and the rational numbers and so on derived structures, then it is perfectly possible for an object with a place in the natural number system to have also a place in the rational and other number systems. The wording of (A) indeed already leaves an opening for such a state of affairs, insofar as it only requires that every reference to an object must involve some structure, and not that it must be the same structure for every reference.

The last sentence of the abstract to [Parsons 2004] is a word of warning that every writer on this topic should take to heart:

> Ideas from the metaphysical tradition can be misleading when applied to the objects of modern mathematics.

Though this is not the main point of the statement, do note the word 'modern', which the larger context suggests means what elsewhere might be called *late modern* as opposed to *early modern*: Though I have not mentioned the fact so far, and though it is perhaps seldom mentioned by structuralists as explicitly as it is in the above statement from Parsons, the concern throughout is with the objects of present-day mathematics, back to

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the late nineteenth century, but not much earlier than the time of Richard Dedekind, though as with almost anything precursors of still earlier date can be found if one is looking for them.

In speaking below of 'mathematical practice' we must understand primarily present-day, professional mathematical practice. Indeed, we had better understand present-day, professional practice in *core* mathematics, to the exclusion of, among other things, the practice of set-theorists. For it simply is not true that it makes no difference whether one follows Zermelo or Von Neumann, if one is concerned with set theorists' practice. For set theorists, Zermelo's reduction, which applies only to the natural numbers, is merely an historical footnote, while Von Neumann's reduction, which generalizes to identifying a cardinal with the least ordinal of that cardinality, and an ordinal with the set of its predecessors, is more or less taken for granted. In my experience, set theorists when writing for each other generally will abbreviate

> Let κ and λ be uncountable cardinals, and let f be a function from the set of ordinals of cardinality less than κ to the set of ordinals of cardinality less than λ.

to

> Let κ and λ be uncountable cardinals, and let f be a function from κ to λ.

And this abbreviation presupposes the Von Neumann identifications. Along with set theory we had better exclude from consideration other areas of logic and foundations, and also category theory (which Parsons himself does explicitly set aside).

Mathematical Thought and Its Objects ([Parsons 2008], henceforth *MTO*) incorporates most of the substance of [Parsons 1990, 1997, 2004] and offers considerable further elaboration. Moreover, wide-ranging as the discussion of structuralism is in *MTO*, the coverage of the objects of mathematical thought is wider still. For the treatment of structuralism in *MTO*, mainly in chapters 2 and 3, is preceded by a treatment in chapter 1 of the notions of 'object' and 'abstract object' more generally. It is here, for instance, that Parsons discusses what he calls *quasi-concrete* abstract objects, a classification that includes expression types, and the contrast between them and pure abstract objects. But even *MTO* is not Parsons' last word on the subject, since a reply to critics is in the works, though I have at the time of this writing seen it only in a draft form, not to be quoted, and will therefore leave it out of account here.

In *MTO* the preliminaries are followed by a long critique of eliminativism, including an extended discussion of modality. I will not say much about Parsons' anti-eliminativism here, having said most of what I

would have to say already, in a review [2008]. But while passing over most of the anti-eliminativist material, I should not fail to mention the discussion of structuralism and applications in §14 of *MTO*.

Here Parsons treats a point on which Benacerraf originally went astray and eventually was obliged to publish a recantation [Benacerraf 1996]. Consider the structuralist claim that the natural numbers, say, have none but structural properties, summed up in the property of their all together forming a *progression* in which each has a distinctive place (0 as initial, 1 as next, 2 as next, and so on). To accept this claim is not to deny that natural numbers have external relations to other objects, but only to insist that all such external relations derive from their structural relations to each other. In particular, natural numbers, even when conceived structuralistically, can still serve as answers to 'how many?' questions, since the elements of any progression can. One simply defines a given element of the progression to represent the number of *F*s if and only if there is a correspondence between the *F*s and the elements of the progression coming before the given one.

