Language, Logic and Concepts
Essays in Memory of John Macnamara
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Chapter 3

On Structuralism in Mathematics

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For some years, I have been pursuing a program I call the structuralist foundation of mathematics (SFM) (Makkai 1996, 1997, 1998, to appear). SFM is based on category theory, a branch of mathematics founded by Samuel Eilenberg and Saunders Mac Lane about fifty years ago (Eilenberg and Mac Lane 1945). William Lawvere's work (e.g., Lawvere 1969) has made category theory the basis of a new foundational approach to mathematics and logic. Lawvere's categorical logic (e.g., Lawvere 1970) and Lawvere's and Myles Tierney's topos theory (Lawvere 1971), subjects that have been extensively developed, are integral parts of SFM.

In relation to other approaches to foundations within category theory, the distinguishing features of SFM are, first, that its aims are global, encompassing the totality of mathematics, and, second, that it is committed to a total linguistic articulation in the spirit of Gottlob Frege. Today, we express the latter commitment by saying that a foundational proposal has to be presented in the form of a formal theory. At the same time, SFM intends to articulate, intuitively and philosophically, a universe of

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John Macnamara was, for many years, a constant source of inspiration and encouragement for me. His interests and insights in philosophy have influenced my thinking profoundly. This chapter is dedicated to his memory.

discourse behind the formal theory. It turns out that the latter articulation involves a fairly novel mathematical metatheory.

There is a structuralist philosophy of mathematics behind the program of SFM. In this chapter, my aim is to make an initial contribution to the statement of this philosophy. This structuralism, it seems to me, is quite different from the structuralism found in the literature of the philosophy of mathematics. The basis of this difference is, of course, the almost total lack of attention paid by the philosophical literature to category theory. Charles Parsons has argued, somewhat tentatively, that structuralism may be used as a philosophical justification of the classical Cantorian iterative set theory (Parsons 1990). The structuralism I have in mind involves a radically different "set theory," a new conception of mathematical totalities, the formal explication of which involves concepts of category theory, (some of which—higher dimensional categories—are being developed at present). I see mathematical structuralism as *identical to a specific conception concerning mathematical totalities*—not something that can exist independently of any such conception and can then be applied to deal with already given such conceptions. According to a pervasive complaint (see Parsons 1990) about structuralism, it cannot be an independent foundation, since it relies on the concept of structure, which, in the final analysis, is a set-theoretical concept. This complaint also assumes that the nature of set theory is given before we can discuss what structuralism is.

In this chapter, I will argue for the desirability of a comprehensive language of mathematics in which *only* structural properties of mathematical objects can be expressed. The discussion intends to lead to the conclusion that "structural properties" are identical to "mathematically meaningful properties." Paul Benaceraff's classic paper, "What Numbers Could Not Be" (Benaceraff 1965), is in fact a sustained argument to the same end: it is a plea for a language in which one talks about numbers *only as* numbers. Section 3.1 of the present chapter is a commentary on the first two sections of Benaceraff's paper. What separates my views from Benaceraff's is that I believe that the articulation of the desired structural language is possible. As a matter of fact, what Benaceraff says in his third section entitled "Way Out," especially the subsection "Identity," points importantly to what I consider to be the solution (compare the references to my work). However, in his subsequent paper "Mathematical Truth" (Benaceraff 1973), Benaceraff seems to have lost faith in a structuralist program, at least in the radical form I am advocating. In section 3.3 of the present chapter, I will try to show that the "standard semantical picture of the world," to which Benacerraff is (essentially) committed in his 1973 paper, is in fact fictitious. This conclusion will involve the thesis that the nature of mathematical objects is not fundamentally different from the nature of myriad nonmathematical everyday abstract objects. The reality of abstract objects is grounded in a *structural* language that, although referring ostensibly to concrete, representing carriers of the abstractions, manages to unambiguously refer to the abstract objects, by the discipline of staying within the framework of concepts that are meaningful for the abstract objects *as* abstract objects. In this case, *reality is grounded in language*.

Makkai 1998 is a semitechnical introduction to the mathematical work; it also contributes to explaining the philosophy of structuralism as I understand this term. However, there is much more to be said about this subject, to which I hope to return another time.

3.1 Reality through Language

In his famous 1965 paper, "What Numbers Could Not Be," Paul Benacerraff describes two children, Ernie and Johnny, "sons of militant logicists," who, before learning about numbers, how to count, and so on, had been instructed in set theory in the standard Zermelo-Fraenkel formulation. After that instruction, the parents "needed only to point out what aspect or part of what the children already knew, under other names, was what ordinary people called numbers" (p. 48). Benacerraff goes on to describe *how* this pointing out took place in the case of Ernie; but he temporarily and cleverly conceals *what* actually took place in the process. The story *looks like* the familiar one, the one of defining the natural numbers as the finite von Neumann ordinals, and defining zero and the successor operation in the familiar ways—the one we teach nowadays in set theory courses. As Benacerraff puts it:

> To recapitulate: It was necessary [for a proper education in arithmetic] (1) to give definitions of "1", "number", and "successor", and "+", "×", and so forth, on the basis of which the laws of arithmetic could be derived; (2) explain the "extra-mathematical" uses of numbers, the principal one being counting—thereby introducing the concept of *cardinality* and cardinal number. I trust that both were done satisfactorily, that the preceding [description of Ernie's education] contains all the elements of a correct account, albeit somewhat incompletely. (p. 54)

Then we learn that Johnny underwent a similar education.

Delighted with what they had learned, they started proving theorems about numbers. Comparing notes, they soon became aware that something was wrong, for a dispute immediately ensued about whether or not 3 belonged to 17. Ernie said that it did, Johnny that it did not. Attempts to settle this by asking ordinary folk (who had been dealing with numbers *as* numbers for a long time) understandably brought only blank stares. (p. 54)

At this point in the story, we learn *what* exactly happened in the respective educations. Indeed, as we thought, Ernie was taught that the natural numbers are (Johnny) von Neumann ordinals. Johnny, on the other hand, was told that they are the (Ernie) Zermelo numerals; that is, zero is the empty set, and the successor of n is $\{n\}$, the singleton whose unique element is n.

