PUTTING STRUCTURALISM IN ITS PLACE

ABSTRACT

One textbook may introduce the real numbers in Cantor's way, and another in Dedekind's, and the mathematical community as a whole will be completely indifferent to the choice between the two. This sort of phenomenon was famously called to the attention of philosophers by Paul Benacerraf. It will be argued that structuralism in philosophy of mathematics is a mistake, a generalization of Benacerraf's observation in the wrong direction, resulting from philosophers' preoccupation with ontology.

John P. Burgess
Department of Philosophy
Princeton University
Princeton, NJ 08544-1006, USA
jburgess@princeton.edu

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I

Philosophical disagreement over whether the continuum problem admits an objective solution takes place against a background of agreement that the problem cannot be solved simply through proof or disproof by ordinary mathematical means. This agreement rests on the one hand on the work of Gödel and Cohen on the consistency and independence of the formula CH from the formal system ZFC, and on the other hand on acceptance of provability in ZFC as a good model of provability by ordinary mathematical means. This has been generally accepted by mathematicians in the sense that in the wake of Gödel’s and Cohen’s results it was generally accepted by mathematicians that they would be wasting their time trying to disprove the continuum hypothesis by ordinary mathematical means. In drawing this conclusion, one is tacitly endorsing the claim that formal derivability in ZFC is a good model for provability by ordinary mathematical means.

It is not pretended that formal derivability in ZFC is a good model of every aspect of ordinary mathematical practice. It is, to begin with, only a model of provability, not of the discovery of proofs, let alone of the experience of mathematicians in discovering proofs. Amazingly, formalization has sometimes been criticized for "leaving out the experience of doing mathematics," which is, to use a stock simile, like criticizing a chemical analysis of a soup for leaving out the experience of tasting it. But for that matter, formal derivability in ZFC may not even give a good model of *presenting* proofs. For formal provability to be a good model of informal provability it is not necessary that formal proof should be a good model of informal proof.

Formalization may well — indeed surely does — involve an element of emendation in addition to exegesis. Formalization has two aspects, *symbolization*, representing everything in special signs rather than words and *codification* representing every notion as being defined and every result as being derived from a fixed initial list of primitives and postulates. The latter feature is present without the former in something like the Bourbaki encyclopdia. Symbolization is obviously a departure from ordinary practice, but what I want to emphasize is that codification is, too. Any codification of a large body of mathematics inevitably involves a certain unfaithfulness to the viewpoint of the mathematical community, at least insofar as it inevitably involves giving preference to one out of several options among which working mathematicians are indifferent.

A case in point was famously brought to philosophers' attention by Paul Benacerraf in his classic paper "What Numbers Could Not Be", which has since motivated the cluster of views in philosophy of mathematics known as "structuralism". I will be endorsing a kind of minimalist account of the Benacerraf phenomenon, which may still seem to some a form of structuralism, but may seem to others a form of anti-structuralism. If it is a form of structuralism, it is a very modest one, and I am certainly anti- the more ambitious forms found in the literature. These, I believe, represent generalizations of Benacerraf's observation *in the wrong direction*. Instead of assimilating the Benacerraf phenomenon to other cases in which codification involves making distinctions where mathematicians are indifferent, not all of which cases by any means involve questions about the existence or identity of objects, structuralism seeks to draw ontological lessons and apply them universally, to all mathematical objects, whether or not of one of the kinds that figure in Benacerraf-style examples.

But is structuralism an appropriate topic for a philosophy conference intended to focus on the objectivity of mathematical truth rather than the existence of mathematical objects? One might think so, since structuralism is often presented as an attempt to maintain mathematical objectivity while eliminating distinctive mathematical objects. But then again one might think not, since structuralism is also often presented as a theory about the distinctive nature of mathematical objects, the pleasantly paradoxical theory that their distinctive nature is to have no distinctive nature. Of the four book-length treatments of structuralism that have appeared in the last decades, two (by Geoffrey Hellman and Charles Chihara) are of the "eliminating objects" variety, and two (by Michael Resnik and Stewart Shapiro) of the "natureless objects" kind. Following Michael Dummett, the two contrasting views are often called "hard-headed" and "mystical", labels easier for those innocent of Latin to keep straight than the alternatives *in re* and *ante rem* (or *in rebus* and *ante res*). The proposal I will be endorsing is less ontological, less concerned with the status of mathematical objects, than *either* kind of structuralism in the literature.