Finally in §18 Parsons briefly sketches his own positive view, a version of structuralism that, while firmly anti-eliminativist, is at the same time supposed to differ from the mystical kind of anti-eliminativism, though it does so in ways that I will not attempt to summarize. This section also

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hints at the view on what structures *are* that Parson wants to defend, a kind of 'metalinguistic' view. I will not have much to say about it, because Parsons himself does not have much to say about it. He acknowledges that his approach will involve an ontological commitment to linguistic expression types, which is why he cannot be an across-the-board structuralist, but says little about obvious ideological commitment to a notion of satisfaction, though as an author of an important and influential paper on the liar paradox, he must know how problematic this notion of 'satisfaction' can sometimes be. Some of the more negative parts of the discussion, arguing that the notion of 'structure' found in core mathematics cannot be identified with the set-theoretic notion of 'structure' that we find in model theory, for instance, I will partly be repeating below, though my discussion derives less from Parsons than from *obiter dicta* of my dissertation supervisor Jack Silver that I heard as a student many years ago.

In sum, I will not be attempting to do anything like justice to Parsons' discussion as a whole. I will focus almost exclusively on his formulation (A), and I will evaluate it only from a single point of view, namely, with regard to how well or poorly it agrees with and helps make sense of mathematical practice. In the jargon of Strawson, I will be examining (A) as 'descriptive metaphysics' rather than 'revisionary metaphysics'.

In §2 below I will take note of several sorts of abstract objects to which a structuralist account would not apply, moving here beyond Parsons' category of *quasi-concrete* exceptions to the larger category of *abstractions* so much discussed in the literature on so-called neo-logicism. The claim of structuralism concerning such objects presumably must be that they no longer play a role in mathematical practice, if they ever did.

In §3 I will look at the first half of (A), which says that mathematical objects come in structures, and consider closely whether this seems to be true of the objects under discussion in elementary number theory (as that subject is actually conducted in mathematical practice, not as it might in principle be 'regimented'). My conclusion will be that mathematical practice refers many sorts of mathematical objects that, even if they do not seem to be conceived of as neo-logicist abstractions, equally do not seem to be referred to in connection with some background structure, let alone as 'structureless points' in such a background structure. It is here that I will draw on the material from Parsons and others alluded to above about the differences between the working mathematician's conception of 'structure' and the set-theoretic conception.

In §4 I will consider what is left of the second half of (A) if the first half is admitted to have many exceptions. On one level it is obvious what is

left, namely, the thesis that in the case of those mathematical objects that *do* come in structures — the natural number system, with which structuralism began, being the most plausible candidate — all there is to them is what follows from their occupying the places they do in their structures. My conclusion will be that it is not so obvious why one should, as (A) does, go beyond saying *all we are justified in taking there to be to them* is what follows from their occupying the places they do in their structures, and say outright that this is *all there* is *to them*. I locate the border between eliminativist and mystical structuralism in the gap between these two formulations.

In §5 I close by sketching an alternative explanation of some of the features of mathematical practice that have motivated structuralism — one that unlike the structuralist interpretation involves no special claims about the ontological character of mathematical objects. Here I will mainly be developing themes I have discussed in a series of lectures over the past several years (on which Parsons had some comments in the work-in-progress alluded to earlier), and in a less condensed form intended for a non-specialist audience in the latter half of chapter 3 of my recent book [2015]. Though I will be differing from the letter of some of what Parsons says, I believe I will still be in agreement with the spirit of much of it, and above all

with the spirit of Parsons' warnings about 'the metaphysical tradition'.

2.   ABSTRACTION

Mathematical objects come 'not single spies, but in battalions'. One
never posits or recognizes or introduces into one's theorizing a single,
isolated mathematical object, but always some whole *sort* of object. Often
objects of the newly-posited or -recognized sort are introduced and
explained in terms of their relations to other sorts of objects already posited
or recognized. In particular this is so in the case of *abstraction*, where the
entities of the 'new' sort, *equivalence types*, are conceived as what entities
of some 'old' sort that are equivalent in some way thereby have in common.

Ordinary language and commonsense thought quite freely engage in
abstraction, moving from recognizing that objects in some sense are
equivalent or 'have something in common' to recognizing some *thing* as
what they have in common. The paradigmatic case is Frege's example: lines
that are parallel or like-directed thereby have in common their *directions*.
Likewise figures that are similar or like-shaped thereby have in common
their *shapes*. In connection with linguistic expressions, the relation of type to
token closely resembles the relation of shape to figure. These examples are
cases of abstractions connected with equivalences among entities that are

arguably concrete, and so arguably are examples of Parsons' category of the quasi-concrete; but the category of abstractions is broader.