After reflecting on the dilemma, Benaceraff arrives at the conclusion that

3 and its fellow numbers could not be sets at all. (p. 62)

Along the way, he says:

But if, as I think we agreed, the account of the previous section [in which the set-theoretic reconstruction of the concept of number was related, albeit with *what* the numbers and their operations actually were left unspecified] was correct—not only as far as it went but correct in that it contained conditions which were both necessary and *sufficient* for any correct account of the phenomena under discussion, then the fact that they disagree which particular sets the numbers are is fatal to the view that each number is some particular set. (pp. 55, 56)

Later:

Furthermore, in Fregean terminology, each [of the two] account[s] fixes the *sense* of the words whose analysis it provides. Each account must also, therefore, fix the reference of these expressions. Yet, as we have seen, one way in which these accounts differ is in the referents assigned to the terms under analysis. (p. 56)

And:

Therefore, exactly one is correct, or none is. But then the correct one must be the one that picks out which set of sets is *in fact* the numbers. We are faced with a crucial problem: if there exists such a "correct" account, do there also exist arguments which will show it to be the correct one? (p. 57)

Casting his net wider, he then probes the Fregean account according to which numbers are predicates or classes, and finds it wanting, for expected reasons related to the paradoxes, but also because there is *no grammatical evidence that seventeen is a predicate of classes* ("has seventeen members" is obviously such a predicate, but that is something else than to say that seventeen is such a predicate). He then summarizes:

If numbers are sets, then they must be *particular sets*, for each set is some particular set. But if the number 3 is really one set rather than another, it must be possible to give some cogent reason for thinking so; for the position that this is an unknowable truth is hardly tenable.... There is no way connected with the reference of number words which will allow us to choose among them, *for the accounts differ at places where there is no connection whatever between features of the accounts and our uses of the words in question.... [A]ny feature of an account that identifies 3 with a set is a superfluous one*, and therefore, 3 and its fellow numbers could not be sets at all. (p. 62)

Benacerraff emphasizes that for the purposes of *explication*, one may very well wish to temporarily *identify* numbers with particular sets and show that we can do with the sets what we now do with numbers. However, as he says:

It is ... obvious that to discover that a system of objects will do cannot be to discover which objects the numbers are. (p. 68)

Benacerraff mentions a result due to G. Takeuti according to which set theory is in a strong sense reducible to the theory of ordinal numbers.

No wonder numbers are sets; sets are really (ordinal) numbers, after all. *But now, which is really which?* (p. 68)

What are we to make of Benacerraff's argument?

The first reaction may be to say that the argument invalidates the usual set-theoretic treatment of numbers. I want to argue that this is not so—that in fact, the set-theoretic Platonist engaged in the foundations of mathematics and Benacerraff can agree without compromising their positions in any essential way. A perfectly satisfactory position the standard Platonist may adopt seems to be that, indeed, set theory is incapable of telling us *what* the numbers are; but in fact, it does not aim at doing that in the first place. What set theory wants to do, and does, is explain *what we mean when we talk about numbers*. It gives us a systematic way of translating "ordinary" mathematical statements involving numbers into set-theoretic statements, and it provides reliable methods for proving and refuting such statements. Indeed, and this is an important additional fact contributing to the explanatory power of set theory, it does its work in a remarkably simple particular manner, by *identifying* numbers and other mathematical objects as certain sets, such that the original statement—say, about numbers—is translated into its set-theoretical version by simply replacing reference to the mathematician's numbers by reference to their set-theoretical surrogates. But note, set theory does this without creating any prejudice concerning the issue of *what the numbers really are*, or, for that matter, *whether they exist at all*! Further, standard set theory can do more than that: it can give a *theory* of *what* constitutes a satisfactory surrogate for the notion of number. This exemplifies a very important point about a good foundation: namely, the requirement that it should have the tools to reflect about itself to the largest extent possible. How set theory does this in relation to the concept of natural number will be discussed below, although the facts are familiar; I have in mind the (second-order) Peano axioms for the natural numbers, and their categoricity.

I conclude that from the set-theoretic point of view, Benaceraff's argument is not a problem. In fact, it may be interpreted as a support of the standard foundations, arguing that set theory is needed precisely to provide the right (because *true*) *paraphrase* of the pretheoretical language concerning numbers, and other dubious entities like them, a paraphrase that ensures the correctness of the language globally, without committing its user to any ontology—in fact, freeing the user from all commitment to any ontology!—concerning the "ordinary mathematical objects" such as numbers. Of course, it does this at the obvious price of exacting the ontological commitment to *sets themselves*; but who is unwilling to submit to such a discipline when the resulting advantages are as great as they in fact are? The set theorist treats numbers as second-class existents, the first-class existents being the sets; numbers are the results of conventions, which, by their being conventions, are arbitrary.

Incidentally, I find that the response to "Benaceraff's challenge" in Penelope Maddy's book *Realism in Mathematics* (Maddy 1992), pressing as it does the interpretation that numbers are properties (see p. 86), is on the wrong track. If set theory does anything well, then that is the job of interpreting mathematical entities as *objects* of the theory, that is, *sets*. Interpreting entities as *metatheoretical* ones such as *properties* is inherently inferior to interpretation-as-objects. Also, I do not think that Maddy is answering Benaceraff's arguments against precisely this move.

What, then, is the force of Benaceraff's argument?