II

Structuralists of both kinds typically follow Benacerraf in taking the case of the natural numbers and arithmetic as their premier example, but I would like to begin instead with the case of the real numbers and analysis. For in this case the advent of indifferentism among mathematicians is a roughly datable historical event, occurring around the middle of the century before last. The background is as follows.

If the ideal of rigor is perfectly realized in an area of mathematical practice, then all theorems in that area are logically deduced from postulates acknowledged in advance, and therefore will be equally true on any interpretation that makes the postulates true. But this fact does not preclude there being a specific intended interpretation, nor prevent intuitions about such an intended interpretation from playing an important continuing role in mathematical practice, even long after they have played their initial role of motivating the postulates. For they may continue to suggest conjectures that one might try to deduce from those postulates, and if such deduction turns out to be impossible, may perhaps then suggest further postulates. Where the ideal of rigor is less perfectly realized, intuitions about an intended interpretation will play an even larger role. In the seventeenth- and eighteenth-century analysis, where rigor was far from perfect, leading mathematicians had a fairly definite intended interpretation of what real numbers are.

On this interpretation real numbers were ratios of geometric or other magnitudes. Such an interpretation was not the only one current in the seventeenth or eighteenth centuries, as there were algebraists who, if one takes their formulations literally, held the untenable view that real numbers are *symbols* of some kind (see Pycior [1997] for an indication of the complexity of the situation). But it is the one held in the most exalted mathematical circles, the one expounded by Newton in his *Universal Arithmetick*, and assumed by Descartes in his *Géometrie*.

This intended interpretation makes real numbers into a species of abstract objects of the kind sometimes called equivalence types. Equivalence types are what things that are equivalent in some respect thereby have in common, as geometrical figures that are similar have in common their shape, or (Frege's favorite example) lines that are parallel have in common their direction. Cardinal numbers are what given things have in common with other things when there are just as many of the former as of the latter — a somewhat special case, since the equivalence predicate involved, equinumerosity, relates plural rather than singular arguments. Ratios are what this and that have in common with this other and that other when the first stands to the second as the third does to the fourth — again a special case, since the equivalence predicate involved, proportionality, relates arguments in pairs rather than singly.

Weierstraß and his successors could not accept the Cartesian or Newtonian understanding of real numbers as equivalence types of this special kind, because it ultimately makes analysis depend on Euclidean geometry. Nineteenth century mathematicians sought "arithmetize" analysis, which is to say, to "de-geometrize" it. Serving as the measure of ratios in Euclidean spaces would remain an important *application* of real numbers, joined by a similar application to non-Euclidean spaces; but the *foundation* of analysis would no longer lie in geometry. Cantor and Dedekind each sought to produce an "arithmetic continuum" to replace the "geometric continuum". They produced two different ones.

The feature of modern mathematical practice to which Benacerraf draws attention amounts in the case of analysis to just this, that though one student may first learn rigorous analysis using a textbook that introduces real numbers in some version Cantor's way, identifying them with equivalence classes of Cauchy sequences, and another using one that introduces them in some version of Dedekind's way, identifying them with cuts, after the students have seen the basic properties of the field of real numbers established, those of a complete ordered field, they can forget about the construction. For while in later work the complete ordered field properties will be used again and again, the identification with sets of sequences or with pairs of sets will never come up. Two analysts who wish to collaborate do not need to check whether they were taught the same definition of "real number", as two algebraists do need to check whether they are working with the same definition of "ring" (which for some does and for others does not include the existence of a multipicative identity).