The geometric relations of parallelism and similarity are two-place, and the general notion of an equivalence relation — a reflexive, symmetric, transitive relation — is one applicable to two-place relations. A closely analogous notion, however, can be considered for four-place relations, or more generally for relations with an even number of places. For instance, proportionality of geometric magnitudes such as lengths is a four-place relation that has the properties analogous to reflexivity, symmetry, and transitivity:

$A : B :: A : B$
if $A : B :: C : D$ then $C : D :: A : B$
if $A : B :: C : D$ and $C : D :: E : F$ then $A : B :: E : F$

In such a case the analogue of an equivalence type may also be posited or recognized, and these will in the case of proportionality be *ratios*. Euclid already in effect recognizes ratios, and Omar Khayyam in commentary proposed that such geometric ratios could be consider a kind of number, construing certain known geometric constructions as the arithmetic operations of addition and multiplication of ratios. This same geometric conception or real numbers is found in Newton's *Universal Arithmetick*, and

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is largely what the nineteenth-century 'arithmetization of analysis' aimed to replace.

The relations considered so far have been ones taking singular arguments: *this* is related to *that* by parallelism or similarity, or *this* and *this other* are related to *that* and *that other* by proportionality. But one may also consider relations taking plural arguments: *these* are related to *those*, and such relations in some cases have properties analogous to the properties of equivalence relations. Such is the case with equinumerosity:

> there are just as many xs as xs
> if there are just as many xs as ys, then there are just as many ys as xs
> if there are just as many xs as ys, and just as many ys as zs, then there are
> > just as many xs as zs

In this case the equivalence types produced by abstraction are *cardinal numbers*: What *these* have in common with *those* when there are just as many of these as of those is their number. Here, where the xs and ys may be things of *any* sort, we have definitely gone beyond the quasi-concrete.

Of course, if one already posits or recognizes ordered pairs, the four-place case reduces to the two-place case, by reconstruing a four-place relation as a two-place relation between pairs. And of course, if one already posits collections (sets or classes or the like) of some sort, the plural case may be reducible to the singular case, by reconstruing the plural relation as

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an equivalence relation between collections. But inversely the ordered pair can be viewed as obtained by abstraction from the four-place relation '$x = u$ & $y = v$'. And inversely collections can be viewed as obtained by abstraction from the plural relation 'each of the xs is among the ys, and each of the ys is among the xs'.

Indeed, Cantor's introduction of *sets* into mathematical theorizing may perhaps be construed as taking something like this form, a passage from speaking in the singular of, say, the points of discontinuity of a function, to speaking in the singular of the *set* of points of discontinuity, as an object, on which operations can be performed, such as the operation of discarding isolated points. Closely related to the general, abstract notion of a set, of which an object may or may not be an element, is the general, abstract notion of function, which applied to one object as input or argument may produce another object as output or value.

A very large range of objects familiar from traditional and early modern mathematics and incipient late-modern mathematics can thus without much artificiality be regarded as equivalence types of one kind or another, or as closely related to such equivalence types; and when so regarded their introduction takes the form of connecting them with the items of which they are the equivalence types or to which they are otherwise

specially related. Structuralism, by contrast, considers sorts of objects introduced as elements or points of a structure, and explained *only in terms of their relationships to each other* within that structure, any external relations being derivative from these. The next task must be to evaluate how well such a conception, as reflected in the first half of thesis (A), fits with mathematical practice.

3.   THE OBJECTS OF NUMBER THEORY

What sorts of mathematical objects are considered in basic number theory, and are they in mathematical practice treated structuralistically? I will not attempt an exhaustive catalogue, but merely take note of four diverse sorts of mathematical objects to be encountered.