As I said, the set theorist and Benaceraff may agree on the status of numbers to the effect that numbers as first-class objects either do not exist at all, or they do, and they are not sets; in short, they may agree on Benaceraff's main conclusion that numbers are not sets. But it is equally clear that they do not agree with respect to their overall positions. The difference is Benacerraff's implicit commitment to the thesis that *there must be an account of numbers that treats them as first-class entities, that treats numbers as numbers.*

Admittedly, this thesis is to a large extent normative; it may be ignored by those who do not care for the underlying imperative. The set theorist may just say, "Go ahead, try to find your precious account of numbers, but I think you are wasting your time; I personally do not care a bit for it, even if it can be found, because my account of number-talk is perfectly sufficient." Is there anything more compelling here than a possibly interesting, but ultimately unnecessary, philosophical suggestion? I believe the answer is yes; it is the reality of the language of mathematics as we find it in practice.

There is a conflict between the language of mathematical practice (the coarse talk) and the language of set theory as it applies to numbers (the refined talk). Interestingly enough, this conflict is not that set theory is unable to express things that the practitioners want to, and can, express, which would be the natural thing to expect in a relationship between an existing practice and an a posteriori foundational reconstruction of that practice. The conflict is of an opposite nature: the practitioners, after acknowledging that the refined talk handles nicely what the coarse talk did maybe not so nicely, complain that the refined talk also introduces nonsense, phrases like "3 belongs to 17," "3 equals the set whose elements are the empty set, the singleton of the empty set, and the set whose elements are the latter two sets," and the like, which clutters up the language. The practitioner immediately recognizes what is and what is not meaningful talk about numbers; there cannot be any doubt about the objective reality of the distinction. And the practitioner demands, or at least desires, that a foundation be provided that observes this distinction.

Benacerraff does not say what I just said; maybe he finds it unnecessary and/or unjustifiable to say it. But I find places in his narrative where he touches the ground of mathematical practice. In one of the quotations above, the ordinary folk are said to be dealing with numbers *as* numbers. This place in Benacerraff's text indicates the acceptance of the practice of mathematics, the locus of numbers *as such*, to be the final arbiter concerning the issue when a proposed language of mathematics is right. The little word "*as*" is the Archimedean fixed point on which the argument pivots. Another of the quotations contains the reference to "our uses of the [number] words"; these are clearly the established uses in the practice that excludes the "nonsense" I mentioned above. I feel that these instances of reliance on mathematical practice are the source of the strength of Benaceraff's argument.

It should be clear by now that I want to turn the issue at hand into one concerning *language*, from the ostensible one that is about *ontology*. True; I think the issue is not *what* numbers are, but *what the nature of numbers is*, nature that can only be revealed in the *language* of the practice dealing with numbers. The question "What are numbers?" is inherently defective. It accepts only a certain kind of answer, one that may be impossible to provide; it presumes that we can point out a domain of things that are already familiar and can proceed to narrow down that domain to arrive at the precise domain of the numbers. The question "What are numbers?" has a reductionist bias. We will not likely get to know the numbers by coming to see that they are just like some other things that we already know; it is quite possible that they are unique in their essential nature. What that nature is, is the good question. And, we then must realize, that nature is to be found in the only tangible reality that contains the numbers, the language of the practice dealing with numbers.

There is natural language as it is found in practice; and then there is formal language that is the result of philosophical reflection on natural language and of a subsequent deliberate act of articulation.

Our problem, then, is this: is it possible to articulate a language of mathematics in which we talk about numbers as numbers?

3.2 Structures

Let us get closer to the way mathematicians talk about, and use, numbers; in this instance, by "number" I mean "natural number" (any of 0, 1, 2, 3, etc.). I will invoke the "Ideal Mathematician" (I. M.), a creation of Philip J. Davis and Reuben Hersh (1980) (a delightful concept indeed!); I will construct the "ideal belief" (not necessarily the *best* belief; see Davis and Hersh 1980), I. M.'s belief, concerning numbers. I. M. says:

I. M. All I need is a system (set) N of entities called henceforth the (*natural*) *numbers*, with a distinguished element called *zero* (0), a distinguished unary operation giving the *successor* $S(n)$ of any number n, such that 0 is not the successor of anything, S is a one-to-one function, and the Principle of Mathematical Induction is valid: given any property $P(\ )$ of numbers (equivalently, any subset P of N), if $P(0)$ holds ($0 \in P$), and $\forall n \in N.\ (P(n) \rightarrow P(S(n)))$, then all numbers have the property P ($\forall n \in N.\ P(n)$).

I. M. is familiar with logical notation. Our undergraduate students make mistakes with the quantifiers, but they know them! Frege's reform of the language of mathematics has percolated thoroughly through everyday usage in mathematics. To continue the imagined quotation from I. M.:

I. M. There is an axiom of set theory asserting that such a system exists. Given such a system, I can do everything you have ever thought of or will ever think of doing with numbers. One such thing is defining by recursion a function one (or more) of whose variables is ranging over numbers. I mentally fix such a system, and will always refer to this one when I think of numbers. That's all there is to natural numbers.

Above, I. M. described a more or less typical *kind of structure*; in this case, an entity of the kind, a structure of the given kind, is a set with a distinguished element and a distinguished unary operation, satisfying certain definite conditions. I. M. claims that all he needs is a *Peano system*, as the structures of the described kind are called (although it was Richard Dedekind who invented them).

Let me say something that may be partly new for I. M. The definition of "Peano system" may be given equivalently in the following way. $(N, 0, S)$, where N is a set, $0 \in N$, and $S : N \to N$ ($f : A \to B$ means that f is a function with domain equal to the set A, and range contained in the set B; I. M. is familiar with this notation), is a Peano system if and only if a certain condition holds. Before I give it, for the uniformity of notation, instead of $0 \in N$, I will write $0 : t \to N$. Here t is any fixed one-element set, (say, $t = \{\varnothing\}$, but it does not have to be that); note that a mapping from t to any set A is really the same as an element of that set, the element which is the value at the unique element of t. The condition for $(N, 0, S)$ to be a Peano system is this: given any $(A, a : t \to A, f : A \to A)$ (thus, so far, we have something that is "like" our system $(N, 0, S)$),

there is a unique function $g : N \to A$ such that the diagram

$$\begin{array}{ccccc} t & \xrightarrow{\;0\;} & N & \xrightarrow{\;S\;} & N \\ {\scriptstyle i}\Big\Vert & \circ & \Big\downarrow{\scriptstyle g} & \circ & \Big\downarrow{\scriptstyle g} \\ t & \xrightarrow[a]{} & A & \xrightarrow[f]{} & A \end{array}$$

commutes.