If we ask what is the square root of two, all can agree with a characterization in terms of the role of that number in the field of real numbers: the unique positive element whose product with itself is equal to the sum of the multiplicative identity with itself. On a seventeenth- or eighteenth-century conception this answer, though correct as far as it goes, is less informative than the answer that the square root of two is the ratio of the diagonal of a square to its side, since it does not tell us what the square root of two *is*. Contemporary working mathematicians, unlike those of two or three centuries ago, are indifferent to this question of the identity of the number. While a textbook may offer a definition of the real field as composed of sets of sequences or as composed of pairs of sets, and on the basis of this definition prove the theorem that the real field is a complete ordered field, only the theorem and not the definition has the status of being generally accepted by the mathematical community. Such is the feature of mathematical practice from "structuralist" philosophical theorizing starts: *An identifying definition is used to prove a theorem, but only the theorem and not the definition plays any subsequent role*. Let me call this feature *indifference to identification*.

III

Structuralism in the literature very quickly moves from the observed phenomenon of indifference to identification to the ontological conclusion that real numbers are not objects of any ordinary sort: not concrete and not equivalence types either. Disagreement among structuralist philosophical theorists over ontological issues then very quickly begins: Is it that there are no such objects as real numbers at all (the hard-headed view), or is it that real numbers are objects of some extraordinary sort (the mystical view)? Disagreement begins so very quickly, indeed, that it is difficult to say what, beyond a few ambiguous slogans, hard-headed structuralism and mystical structuralism have in common to justify considering them two species of the same genus. Let us postpone ontological questions by transposing the question from the material to the formal mode. Instead of asking what the real field is, let us ask what the phrase "the real field" denotes — or more cautiously, so as not to presuppose that it is a denoting expression, how the phrase "the real field" functions in mathematical language.

The interpretive hypothesis I would like to endorse is one that was arrived at before me by Richard Pettigrew, building on work of Stewart Shapiro. The story, so far as I am involved in it, began some years ago when I was asked to review one of Shapiro's books, and I noticed a curious thing about it. He wanted to illustrate the notion of "position in a structure" by an intuitive example, and had the idea of compairing a position in a number system to a position on a sports team. But the sport he chose for purposes of illustration was baseball, which struck me as odd, since his publisher was British and his intended audience international, presumably containing many readers who know no more of baseball than I know of cricket. And then I realized what is peculiar about baseball as opposed to other team sports I knew of, namely, that there are no symmetric left/right pairs of positions. And then I noticed that the lack of symmetries was also a feature of his mathematical examples, too. He discussed examples like the position of $\sqrt{2}$ among the real numbers, but not the position of *i* among the complex numbers. I mentioned this point in passing in my review as an observation, and independently Jukka Keränen developed it at length as an objection.

Fairly recently Shapiro undertook to address the question what *i* denotes, and his answer was nearly enough the following. The letter *i* is used like those letters that are introduced in natural deduction systems when one applies the rule of *existential instantiation*. Having assumed or deduced from other assumptions the result $\exists xFx$, one says "Let a be such that Fa" where a is a previously unused letter, and then goes on to deduce various further conclusions. The crucial property of such letters is that whenever one deduces a conclusion Ga one can go on to deduce the universally quantified conditional $\forall x(Fx \rightarrow Gx)$. Beyond this crucial feature there is some disagreement in philosophy of logic as to just how such letters function, and even as to what they should be called. Shapiro calls them "parameters"; I used to call them "quasi-constants", but for uniformity will adopt Shapiro's terminology. Terminology aside, Shapiro's hypothesis was that i was like what he calls a parameter, except that instead of being used just in the course of one proof, it is used throughout subsequent mathematics. In the complex number field there are two square roots of negative one, and mathematicians in effect say "Let i be one of them." There's no asking *which*.

Pettigrew, having as I understand been in the audience when Shapiro was speaking on these matters, observed that if we can take i, the symbol for the imaginary unit, to be a permanent parameter in this sense, as Shapiro does, we could just as well take **R**, the symbol for the real number system, to be such a permanent parameter, too. What Cantor, Dedekind, and the different textbooks in their different ways in effect do is to deduce that there exists a complete ordered field. What mathematicians then in effect do is to say, "Let the real field be a complete ordered field," in a move that, like any move where one says, "Let *the B* be *an A*," would best be construed as an application of existential instantiation. This hypothesis accounts for the phenomenon of indifference to identification, the main motivation for structuralism as it appears in the literature. For by the crucial property of parameters, one would not be justified in saying "The elements of the real field are sets of sequences" unless the elements of *every* complete ordered field were sets of sequences, which is not the case; and similarly for pairs of sets. The hypothesis makes such a question as "Are real numbers sets of sequences or pairs of sets?" resemble the question attributed to Prior, "Let Bossie be one of the cows in farmer Brown's barn. How much does Bossie weigh?"