Natural numbers. To begin with, of course, there are the natural numbers, which are higher arithmetic's principle object of study. It is notable that even in the purest pure number theory the natural numbers are treated not merely as elements of an algebraic system with an order relation (and addition and multiplication operations, though these are definable once the order relation is given), but also answers to 'how many' questions. For consider the Euler totient function ϕ, which makes a very early appearance in any introductory text. On the one hand ϕ is a function from natural

numbers to natural numbers, which means that $\phi(n)$ is a natural number, while on the other hand $\phi(n)$ is defined to be the number of natural numbers $< n$ that are relatively prime to n, which means that $\phi(n)$ is an answer to a 'how many' question. To be sure, as all structuralists today would insist, and as I have granted in §1, the elements of any algebraic structure of the right kind *can* be used as answers to 'how many' questions. Thus the fact that natural numbers are treated as answers to 'how many' questions is not *decisive* counterevidence against the claim that they are treated as mere featureless points in a structure; but it is hardly evidence *for* the structuralist claim, and *against* the rival hypothesis that they are treated as finite cardinals construed as abstractions.

Other numbers. As auxiliaries to the natural numbers, the negative integers are almost immediately introduced, to give us the full system of integers, and then the fractions, to give us the full system of rational numbers. At least some irrational and imaginary numbers make a fairly earlier appearance, ones of the form $m + n\sqrt{2}$ or of the form $m + ni$, say, with m and n integers. And there is not the slightest hint that the natural number 2 needs to be distinguished from the positive integer +2, or the latter from the rational number +2/1, or the latter from the real number 2.000…, or the latter from the complex number $2.000… \pm 0.000…i$. To be sure,

mathematicians do quite frequently admit to engaging in what they call 'abuse of language', and the commonest form thereof is the use of the same notation or terminology for conceptually distinct items. So the fact that all the same notation '2' and terminology 'two' is used for elements of $\mathbb{N}$ and $\mathbb{Z}$ and $\mathbb{Q}$ and $\mathbb{R}$ and $\mathbb{C}$ is not *decisive* evidence against the claim each kind of number belongs exclusively to its own home structure, though it is hardly evidence *for* that claim, or *against* the rival hypothesis that a number or other mathematical object is not tied to any one structure, but can reappear in several. But in any case, as noted in §1, formulation (A), unlike some less restrained rhetoric sometimes indulged in by structuralists, does not insist that every mathematical object has a single home structure, and Parsons' doctrine of constructed structures explicitly allows that it need not.

Sets of and functions on numbers. Along with the natural numbers we consider sets of them and functions on them. And I do not just mean that we consider certain particular sets, such as the primes, and certain particular functions, such as the Euler ϕ already mentioned, or the Möbius μ. I mean that *universally quantified*, general theorems about *all* sets of natural numbers and about *all* functions from and to the natural numbers are stated and proved early on. The well-ordering or least-number principle would be an instance for sets, the Möbius inversion formula for functions. And the

sets and functions here seem to be taken quite naively, as entities with intrinsic natures relating them to more basic entities, the sets as unities formed from pluralities of numbers, the functions as operating on numerical arguments and assigning them numerical values. They do not seem to be treated as 'structureless points or places in a structure'. (What structure would it be?)

But the formulation (A) contains a phrase 'in the context of' that is indefinite — or to put it more positively, guarded — enough perhaps to allow number-sets or number-functions can be construed as being 'in the context of' the background system of the natural numbers, even though the sets and functions are not, of course, literally points in that structure. At least it can be said that, if one changes one's concept of natural number, one will have to change one's concept of number-set or number-function along with it, and in this sense the sets and functions are tied to the structure, without being elements of or points in it, let alone featureless or structureless ones.

Structures. In present-day approaches to number theory one very early encounters a diverse range of groups, rings, and fields. Bourbaki popularized the label '(algebraic) structures' for the genus of which groups, rings, and fields are species. A difficulty for the structuralist is that core mathematicians do not seem to speak of such structures as the ring of

Gaussian integers $\mathbb{Z}[i]$ or the field $\mathbb{Z}_p$ of integers modulo a prime p — and

the student meets with these and many similar structures very early in the

study of number theory — as featureless points in some larger structure, or

make reference to them only in the context of some background structure.