For example, when we first apply S, and then g (on the right), the effect is the same as when we first apply g, and then f (this is the commutativity of the right-hand square); also, the left vertical i denotes the one possible function from t to t, the identity function. When we rewrite this in "algebraic notation," we get this:

$g(0) = a$ (here, I revert to $0 \in N$, $a \in A$ from $0 : t \to N$, $a : t \to A$)
$g(S(n)) = f(g(n))$ $(n \in N)$.

We realize that, in case $(N, 0, S)$ is the Peano system that I. M. fixed in his mind, $g(n)$ is equal to

$$\underset{\substack{\uparrow \\ n}}{f}(\underset{\substack{\uparrow \\ n-1}}{f}(f(\ldots \underset{\substack{\uparrow \\ 1}}{f}(a)\ldots)) = f^n(a),$$

the result of applying f to a n-times. Really, g is defined by a very special recursion, *iteration*. The fact that we thus have an equivalent definition is to say that the Principle of Iteration is equivalent to the Principle of Induction when the latter is bolstered by the first two Peano axioms.

This alternative definition of "Peano system" is due to F. W. Lawvere. I. M. should now sit down and prove to his satisfaction that indeed, we have an equivalent definition. Of course, he is allowed to use set theory; it is unlikely that he is unfamiliar with the needed tools.

This definition is nice for instance because it is more compact than the traditional one. There is no talk about S being one-to-one and missing 0 in its range; also, it is uniform in the sense of being "diagrammatic," using sets and functions, but not properties (subsets). But really, the nicest thing about it is that it has a *pattern*. It looks like we can talk about a *morphism* between any two "such things," $(B, b : t \to B, h : B \to B)$ and $(A, a : t \to A, f : A \to A)$ (let us call "such things" *pre-Peano systems*), as being a map $g : B \to A$ making the diagram

$$\begin{array}{ccccc}
t & \xrightarrow{\;b\;} & B & \xrightarrow{\;h\;} & B \\
{\scriptstyle i}\Big\| & \circ & \Big\downarrow{\scriptstyle g} & \circ & \Big\downarrow{\scriptstyle g} \\
t & \xrightarrow[\;a\;]{} & A & \xrightarrow[\;f\;]{} & A
\end{array}$$

commute; and then we can say that a Peano system is distinguished among pre-Peano systems by the fact that it has *exactly one morphism* to any pre-Peano system. (An "impredicative" definition if there ever was one!)

Let us probe I. M.'s convictions about the notion of number.

Q. You said that you fixed any Peano system for the purposes of "number." Does it not matter which one you have chosen? To put it differently, suppose you are probing some property $P(n)$ of the natural numbers, and you are not sure whether the property is universally true or not of numbers. You discover that it is indeed true in your chosen system. How do you know that it would have turned out to be true if you had picked another Peano system as your numbers?
I. M. Aha! Good question. Well, the answer is simple. If I have discovered that the natural numbers have that property you have in mind, then I must have inferred this on the basis of the general principles of mathematics (set theory if you wish), and *on the basis of the definition of "Peano system"*; as I say, I make a point when I reason about numbers of not using anything else than the defining properties of "Peano system."
Q. But wait; does everybody make such a commitment? Maybe I explicitly refuse to make such a commitment; I simply take my system of natural numbers, a Peano system, and by direct examination, mixed with ingenuity, I see that in my system $P(n)$ in fact holds for all n. I tell you about this; I describe to you how I directly examined my Peano system. You said that you can do anything that I can do with numbers. Are you sure that you can now also show that all n satisfy $P(n)$, given that in effect you have committed yourself to using only methods of proof that apply to *all* Peano systems?
I. M. How clever—that is a good question indeed. But there is an important fact here: actually, any property that is verified in one Peano system is going to be true in any other. More than that: *any property of any Peano system as a whole will also be shared by any other Peano system.* Take the example that the underlying set of a Peano system (the set of natural numbers, in any one interpretation) can be mapped in one-to-one fashion into any infinite set. This is a fact; but even if I did not know that, but knew that it was true for one Peano system, I would know that it was true for any other. The reason is that any two Peano systems are *isomorphic*; there is a one-to-one and onto mapping from one to the other taking the zero of one to the zero of the other, and taking a pair that is in the relation of one term's being a successor of the other term into a pair in the same relation in the other. Isomorphic structures share all conceivable properties.
Q. That is indeed interesting. I can actually see why any two Peano systems are isomorphic; this is quite easy using Lawvere's definition. We

have a morphism from one to the other, and from the other to the one; moreover, their composites are self-morphisms of the systems, and thus they must be identities; I really have an isomorphism, because I have an invertible morphism; great! But how do you know that isomorphic structures share all properties? I can see this holds for the one you just quoted; this is quite obvious. But why so in case of any conceivable property? Anyway, you cannot be quite right. Look: suppose you take the property that the empty set $\varnothing$ is an element of the Peano system. This is clearly not going to be shared by all Peano systems; I can willfully make this both true and false by simply exchanging elements, clearly not disturbing the fact that I have a Peano system! Something is wrong.

I. M. No, no! Of course, when I said "all conceivable properties," I meant "all meaningful properties," "all properties of Peano systems *as* Peano systems." $\varnothing$, being an element of the Peano system, is not a meaningful property of Peano systems at all; no sane person would contemplate such a property.