Shapiro's and Pettrigrew's work has recently appeared in *Philosophia Mathematica*. Before that work came out I had arrived at a position that, except for terminology, was quite close to the one just described. I wrote up my thoughts in a draft of what has become this talk, at a time when this conference was still expected to be held in fall 2008, and sent the draft to Shapiro. His response brought his own related work and Pettigrew's to my attention. Since that work is now readily available, I need not offer any further exposition of my own of the basics of the view I would like to endorse, which I would sum up this way: *In cases of indifference to identification, the key expression involved is functioning as a parameter, only one that is not just used for the space of one proof, but throughout subsequent mathematics*. Let me call this the *permanent parameter hypothesis*.

IV

The permanent parameter hypothesis is a kind of minimal account of what is going on in Benacerraf-type cases. Structuralism as it appears in the literature develops and generalizes the account in one direction, arriving at a thesis about the nature of mathematical objects in general. I would generalize in a different direction, citing the permanent parameter phenomenon as just one instance of a more general phenomenon of the indifference of working mathematicians to certain kinds of decisions that have to be made in any codification of mathematics.

To start with the associated phenomena closest to Benacerraf-type cases, with existential instantiation we take an existential statement $\exists xFx$ and "give a name to such an x", thus arriving at Fa. But it is also common in mathematics to take an existential-universal statement $\forall y\exists xRyx$ and "give a name to such an x", thus arriving at $\forall yRyf(y)$. This step — *Skolemization* — is one logicians have given some attention. The point I want to make about it is that in addition to Benacerraf-type cases, where what superficially appear to be references to a definite, specific structure such as the real numbers turn out to be cases where mathematicians are doing something like existentially instantiating, there are also cases where what superficially appear to be references to the products of a definite general construction turn out to be cases where mathematicians are doing something like Skolemizing.

For instance, having proved that every field has a minimal algebraically closed extension, unique up to an isomorphism fixing the given field, but by no means literally unique, they will proceed to speak of "the" algebraic closure of a field. Similarly they speak of "the" field of quotients of an integral domain, and so on. In this connection I should also note that though I said earlier that textbook constructions along Cantor's or Dedekind's lines are in effect existence proofs that can be forgotten once the existence of a suitable field has been established, this is not quite accurate. For one often has to recall earlier proofs if one wants to *generalize* an earlier result, and the Cantor and Dedekind constructions do both generalize. In their general forms they are known as *completion of an order* and *completion with respect to a metric*. These also are general constructions, and I believe the analogue of the permanent parameter hypothesis — one might call it the permanent Skolemization hypothesis — applies to them as well as to the algebraic closure and field-of-quotients constructions.

Closely related to Skolemizing is the application of second-order versions or variants of existential instantiation. Weiner and Kuratowski can be viewed as proving, each in his own way, that there exists an operation f such that for any sets u, v, x, y, we have $f(u, v) = f(x, y)$ if and only if $u = x$ and $v = y$. The mathematical community can then be viewed as "giving a name" to such an f, namely, the name "ordered pair" symbolized by brackets. There is then no asking whether $\langle x, y\rangle$ is the

Weiner-set or the Kuratowski-set. The indifference of working mathematicians to the choice between Kuratowski and Weiner is of a piece with their indifference to the choice between Cantor and Dedekind.

To give another kind of example of indifference, I recall my thesis adviser Jack Silver, back when I was a student, mentioning cases of the following kind. What is a topological group? Is it an ordered triple consisting of a set, a group operation on it, and a topology on it, or an ordered triple consisting of a set, a topology on it, and a group operation on it? I suppose a group-theorist might think of it in the former way (it's a group, and then you add a topology) and a topologist the other way (its a topological space, and then you add a group operation), but the mathematical community as a whole surely has no preference, which means that neither choice is "right" in the sense of being faithful to a pre-existing understanding.