(Or rather, if mathematicians do ever treat such algebraic systems as

featureless points, it is only in something like the more abstract context of

category theory, which is not encountered in elementary number theory, and

which we have set aside.) Well, perhaps all these examples, too, are to be

construed as derived or constructed structures, to which (A) is not to be

taken to apply directly; but if so, then it must be conceded that the basic

structures to which (A) *does* apply are comparatively few and far between,

and with them the mathematical objects that can properly be said to be

structureless or featureless.

Alternatively, the structuralist might be tempted to claim that the

various groups and rings and fields encountered in number theory are all

sets, and sets are ultimately structureless or featureless points. After all, the

Bourbaki group, in order to develop all the branches of mathematics within a

common framework, so that results from any one branch may be appealed to

in any other, adopt set theory as the only worked-out option for such a

framework (despite their having less than no interest in set theory as a

subject in its own right). Officially, a bourbachique structure is supposed to be a set-theoretic object of a specific kind: an ordered n-tuple consisting of a set in the first place, and various distinguished elements thereof and/or relations thereon and/or operations thereon and/or families of distinguished subsets thereof, in the other places. And most structuralists have wished to claim that set-theoretic objects, which is to say sets, are mere 'structureless points or positions in a structure', namely, that of the set-theoretic universe or *a* set-theoretic universe. (Recall that Quine was disqualified as a structureless on account of his not wishing to take this line.) But in the first place, nothing in mathematical practice suggests this structuralist view of sets, as already noted in connection with sets of numbers. And in the second place, the bourbachique identification of groups, rings, and fields with ordered n-tuples isn't in practice taken very seriously by mathematicians.

Is a topological group a triple $(G, \cdot, O)$ consisting of a set, a binary operation (group multiplication), and a family of distinguished subsets (the open sets of a topology)? Or is it a triple $(G, O, \cdot)$? Any one book may be expected to pick one convention and stick to it, but if two different books choose opposite conventions, no one will think that this circumstance makes them to be about different subject matters, groups-with-topologies and topological-spaces-with-group-operations. The same lecturer may use the

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one formulation one day and the other the next, if indeed the lecturer ever says anything more than that with a topological group we have a set or space and we have a group operation and we have a topology, in no particular order. Mostly in practice they will use the same letter G for a structure such as a topological group as for its underlying set, though this is admittedly an abuse of language.

Still less does a working mathematician ever think about whether an ordered triple (x, y, z) is an ordered pair $(x, (y, z))$ with an ordered pair in the second place, or an ordered pair $((x, y), z)$ with an ordered pair in the first place, or something else, or in practice think of a one-place operation or function as a set of ordered pairs, a two-place operation or function as a set of ordered triples, or whatever. Least of all does the core mathematician consider whether the ordered pair (x, y) is the set $\{\{x, \emptyset\}, \{y, \{\emptyset\}\}\}$ or the set $\{\{x\}, \{x, y\}\}$, or something else.

Some workers in foundations sometimes use a different notion of structure (perhaps metalinguistic, perhaps something else) from the set-theoretic one, so as to be able to consider the universe of sets as a whole as a structure, despite the non-existence of any set of all sets. But such conceptions play no role in core mathematics (and as for the metalinguistic view specifically, the very expressions 'predicate' and 'satisfaction', in

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terms of which workers in foundations may discuss such 'structures' hardly

belong to the vocabulary of the core mathematician). Core mathematicians

are even further from adhering to any such alternative view of 'structures'

than they are from *seriously* adhering to the set-theoretic view, since they

may at least pay lip-service to the latter on official occasions. In sum, the

scope of a claim like (A) must be significantly restricted if it is to be offered

as a description of established practice rather than a prescription for reform.

4.   ELIMINATIVISM *VS* MYSTICISM

Since the second half of (A) presupposes the first half, which I have

just called into question, what is left of (A) would be the claim that in the

case of those mathematical objects that *do* come in structures — the original

case of the natural numbers remains the paradigm case — they have the

'metaproperty' of having none but structural properties. (I believe I first saw

this word 'metaproperty' in Parsons. The notion enables us to dismiss the

complaint that the claim that mathematical objects such as natural numbers

have none but structural properties is self-contradictory, on the grounds that

having none but structural properties seems to be itself a property, and a

quite remarkable one.) But if we are looking at things from the standpoint of

mathematical practice, we should not be too quick to accept even this

formulation, which is in fact more congenial to mystical than to eliminativist structuralism.