It is clear that communication is breaking down at this stage. I. M. cannot really say what he means by a meaningful property of Peano systems, although he can unerringly say of any particular proposed property whether it is meaningful or not. Of course, he could say that a meaningful property is, by definition, one that is invariant under isomorphism: if it holds of one structure, it holds of any other that is isomorphic to the first. But that begs the question; how is he so sure, and so quickly, when he is presented with a particular property? It is important to see that he can make the judgment in the cases when he has no idea whether the property in question does in fact hold in Peano systems. There is, in fact, a more basic problem. Defining "meaningful" by referring to the condition of invariance under isomorphism, one would be making an inadmissible move. The meaningfulness of a phrase should be a matter of grammar, not of a question of a possibly difficult-to-verify mathematical fact. In fact, it is demonstrably undecidable whether a property formulated in set theory of Peano systems is invariant under isomorphism.

I. M.'s colleague, I. L., a student of Alfred Tarski, overhears the discussion and joins in.

I. L. This is a question of logic. The meaningful properties of structures are the logical properties. You have in fact several precisely and explicitly defined logical languages in which you can formulate properties of structures, and you can rigorously show that these properties are invariant under isomorphism. The most basic of these languages is first-order logic. As Tarski has shown, we have a precise notion of truth for sentences of first-order logic in structures of the right kind (interpreting all the symbols in the sentence in question), defined globally for the whole language of first-order logic at once. When for instance we were asking whether $P(n)$ held for all n, we were asking whether the Peano system $(N, 0, S)$ satisfied a sentence $\forall n \hat{P}(n)$, where $\hat{P}(n)$ is a first-order formula expressing the property $P(\_)$, of course provided that such a formula is available. As a matter of fact, in most cases of interesting Ps it is *not* available; in most cases, it would become available if we added new operations such as addition and multiplication to the primitives of the structure $(N, 0, S)$.

Now, the important point is that, with the help of the Tarski truth-definition, there is a rigorous proof of the fact that properties given by first-order sentences are invariant under isomorphism; it is a proof by induction on the complexity of the sentence in question. Thus, there is a partial answer to the question "What are I. M.'s meaningful properties?"; they include the first-order properties. It is true, however, that most mathematically interesting properties of structures—and now I am talking about not just Peano systems but all the various structures mathematicians use: groups, rings, topological spaces, and so on—are in fact not expressible in first-order logic. The way out is that there are other, more expressive languages for which the Tarskian way of defining truth is also available; now the truth-definition may be more conspicuously dependent on set theory, but in fact, it was so dependent already in the case of first-order logic. These languages include second-order logic, higher-order logic, infinitary logics, logics with generalized quantifiers. It turns out that the proof that a property expressible in any of these languages is isomorphism invariant is a very straightforward structural induction on the complexity of the expression involved.

Q. May we then say that the idea of a "meaningful property of a structure" is ultimately an open-ended one; there is no uniform syntactical criterion that describes all the meaningful properties that mathematicians contemplate?

I. L. Yes, this is correct; there are large classes of explicitly described properties that are meaningful, for which we indeed have a proof in advance that they are isomorphism invariant, but it seems rather impossible to give such a class that would be all-encompassing.

I. M. Mind you, this is not a problem in practice. I am working in group theory, and I have never yet asked myself whether something somebody proposed or asked about groups *made sense*; it simply always did when the person was mathematically competent, even on a minimal level. And when somebody proposes something like "Is the monster-group equal to the first prime number greater than 10^{100}?", then I know that this is meaningless, you are comparing apples and oranges. Mind you, when you ask "Is the monster-group the same as the largest sporadic simple group?", you are not asking about a literal equality of two objects; you are asking whether those two things are *isomorphic*. You see, provided you have identified the monster-group as a specific set, the set-theoretic statement "The monster-group is equal to the first prime number greater than 10^{100}" is meaningful as a set-theoretic statement, but not as a *mathematical* statement.

I will summarize by saying that we have a tantalizing two-faced situation within the set-theoretical foundation. On the one hand, through the process of self-reflection it provides glimpses, collectively called "logic," of large connected parts of a language of mathematics that treats numbers as numbers, and in general, structures as structures inasmuch as it allows only meaningful propositions about those things. On the other hand, it does not give an articulated and complete statement of what such a language, Logic, is; rather, it makes it look likely that such Logic does not exist. I would like to obtain a foundational language in which all well-formed propositions are meaningful.

3.3 The Nature of Things

I. M. was urging the view that one does not have to assume that terms like 3, 17, 10^{10} have absolutely fixed denotations; the latter can be made dependent on a free choice of a structure of natural numbers, a Peano system, in which the denotations then become determined. It also seems that this attitude toward denotation is the prevailing one in mathematics, dealing as it does with definite descriptions of various entities—"the monster-group," "the field of the real numbers," and so on—in a systematically ambiguous manner. This may be so in mathematical practice, but it is entirely possible that this way of behaving is necessarily a metaphorical one, and when it comes to the crunch of articulating a foundation for mathematics, this ambiguity has to be given up. This would not necessarily mean that one would have to adopt the precise foundations of Zermelo-Fraenkel set theory, but it would show that something like that system, with a single domain of well-individuated objects at its base, is necessary, after all; and in particular, it seems to me, it would answer our question "Is it possible to articulate a language of mathematics in which we talk about numbers as numbers?" in the negative.

And indeed, there are many signs in mathematical practice suggesting that ultimately it may be necessary to fix reference of terms unambiguously.