To give yet another kind of example of indifference, is the rational field a subfield of the real field? If we think of the symbol **R** as introduced by what amounts to an application of existential instantiation to an existence theorem, is the theorem simply that there exists a complete ordered field (as I have so far been tacitly assuming for illustrative purposes), or is it that there exists a complete ordered field *extending the rational field*? Mathematicians perhaps most often speak as if the rationals were a subset of the reals, but in some contexts, as when working with symbolic computation programs, the natural number 2, the rational number 2/1, the real number 2.000…, and so on do get treated as distinct items.

Someone might, I suppose, suggest that the rational and real number two are always in principle distinguished in thought, though they are not usually in practice distinguished in notation, since nothing is more common in mathematics than so-called abuse of language, using the same notation for two different things. But then again someone might suggest instead that they are always thought of as the same, and that the use of different notations with symbolic computation programs resembles the situation in set theory, where one writes $\aleph_0 + \aleph_0 \neq \omega + \omega$ even though $\aleph_0 = \omega$, because, though the logical thing to do would be to put subscripts on the plus sign to distinguish cardinal from ordinal addition, the conventional thing to do is use aleph notation when cardinal addition is meant and omega notation when ordinal addition is meant. But it is far more likely, I think, that there simply is not any single answer to the question of the relation between the rationals and the reals consistently maintained across the whole of mathematical practice.

Indifferentism is not limited to questions about the identity of objects, but can be seen also in connection with questions about the definitions of concepts. As a simple illustration, consider the various characterizations of the exponential function: in terms of its power series, as the inverse of the logarithm function, and so on. Any codification of mathematical analysis inevitably must choose one of these characterizations and make it the very definition of the function, while the others then become merely theorems about it; whereas the mathematical community is indifferent as to which characterization is considered to have definitional status. One textbook may make the power series the definition, and the relation to logarithms a theorem, while another may do the reverse, and both approaches will be considered all right, and neither *uniquely* right, by working mathematicians.

In this example we easily see why the choice of definition that the codifier must make will in the end make no difference to what is provable. The reason is simply that the only candidates for the status of definitions are characterizations that are theorems on any candidate definition, and moreover that if taken as definition would in turn yield the other characterizations as theorems. With other kinds of indifference the explanation why it makes no difference to provability will have to proceed along different lines. The development of a general account of mathematical indifference and why it makes no difference to provability remains a task for the future.

V

Structuralism as found in the literature develops the basic insight of the permanent parameter hypothesis in a different direction, one that does not emphasize the affinities of Benacerraf-type cases with other cases of indifference, whether it is a matter of indifference to the identities of objects or to something else, but rather seeks first to draw ontological conclusions about objects such as real numbers that figure in Benacerraf-type cases, and second to generalize these conclusions to all mathematical objects. Now even to take the first step, to draw any ontological conclusions at all, one needs to go beyond the bare permanent parameter hypothesis by adding to it some ontological interpretation of the role of parameters. And here, as Shapiro has noted, there are different possibilities, leading to different structuralisms.

On one account, the parameter *a* that is introduced by applying existential instantiation to the theorem that an *F* exists, though it looks like a constant that should have a specific *F* as denotation, is really a free variable that may have any *F* as value. What looks like a closed sentence *Ga* that should have a specific truth value is really an open sentence that may be true for some values of the variable and false for others; though indeed if we are able to *deduce Ga*, we will then have truth for *all* values of the variable, and all *Fs* will be *Gs*, as per the crucial property of parameters.