Elimitativists wish to treat a symbol like $\mathbb{N}$, not as the name of some specific structure, but as something like a free variable ranging over all structures of a certain kind, in this case progressions. One is only justified in asserting a formulation involving a free variable if one would be equally justified in asserting it regardless of the value of that variable (within the intended range). And so on the eliminativist view, while one indeed is not justified in attributing to $\mathbb{N}$ any feature except that of being a progression, or to 0 any feature except that of being the initial element in that progression, or to 1 any feature except that of being the next-to-initial element in that progression, and so on, neither is one justified in denying a formulation involving a free variable unless one would equally justified in denying it regardless of the value of the variable. And so one would not, it seems, be justified in denying that the number two is Julius Caesar, since there is, after all, surely some progression in which Julius Caesar is the next-to-next-to-initial element.

Actually, however, eliminativists speak on two levels. Out of one side of the mouth the eliminativist will say that of course there is such a thing as the number two, since every progression has a next-to-next-to-initial

element. Out of the other side of the mouth the eliminativist will say that there is no such thing as *the* number two, since there are many different progressions with different next-to-next-to-initial elements. The former way of speaking is mathematical, the latter metamathematical. Santayanesque or Benacerrafic formulations such as 'There are no numbers, and infinitely many of them are prime' are examples of something like zeugma, where discourse drops from the meta-level to the object-level in mid-sentence.

The mystic, by contrast, is prepared to recognize $\mathbb{N}$ as a property-deficient entity, a structure of a certain kind, with the metaproperty of having no properties other than that of being a structure of this kind, namely, a progression. Now all progressions are isomorphic, and therewith have in common their *isomorphism type*. But an isomorphism type is not itself a structure, and in particular, not a structure isomorphic to the various structures of which it is the isomorphism type. The mystic's $\mathbb{N}$ is like Triangularity on a Platonic conception, in which the Form is itself triangular, while the isomorphism type is like triangularity on an Aristotelian conception, a universal exemplified by various triangles, but not by itself. Mystical structuralism is thus mystical in the same way in which Platonism is mystical. (I mean Platonism properly so-called: I am not using the word in accordance with the deplorably promiscuous, and historically absurd,

contemporary usage according to which to be a 'Platonist' it is enough not to be a nominalist.) The one difference between contemporary mysticism and Platonism is that the structuralist does not claim that the reason why various progressions met with in set-theoretic or logicist reductions *are* progressions is only because they 'participate' in $\mathbb{N}$.

$\mathbb{N}$ being property-deficient, its elements must be so as well. For if 2, say, the next-to-next-to-initial element, had the property, say, of having conquered Gaul, then $\mathbb{N}$ would have the property of having in its next-to-next-to-initial place a conqueror of Gaul. For similar reasons, if $0, 1, 2, \ldots$ are all property-deficient, then so must be the structure $\mathbb{N}$ to which they belong and that they, taken together as structurally related, constitute. Property-deficiency of the structure and of the various elements standing in various positions therein go hand in hand.

But all this business about property-deficiency is metaphysics, not to be mentioned in the course of mathematical practice. Since when doing mathematics the eliminativist will be speaking only at the object-level and not the meta-level, there need be and will be no difference in mathematical practice between the two versions of structuralism, just as there will be no difference in core mathematical practice between mathematicians who as

students may have been taught the Zermelo identification and those who as students may have been taught the Von Neumann identification. And this is to say that nothing said in mathematical practice seems to support a mystical as opposed to an eliminativist reading — or vice versa.

I have questioned in the preceding section how well any form of structuralism fits with mathematical practice, except in restricted cases where, say, the natural numbers are the only mathematical objects under discussion. Even in these restricted cases, I now say, it would be hard to claim that one version of structuralism fits mathematical practice better than another. It is only at a metamathematical, metaphysical level of discourse that the divergence between the two forms of structuralism even emerges.