In "Complete Functors in Homology I" Max Kelly writes:

> (The objects of a category have but a tenuous individuality; 'the group $\mathbb{Z}$ of integers' is not the same set of elements to different writers, or even to the same writer in different contexts. In a sense it is only the isomorphism class that counts; and yet we must at any instant be considering a definite set of elements, in order that we may sensibly talk of homomorphisms: a group is not the same thing as a group type, because there are non-identical automorphisms. . . .) (1964, 722)

This partially quoted parenthetical remark is in a section called "Generalities on Functors." It confirms the view that the systematic ambiguity of mathematical objects discussed above *reigns within a category*. However, when it comes to a functor $F : \boldsymbol{X} \to \boldsymbol{A}$, that is, a mapping connecting two categories, it seems that the very notion denies systematic ambiguity; it assigns a definite object $F(X)$ as value to any argument object X in the domain category $\boldsymbol{X}$. In other words, when we step out of the context of a single category, where the precise identity of objects did not matter (see Kelly's "tenuous individuality"), and want to bridge the worlds of two categories in a global context, we seem to be forced to use tools (the functor, for instance) that do refer definitely (to the object $F(X)$ in this case), and not systematically ambiguously.

And indeed, there is an entrenched view in philosophy that truth in mathematics must be explained by a standardly referential semantical theory. Benaceraff (1973) argues to this effect, and further that this circumstance leads to an inevitable conflict between the demands of ontology and epistemology of mathematics. Without committing myself on Benaceraff's conclusions, I want to call into question the part of his basic position that concerns the necessity of a standardly referential semantical theory of mathematical truth. Along the way, I want to open up the possibility of a nonstandardly referential theory of truth, in which a systematic ambiguity of terms reigns, and thus to make it plausible that it is possible to deal with numbers as numbers in an articulate manner. Incidentally, there is a specific parallel technical issue, in the context of functors; one needs to show that "functors are not necessary," and I have made an effort to this effect in Makkai 1996. It is interesting to point out that two parenthetical paragraphs in Kelly 1964 contain a partial formulation of the notion that should replace that of functor (the concept of "anafunctor," as I call it; Kelly's description is partial since the necessary condition of "saturation" is not mentioned), though without a firm suggestion that this notion could be used to systematically reintroduce ambiguity of objects in categories.

I want to challenge Benaceraff's (1973) basic commitment to

> (1) the concern for having a homogeneous semantical theory in which semantics for the propositions of mathematics parallel the semantics for the rest of the language ... (p. 661)

The quotation is footnoted as follows:

> I am indulging here in the fiction that we *have* a semantics for "the rest of the language", or more precisely, that the proponents of the views that take their impetus from this concern often think of themselves as having such semantics, at least for philosophically important segments of the language. (p. 661)

The footnote shows that Benaceraff is trying to leave the door open for himself to abandon the commitment to the above concern. Even if he did not himself have this concern, his overall argument to the effect that

> almost all accounts of the concept of mathematical truth can be identified with serving one or another of these masters [the other master is "(2) the concern that the concept of mathematical truth mesh with a reasonable epistemology"] *at the expense of the other* (p. 661)

would stand up. On the other hand, the rest of the paper shows that he is, after all, committed to the stated concern. For instance, he writes:

> Some (including one of my past and present selves [and here he refers to Benaceraff 1965]), reluctant to face the consequences of combining what I shall dub such a "standard" semantical account with a platonistic view of the nature of numbers, have shied away from supposing that numerals are names... (p. 664)

Thus, to repeat, I am not really arguing with Benaceraff's main thesis. What I am arguing is on a more basic level. I am going to deny that one *can* maintain (1); I will assert that *one cannot have a homogeneous semantical theory of the world as we know it, even if we disregard mathematics proper*; I will assert that the "fiction" Benaceraff refers to is a fiction properly, and it is to be discarded.

To fix ideas, let us look further at Benaceraff 1973:

Consider the following two sentences:

(1) There are at least three large cities older than New York.

(2) There are at least three perfect numbers greater than 17.

Do they have the same logicogrammatical form? More specifically, are they both of the form

(3) There are at least three *FG*'s that bear *R* to *a*.

. . . ? What are the truth-conditions for (1) and (2)? Are they relevantly parallel? . . . [I]t seems clear that (3) accurately reflects the form of (1) and thus that (1) will be true if and only if the thing named by the expression replacing '*a*' ('New York') bears the relation designated by the expression replacing '*R*' (① is older than ②) to at least three elements (of the domain of discourse of the quantifiers), which satisfy the predicates replacing '*F*' and '*G*' ('large' and 'city', respectively). . . . But what of (2)? May we use (3) in the same way as a matrix in spelling out the conditions of *its* truth? That sounds like a silly question to which the obvious answer is "Of course". (p. 663)

Thus, for Benaceraff the homogeneity of our semantical theory means that it is like the account of (1) given above via (3). I claim that we do not have this kind of account of our reference, and truth-determination, even in discourse about certain entities that we habitually encounter in real life.

To clarify the respective positions further, before I get to the claim itself, let me point out that we see here the causal theory of reference at work. "I believe in addition in a causal theory of *reference*. . . ," writes Benaceraff (1973, 671). The causal theory of reference is described, for example, by Maddy (1992, 38–41), who attributes it mainly to Saul Kripke in *Naming and Necessity* (Kripke 1972). According to this, the naming that Benaceraff refers to when he talks about the "thing named by the expression 'New York'" involves a causal chain of events that starts with an act of "initial baptism," the dubbing of the thing New York with the name 'New York,' an event in which New York and the dubber had to be both physically present.

I believe in a fundamental *inhomogeneity* in the kinds of things that make up the world. On the one hand, we have the "*medium-sized physical objects*" to which the causal theory of reference and of knowledge applies; but we also have, in a way parasitically living on those objects, things that I call *representational objects*: things that exist only in ambiguous representation by physical objects, but nevertheless have an irreducible presence in our world. We do not refer to representational objects directly; we refer to them *through* representations of them. Before I say what these representational objects are, I want to emphasize that I do not claim to have classified all things into the two groups I mentioned; in fact, I think, there are further kinds of things that are neither "medium-sized physical objects" nor representational objects. However, I also believe that mathematical objects have much to do with representational objects, even if I am not prepared to fully identify the former with some of the latter. Thus, my case for representational objects is but a preparation of a case for mathematical objects *as such*.