(It may be added parenthetically that the free-variable account has a variant, the schematic-letter account, according to which the parameter is not a variable ranging over *F*s, but a schematic letter for which may be substituted terms denoting *F*s. This variant may be attractive in the second-order case for those with certain ontological qualms. For instance, the proof of the basic law of ordered pairs may be construed as a proof of an existential theorem in an NBG-like system, to the effect that there exists a three-place relation with certain properties, or alternatively as a proof of an existential metatheorem about ZFC-like system, to the effect that there exists a three-place formula with certain properties. Then the ordered-pair predicate will be on one construal a free variable having relations of the appropriate kind as values, and on the other construal a schematic letter having formulas of the appropriate kind as substituends.)

On another account, the parameter *a* introduced by applying existential instantiation is like an ordinary constant in denoting a specific *F*, albeit unlike an ordinary constant in that the specific *F* it denotes is an extraordinary one, the *arbitrary F*, distinguished from other *Fs* by having the metaproperty of having no properties not shared by all *Fs*. (For instance, the arbitrary triangle is neither acute nor right nor obtuse, neither equilateral nor isoceles nor scalene.) It is this metaproperty of the arbitrary *F* that explains the crucial property of parameters. (For instance, if the angle sum in the arbitrary triangle is two right angles, then the angle sum in any triangle is two right angles.) The view may sound fantastic, but Kit Fine, in his *Reasoning about Arbitrary Objects*, in effect defends taking "the arbitrary *F*" to be a genuine denoting expression, and not an expression like "the average *F*", that is meaningful in the context of various sentences, but does not denote a specific object. Fine, however, offers a set-theoretic gloss on what arbitrary objects are that no structuralist would welcome. It is something like Fine's theory *without* his gloss that is pertinent in the present context.

Yet another account would be that a parameter, like a Hilbert epsilon-term $\varepsilon x[Fx]$, denotes some specific but unknown object. It is lack of knowledge that explains the crucial property of parameters, that we are in a position to assert that *a* is a *G* only when we are in a position to assert that all *Fs* are *Gs*, since for all we know *a* might be *any F*. It is easy to imagine a mathematical community for whose practice this theory, at least as applied per the permanent parameter hypothesis to the case of analysis, would be appropriate. For one need only imagine a community of mathematicians who defer to a few specialists among them on foundational questions. The specialists may have decided that Cantor's construction is superior to Dedekind's (or vice versa) for some technical reason mattering only to specialists, and may for this reason or purely as a convention have declared the Cantor definition (or the Dedekind definition, as the case may be) the official definition of the real field. Ordinary mathematicians in this community would intend "the real field" to denote whatever the specialists officially take it to denote, though like Hilary Putnam with elm trees and beech trees, they would not actually know what that *is*.

The permanent parameter hypothesis, combined with the first interpretation of parameters, as free variables, leads to something like hard-headed structuralism, according to which statements apparently about "the" real numbers are really generalizations about *all* complete ordered fields. The permanent parameter hypothesis, combined with the second intepretation of parameters, as denoting an object of a given kind that is distinguished from other objects of that kind by having the metaproperty of having no distinguishing properties, leads to something like mystical structuralism, according to which the real number system is *nothing but* a complete ordered field, and the square root of two *nothing but* the unique positive element in the real number system whose product with itself is equal to the sum of the multiplicative identity with itself. The permanent parameter hypothesis combined with the third intepretation of parameters leads to a kind of epistemicist structuralism that has not, so far as I know, been advocated in the literature, though Shapiro has adumbrated it without advocating it.

VI

So structuralism takes a first step beyond the permanent parameter hypothesis just by drawing any definite ontological conclusions at all. It also seems to take a second step, not just beyond the permanent parameter hypothesis but beyond the combination of that hypothesis with an ontological hypothesis about parameters, when it comes forward as universal theses about *all* mathematical objects. For the permanent parameter hypothesis as an interpretive hypothesis extends only as far as the phenomenon it interprets extends. It is to apply in all cases where there is genuine indifference to identification, but it is not clear that such indifference is universal, and hence not clear that the permanent parameter hypothesis applies universally, as structuralism aspires to do.