Speaking at that level, the mystic will say that numbers lack spatiotemporal location, do not participate in causal interactions, and so on, while the eliminativist will say, 'Well, of course not, since there are no such things as numbers.' Suppose, for instance, some graduate student in physics, working on the missing-mass problem, concludes that the hypothesis that neutrinos have mass fails to solve the problem, and proposes instead to consider the hypothesis that *numbers* have mass. Both kinds of structuralists, as well as various kinds of non-structuralists, will agree that there can be no justification for saying that numbers have masses, but for the mystic this will

be because numbers are massless, while for the eliminativist it will be because they are nonexistent.

But if the absence of any difference in mathematical practice is enough to disqualify both the Zermelo and the Von Neumann identifications, why is it not also enough to disqualify both forms of structuralism? May there not be some other interpretation of mathematical practice that accounts for a wider range of phenomena, not restricted to the case of natural numbers and a few others, and not involving one in metaphysical disputes that are mathematically irrelevant? I believe there is.

5.   HARDY'S PRINCIPLE

By early in the last century, the new 'arithmetized' — which is to say, 'degeometrized' — foundation for analysis was not just accepted among sophisticated professional pure mathematicians in place of older conceptions like Newton's, but was being taught to their students as well. Accounts of Dedekind's construction began to find their way into textbooks for undergraduate students of mathematics, even freshmen, where one still finds them today. The Dedekind cut construction appears, for instance, already in the second (1914) edition of G. H. Hardy's *A Course of Pure Mathematics*.

Hardy's account of the construction is followed by an interesting

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remark:

> The reader should observe … that no particular logical importance is to be
>
> attached to the precise form of the definition of 'real number' that we have
>
> adopted. We defined a 'real number' as being … a pair of classes. We might
>
> equally well have defined it as being the lower, or the upper class; indeed, it
>
> would be easy to define an infinity of classes of entities each of which would
>
> possess the properties of the class of real numbers.

What Hardy is observing here is that the Dedekind construction admits many minor variants. He might have added that there is also a well-known rival construction due to Cantor, that takes real numbers to be given by equivalence classes of Cauchy sequences of rational numbers. That construction, too, admits many minor variants. And there are let other constructions (for instance, as equivalence classes of narrowing nested sequences of intervals). The common feature of all the constructions is that they give us a complete ordered field, and it is known that all such fields are isomorphic to one another.

Hardy comments on this situation in the following terms:

> What is essential in mathematics is that its symbols should be capable of *some*
>
> interpretation; generally they are capable of *many*, and then, so far as mathematics
>
> is concerned, it does not matter which we adopt.

We may call this last remark *Hardy's Principle*. I make a point of attaching

Hardy's name to the principle because the observation that it does not matter

for mathematical purposes what mathematical objects *are* is often credited to

Wittgenstein, who called Hardy a bad philosopher. (Incidentally, it seems to

be established that Wittgenstein read Hardy's book.) Hardy himself in effect

credits the observation to Russell. For he goes on to say the following:

> Bertrand Russell has said that 'mathematics is the science in which we do not
>
> know what we are talking about, and do not care whether what we say about it is
>
> true', a remark which is expressed in the form of a paradox, but which in reality
>
> embodies a number of important truths. It would take too long to analyze the
>
> meaning of Russell's epigram in detail, but one at any rate of its implications is
>
> this, that the symbols of mathematics are capable of varying interpretations, and
>
> that we are in general at liberty to adopt whichever we prefer.

In line with Hardy and with his take on Russell, different textbooks

introduce the real numbers in different ways, and while reviewers may find

one approach *pedagogically* superior to another, no one considers any

definition *mathematically* superior to any other, so long as that other still

suffices for deducing the basic properties of the real numbers, those of a

complete ordered field. Two analysts who wish to collaborate do not need to

check whether they were both taught the same definition of 'real number', as

conceivably two algebraists may have to check whether they are both using

the same definition of 'ring' (since on some usages the definition includes

having a multiplicative identity while for others it does not). For it is only

the properties of a complete ordered field that will be used in their

collaboration, and never the definition of 'real number'.

We have here so far merely another instance of the phenomenon of

multiple reductions, with which structuralism began, and most of what

Hardy says would be congenial at least to an eliminativist, if not to a

mystical, structuralist. However, Hardy's principle is not restricted to

claiming indifference between isomorphic structures, but is quite a bit more

general.