Representational objects are the ones that are symbolically represented; they are invariably artificial, human artifacts. Take William Blake's poem "The Chimney Sweeper." You probably know this poem; if you don't, I will tell you how you may find it, although this will not be the only possible way. The reference is *Blake's Poems and Prophecies*, edited by Max Plowman, in Everyman's Library (London: Dent, New York: Dutton), 1972 reprint. The poem is found on page 11. Actually, I did not in this way give you a unique way of finding the poem. In referring to *Blake's Poems and Prophecies*, I gave you only a type, not a token; here, in my hand, I have a particular copy of the book, and I can show you the poem on the 11th page. Thus, the book I described is another example of a representational object! This immediately tells you something about the hierarchical interrelatedness of representational objects.

Can I *doubt* the existence of Blake's poem "The Chimney Sweeper"? Does Blake's poem "The Chimney Sweeper" exist? Is this question a meaningful one? It seems that the question may have two different meanings. The "nonphilosophical" meaning is the question asked by someone having a possibly superficial but referentially still firm idea about Blake to the effect of whether Blake has indeed written such a poem (we hope he did not write two with this same title...). This is an unproblematic question from the point of view of the theory of truth and reference; the ways of answering it are, in principle, satisfactorily circumscribed by the "causal theory of reference and knowledge." But there is also the "philosophical" meaning of the same question; it is in force when we ask, "Does Blake's 'The Chimney Sweeper' exist *as a poem*?"

You may maintain, "No, there are no poems as such; what there are, are the ordered sets of inscriptions that run as follows:

"The Chimney Sweeper"
When my mother died I was very young,
(etc.)

Well, what about an electronically stored copy of Blake's poem? You say, "Of course, I meant that also in an extended sense of 'ordered set of inscriptions.'" Can you be more explicit? You may resort to the formula on the inside covers of books ("No part of this book shall be reproduced, stored in a retrieval system, or transmitted by any means, electronic, mechanical, photocopying, recording, *or otherwise*..."), in order to exhaust all the possible representations of the poem. But note the indefiniteness in the italicized phrase "*or otherwise*"; the practical people know that it is impossible to foresee all possible ways one may represent the poem, and they want to forestall *all* (lucrative) representations. Trying to say that the poem's essence is somehow synonymous with the totality of its representations is wrong-headed, for two reasons, at least. One is that we know perfectly *now* what the poem is, but we cannot know *now* all its representations, not even the possible types of its representations. The other is that *one* representation is perfectly enough for knowing what the poem is!

Then again, you will now say that referring to Blake's poem is just a *façon de parler*; you can eliminate it from any context, in exchange for references to first-class objects. I doubt that; how do you do that when I say, "I like Blake's poem 'The Chimney Sweeper'"? Here we enter an infinite, and familiar, controversy, involving behaviorism and the like. Suffice it to say that the corresponding question that we really care about, the one concerning objects in mathematics, is answered definitively: we know that, for instance, the presence or absence of the notion of natural number does make a difference in our ability to prove theorems (a form of Gödel's incompleteness). In other words, we cannot eliminate the notion of natural number from contexts.

In the last two paragraphs I tried to show a bit what it would be like to deny the existence of "The Chimney Sweeper" *as a poem*. I think, however, the best reaction to the "philosophical question" of the existence of "The Chimney Sweeper" *as a poem* is that it is *meaningless*. There is no mystery, we have everything in front of us; the question cannot seriously imply a search for something temporarily unknown answering a description, and it seems that how we answer the question is largely arbitrary.

Before we probe matters along these lines a bit more, I want to suggest that the question of the *existence* of the system of the natural numbers may be to a large extent similar and, as a consequence, *meaningless*. Here you will quickly object, "No; whereas in the case of 'The Chimney Sweeper' the question of the *existence of a representation* is not in question, now, with the system of the natural numbers, this is precisely the important question." To which I will reply, "Certainly, if by a representation of the system of the natural numbers we mean something involving an actual aggregate (set) of elements; but I do not think we have to, or even should, do so." Note also that the existence of a Peano system *within* a formal system of set theory, say, is of course meaningful; in fact, it is expressed by a proposition of the theory, which may or may not be provable in the theory.

The being of the poem "The Chimney Sweeper" as a poem is exhausted by our ability to talk about it as a poem. Talking about it as a poem is to be referring to its representations, possibly simultaneously more than one (for instance, the ones that you and I have in our minds), and *to make sense* while doing so. For instance, "in the second line of the poem" makes sense as talk about the poem, even though, strictly speaking, it is each of the representations that has a second line. On the other hand, "the typeface of the poem" does not make sense as talk about the poem as such; rather, with that talk we immediately know that we are talking, not about the poem as such, but about one of its representations. In natural language, we do not want to rigidly separate the higher-level talk about the poem as such and the lower-level talk about its representations; it is the very spirit of natural language that we want to be able to jump from one context to the other instantaneously. On the other hand, the legendary *purity* of mathematics is rooted in an opposite kind of requirement; in mathematics, we want to be able to talk about structures as structures, and more generally about mathematical objects as such, in a permanent manner. It is this requirement that compels us to articulate formal languages for mathematics.

We should add that talk about the poem as such grows out of talk about its representations. Continuing the work of the poet, we bring the poem into existence as a poem by articulating our talk about it as such. This is another reason why the two types of talk are not, and cannot be, rigidly separated. In fact, the process of creating the poem is an ongoing one; it has not been quite completed. In mathematics, we have completed our talk about our abstract objects to a greater extent, although the difference is only a matter of degree.