One general construction where it is not clear that there is indifference to identity is that of the *Cartesian product*. As used by mainstream mathematicians, "the Cartesian product of the sets *X* and *Y*" seems to denote something more or less definite, the set of ordered pairs of elements of *X* with elements of *Y*. I say "more or less" definite to leave room for indefiniteness as to the identity of ordered pairs; but I see no further indefiniteness beyond that. This is in contrast to the "the algebraic closure of a a field *F*". It is also in contrast to "the Cartesian product of *X* and *Y*" as understood by category theorists, whose usage does perhaps invite application of the permanent parameter hypothesis. Perhaps category theory is the wave of the future, and mainstream mathematicians will come to follow the usage of its adepts; but I do not see them doing so at present.

One specific structure where it is not clear that there is indifference to identity is ironically that of the *natural numbers*, which structuralists often cite this case as a paradigm of indifference to identity. Many contemporary mathematicians do indeed profess to be interested in the natural numbers only as another algebraic system, on a par with the integers, the rationals, the reals, and so on. Mathematicians do often use the numeral "two" for "the next to next to initial element in the sequence of natural numbers" where "the natural numbers" is being used like a parameter. But may they not sometimes slip into using it for a certain equivalence type, namely, the cardinal number given things have if there is a thing among them and another and no more? One may suspect them of slipping into using the numeral in this second way when they address questions about how many factors a given number has, or (Frege's favorite example) how many solutions a given equation has. And even if they are always using the numeral in the first way when engaged in research in pure mathematics (as it would certainly be possible in principle for them to do), are they still doing so when balancing their checkbooks or figuring their income tax?

VII

We may leave that question undecided. For the crucial case is that of set theory. Here it seems the permanent parameter hypothesis simply cannot apply, if one takes the analogy with parameters introduced by existential instantiation seriously. For if one takes the analogy seriously, the hypothesis cannot apply to the ultimate background theory that provides the framework — or more pretentiously, the foundation — for other mathematical theories. This is because parameters are not present in primitive notation, being rather introduced only by instantiating existential theorems or axioms. So the primitive expressions of the ultimate background theory that supplies the axioms from which to deduce existential theorems cannot be regarded as parameters.

Surely it is set theory that appears to function today as the ultimate framework or "foundation" for contemporary mathematics; and if so, then the permanent parameter hypothesis does not apply to set theory. Certainly the Benacerraf phenomenon that *motivates* the permanent parameter hypothesis is not visible in set theory. Mathematicians do consider different constructions of real numbers out of more basic entities and display indifference between them, but it cannot be said that they consider different constructions of sets out of more basic entities and display indifference between them. For there are no such constructions to be considered since there are no more basic entities than sets in contemporary mathematics.

The extension of structuralism to set theory creates well-known internal difficulties for that doctrine. Consider first the hardheaded variety. On this view, there are no such things as real numbers, statements about them being disguised generalizations about complete ordered fields. But if there are no such things as sets, either, where are the complete ordered fields to come from? And indeed, how is the property of completeness to be formulated? This last is a problem for the mystical variety of structuralism, too. The mystical structuralist wants to say that the natural numbers are a progression. But the notion of progression as usually explained includes the feature that subset of the domain that contains the initial element and the successor of any element it contains contains all elements. What sense can bemade of that feature if sets, too, are just positions in a certain structure?

These problems, as I said, are well known, and different structuralists have elaborated different purported solutions. Another problem, also not unfamiliar, seems to me in the end more serious. Structuralists, hard-headed and mystical alike, typically want to treat set theory and the universe **V** on a par with analysis and the real field **R** or arithmetic and the natural-number system **N**. But how is this to be done, since there is no categorical set of axioms for it, comparable to the complete ordered field axioms for analysis, or the (second-order) Peano postulates for arithmetic? Typically something like second-order ZFC is pressed into service by the structuralist as a counterpart. Set theory is then construed either as the study of all models of such a theory (the hard-headed option) or the study of a special model whose distinctive metaproperty is to have no distinctive properties (the mystical option). But if that is how set theory is conceived, then there seems to be no room for the activity, important to many set theorists, of going back to an intuitive notion of set motivating the axioms in order to motivate more axioms to settle questions not settled by the existing axioms. Structuralism here ties set theory to a particular axiom system in a way that seems to *block the road of inquiry*. So much the worse for structuralism, I would say.

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