It would apply equally to, for instance, indifference between two

different set-theoretic definitions of ordered pair, such as Weiner's and

Kuratowski's, though that is not a case of the kind on which structuralist

philosophers focus. Ordered pairs as such do not form a 'structure' in the

way that natural or real numbers do, nor are they defined only in relation to

each other; rather, they are defined in relation to the objects of which they

are the ordered pairs. Presumably the eliminativist or mystical structuralist

could go on to claim that ordered pairs, too, are nonexistent or property-

deficient, despite their *not* being 'structureless points or places in a

structure'. But to do so would already be to concede that the emphasis on 'structures' was to some degree misplaced.

The scope of Hardy's principle, and of the indifference of mathematicians between various pairs of options, is in any case wider than the examples of number systems and ordered pairing by themselves bring out. Consider, for instance, the base e of the natural logarithms. It is certainly important that the symbol 'e' should have a meaning, and one that fits with the way the symbol is used by mathematicians; but is it important precisely what one defines it to mean? One may define it as the limit of a certain sequence, thus:

$$e = \lim_{n \to \infty} (1 + n^{-1})^n$$

and deduce as a theorem that it also the sum of a certain series, thus:

$$e = 1/0! + 1/1! + 1/2! + 1/3! + \dots$$

But one could equally well take the series characterization as the definition, and deduce the sequence characterization as the theorem. One book may do it one way, and another another, but from the standpoint of mathematical practice there is no saying that one book is right and the other wrong. It just does not matter which of the two characterizations is taken as definition, and

which as theorem. And this is a kind of indifference that has nothing to do with ontology. There is a significant feature of mathematical practice on display here, but a preoccupation with ontology is likely to get in the way of seeing it.

I do not have space to do more than hint at what I think the feature or phenomenon at work here amounts to. We may begin with the observation that the mathematics of the last hundred years or so is rightly held to have maintained a higher standard of rigor than that of any other period. The highest manifestation of such rigor might be thought to lie in the encyclopedic codification of Bourbaki, *Éléments de Mathématique*, a modern successor to Euclid's *Elements*. And yet, there is a discrepancy of sorts between mathematical practice in the Bourbaki codification.

Any rigorous development must logically deduce all results from postulates acknowledged at the outset, and logically define all notions from primitives acknowledged at the outset. That is what rigor amounts to. But to fulfill this requirement Bourbaki must make many choices — for instance of definition of real number system, of ordered pair, or of the number e — that are purely conventional, in the sense that various other choices would have done just as well. Mathematical practice as a whole does not itself make any definite choice in such cases. If some German rival to the French group were

to publish an *Elemente der Mathematik* making different choices, no one but the most atavistic nationalist would claim that one group was right and the other wrong.

The individual contributor X to a project like Bourbaki's, working say on chapitre 11 of tome 9, needs to deduce all new results and define all new notations logically from results and notions in tomes 1-8 and chapitres 1-10 of tome 9. But it is not incumbent upon X, when wanting to use a result or notion from tome 3, chapitre 7, to check how it was deduced or defined from first principles, the postulates and primitives in tome 1, chapitre 1. If X only needs the fact that the real number system is a complete ordered field, then how the real number system was defined will not matter for X; if X only needs the fact about ordered pairs that $(a, b) = (c, d)$ if and only if $a = c$ and $b = d$, then how ordered pairs were defined will not matter for X; if X only needs the series characterizations of e, then whether it was a definition or a theorem will not matter to X. And the requirement of rigor on an arbitrary working mathematician is simply to proceed *as if* one were in X's position and writing a chapter in some hypothetical comprehensive codification. That is why some aspects of previous work matter, while others can be forgotten about.

But how can we tell whether a given choice is going to matter? I do

not believe there is any simple formula to answer this question. Certainly the answer 'The choices that don't matter are those between isomorphic structures' will not do. This formulation ignores the fact I have been insisting upon, that there are other kinds of choices that don't matter, and it overstates the indifference of mathematicians to differences between isomorphic structures, since the whole large field of group representations, for instance, is in a sense concerned with nothing *but* differences between isomorphic groups. In the end, I think there is no way to answer the question except by observing mathematical practice. Perhaps some general principles will emerge from such observation, but at this stage I have none to offer.

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