One might think that talking about poems is inappropriate in connection with mathematics. Of course, there are many other types of representational objects, and none is more important than the ones that arise in the world of computers: computer programs, pieces of software. It is in the domain of computer software that we see the characteristics of representational objects most clearly. The talk about the software becomes sharply separated from the representation, which is a particular implementation of the software. The existence of the software as such becomes more pronounced; we are now more ready to accept the existence of the software as something separate and independent from any implementation, more ready than in the case of the poem (perhaps). Another element in favor of the existence of the software as such is its potency. One may doubt whether poems *do* anything; but we do not doubt that pieces of software do things. They have very specific and occasionally large effects—of course, never in themselves, always in an implementation. Discussing, describing, planning around the effect of the software takes place mostly in talk that is independent of any particular implementation. I am inclined to the view that mathematical objects are like software, more specifically, like the *datatypes* of software.

But let us return to poetry once again. Maybe our brave talk about poems as genuine representational objects can be shown up as hot air, after all. It is very well to talk about the many representations of "The Chimney Sweeper" in books, in people's minds, and so on; but there is a distinguished, an authentic, an original representation: the one that Blake himself put to paper. When we talk about the poem "The Chimney Sweeper," we talk about this original copy; we talk about a first-class object pure and simple. When we use ostension and point to the copy on page 11 of my book, what we do is deferred ostension, to use Quine's expression. We mean referring to the Original; we *refer* to the Original. This is what Benaceraff meant by the "homogeneous semantical theory of the rest of the language," that is, the language that refers to reality as opposed to mathematics.

Before we try to answer this devastating argument, we have to acknowledge its force. In this we can see why we want the Platonistic, uniquely referring, standard semantical theory: we want authenticity, an objective point of reference, to which we can return when in doubt. There may always be a question whether a particular printing of the poem is correct; ultimately, the only way to answer this question is to go to the original and verify whether the copy in question and the original can be mapped to each other perfectly.

The attempt to answer the argument is this. Assume you discover that in fact, there is no original "The Chimney Sweeper." In fact, worse than that, you discover that there is something entitled "The Chimney Sweeper" in a manuscript of Blake's, but it differs substantially from the "standard version," the version *that has been most frequently reprinted*; moreover, no trace of a version of the poem *as we know it* can be found in Blake's manuscripts. You counter: This does not matter; the poem *as we know it* was composed by someone at some definite time and place, and when we talk about the "The Chimney Sweeper" *as we know it*, we talk about *that* original, even though it may differ from anything attributable to Blake himself. To which I say that now the original of "The Chimney Sweeper" *as we know it* is a purely fictive entity; it has lost its role as the grounding of authenticity. However, in fact still there is no problem about what "The Chimney Sweeper" *as we know it* is; it simply is what it is, and no one will try to authenticate it, especially now (we are still within the assumption that we have lost the good connection to Blake) that we know that it cannot be authenticated. To which you say: There is no way of stopping people from fixing the first, or most visible, or whatever, but in some definite way *unique* source where the "The Chimney Sweeper" can be found, a source which becomes the *standard reference* to "The Chimney Sweeper" *as we know it*. And then I reply: Oh, but now you cannot say that "The Chimney Sweeper" *as we know it* is, by definition, that given by the standard reference, since in fact there was something entirely clearly defined before we had located that standard reference; the location of the standard reference was an *a posteriori act* of mock authentication, an act entirely dependent on the clear and prior idea of "The Chimney Sweeper" *as we know it*.[1]

Maintaining "the fiction of the homogeneous semantical theory for the language of the nonmathematical world" (Benaceraff's words, essentially) is hard work. I am suggesting that it is too hard, what with all those gadgets that come flooding into our lives, each having little or large pieces of structure, defined in terms of some abstract functionality or what have you. To make sense of all this, we resort to representational objects, rather than trying to find unique authentic originals to which we would have to run in case of doubt. Certainly, these representational objects are abstractions (I do not mean to imply that the term I just used *explains* them; rather, its use puts them in their place as inferior in some sense to concrete things) and thus decidedly second-class objects that cannot live without being rooted in medium-sized physical objects. But still, we cannot do without them.

Is this a *pragmatic* argument? Yes, it is; but remember, the argument is not about the existence or nonexistence of representational objects, but about the fact or nonfact of the homogeneity of the semantics for the language of the nonmathematical world. The conclusion may be that the idea of the homogeneous, uniquely referential semantics vis-à-vis the world is in fact *incoherent*; if so, it is even worse for the fiction that Benaceraff admits to be indulging in. It now seems to me that we do mathematics precisely to get away from all this compulsion of authentication, and deal with what we call mathematical objects as we know them.

At this point, I feel optimistic that a foundational stance in mathematics that lacks the standard feature of unique reference to objects may be viable. However, the real test of it still remains a Fregean full articulation. Before we have built the whole language, we cannot be sure whether we do not, ultimately, smuggle in the "originally baptized," authentic entities that the Platonists want.

Note

1. Ray Jackendoff has suggested to me a real example paralleling the hypothetical case described above of being unable to find an authentic original. I describe it essentially in his words.

There is a piece of music that has been in the repertoire at least since the early nineteenth century, which is called "Sinfonia Concertante for oboe, clarinet, horn, bassoon, and orchestra by Mozart." Only there is no known manuscript for this piece. In a letter, Mozart himself referred to having written a sinfonia concertante for flute, oboe, horn, and bassoon—which is totally unknown—and people generally assume that the piece we know is a later reorchestration of that. The trouble is that all attempts to "reconstruct" the original have been awkward at best; basically, the clarinet part in the piece we know cannot easily be transferred to idiomatic writing for any of the instruments mentioned in Mozart's letter. Moreover, the last movement of the piece we know has some parts that Mozart never would have written. So no one knows where this piece came from, prior to its first publication some years after Mozart's death.

Jackendoff also pointed out to me that he has had to deal with representational objects in his work on music, which involves an additional layer of performance, making the semantics of, say, "Beethoven's Fifth Symphony" even stranger.

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