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Critical Studies/Book Reviews

CHARLES S. CHIHARA. ***A Structural Account of Mathematics.*** Oxford: Oxford University Press, 2004. Pp. xiv + 380. ISBN 0-19-926753-7.

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There are now four books by Charles Chihara. I learned a great deal from Chihara when I was his student, learned more from each of his first three books, and have found yet more to be learned in this new one. Like its predecessors it is a combination of criticism of other philosophers of mathematics with exposition of a positive program. On the positive side, Chihara now merges the nominalism of his earlier works with a form of structuralism. On the critical side, the philosophers attacked range from the reviewer and his co-author Gideon Rosen, who are neither nominalists nor structuralists, to Geoffrey Hellman, who is both but combines the two 'isms in a way not to Chihara's liking. Two structuralists who are not nominalists, Michael Resnik and Stewart Shapiro, come in for especially heavy criticism. Given the wide range of views and topics considered, any review of tolerable length must be highly selective. About Chihara's criticism of Shapiro, Resnik, Hellman, and others, I will only say that Chihara is reasonably fair and thorough in his examination of arguments in the literature *pro* and *con* various positions, and that he adds diverse observations and insights of his own, so that the reader, whether sympathetic or not to structuralism or nominalism, will come away from the discussion with a good sense of the complexity of the questions in this area. That said, I will for the remainder of this review confine myself to exposition and critique of Chihara's own positive program. Implicit in my criticism of Chihara's position will be responses to a few of his many criticisms of mine.

1. Structuralism without Nominalism

Let me begin with non-nominalist structuralism, illustrating the view by its application to the case of real analysis.[1] A typical work in real analysis contains theorems that appear to be about a specific structure,

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[1] Actually, what I will describe is just one of several interpretations that are called 'structuralist' in the literature, but it is the one most relevant in the present context, since Chihara's interpretation is a nominalist variant of it.

the real-number system, and specific objects in its domain, such as the number one. But a typical work actually says nothing about 'the' real numbers and 'the' number one that would not be equally true of any complete ordered field and its multiplicative identity. On the *structuralist* interpretation, the theorems are taken to be generalizations about *all* complete ordered fields: The symbol $\mathbb{R}$, which appears to be a name of a specific structure, is taken to be a variable ranging over structures. Analogous structuralist interpretations exist for other branches of mathematics.

Now a distinction must be made here between two senses of 'interpretation'. A structuralist interpretation of the theorems of some branch of mathematics might be advocated either as a *description* of the meaning that currently is attached to the words of these theorems, or as a *prescription* as to what meaning ought to be attached to the words of these theorems. Chihara repeatedly and emphatically says that the nominalist variant of structuralism he advocates is not being put forward as an empirical hypothesis about the meaning of current mathematical language. And he has good reason not to advocate structuralism as such an hypothesis. For while the structuralist interpretation, taken descriptively, may be plausible for the writings of mathematicians of the present day in many branches of mathematics, it is not very plausible as a universal hypothesis about the intended meaning of what has been written by mathematicians *and non-mathematicians* of the present day *and earlier historical periods* not just in some but in *all* branches of mathematics. To illustrate, I will enumerate five areas where I think a structuralist interpretation is questionable.

Set theory. A generalization about 'all' structures of a certain kind, such as complete ordered fields, is vacuous unless there *are* some structures of that kind. For a proof that there are some, one turns to set theory, since in present-day mathematics, it is set theory that serves as the background theory in which other branches of mathematics are developed. (It is, for instance, the topic of the first volume of Bourbaki's encyclopedic *Éléments de Mathématique*.) In the case of the existence of complete ordered fields, which has to be established to show the structuralist interpretation of real analysis is non-vacuous, there are several constructions that can be used, beginning with Cantor's and Dedekind's, and all are set-theoretic in character. It is also to set theory that one turns for the very definition of 'structure': A structure is a set together with certain distinguished relations and/or certain distinguished functions and/or a distinguished family of subsets.[2] The domain of a structure is a set, and the objects in the domain are its elements. This creates a serious problem of circularity if we try to impose a structuralist interpretation on set

[2] There is alternate usage, avoided here, in which these items are called 'systems' and the word 'structure' is reserved for isomorphism types of such items.

theory itself, and to interpret assertions about 'sets' and 'the null set' not as assertions about a specific kind of object and one specific object of that kind, but as assertions about any model of the axioms of set theory, and whatever object in the domain plays a certain 'null' role in the model.

Abstract algebra. After the first page or so, group theory is *not* concerned with the features common to all models of the group axioms, but rather with relationships among different models of those axioms, which is to say, different groups, especially those relationships established by certain kinds of functions, called homomorphisms. This makes it difficult even to imagine what a structuralist interpretation of the classification theorem for finite simple groups, say, would look like, unless it were simply a structuralist interpretation of the background set theory in which the existence or non-existence of groups and homomorphisms with various properties is established, and the theorem just mentioned ultimately proved. Similar remarks apply to other branches of abstract algebra.

Real analysis before Dedekind and Cantor. However appropriate the structuralist interpretation of real analysis may be for the period since the 'arithmetization of analysis' in the nineteenth century, there is quite direct evidence that mathematicians of earlier periods had a quite different interpretation in mind. Newton, for instance, tells us quite directly, in the first pages of his *Universal Arithmetick*, that real numbers are 'abstracted ratios' of magnitudes such as lengths. Thus there is quite direct evidence that on Newton's understanding of them, theorems of real analysis were theorems about a specific structure, the objects of whose domain were ratios of magnitudes such as lengths. This was, in fact, the dominant understanding in the seventeenth and eighteenth centuries.

Euclid's geometry. Going back further in history, to Euclid, we find that, unlike Hilbert at a much later period, he does not take points, lines, and circles to be just any objects satisfying the axioms of geometry, but rather takes them to be objects of certain specific kinds with which his readers are expected to be familiar. Perhaps the clearest indication of this fact is provided by the feature of the *Elements* whose seeming pointlessness has so often puzzled those brought up on modern axiomatics: his offering of definitions that play absolutely no role whatsoever in the subsequent development of his theory, beginning with the notorious 'A point is that which hath no part'.

Business arithmetic. As a structuralist analysis takes theorems of real analysis to be theorems about all models of the complete-ordered-field axioms, so a structuralist analysis takes theorems of number theory to be theorems about all models of the Peano postulates. However, the derivation of the 'cardinal' properties of natural numbers from the Peano postulates is considerably less immediate than the derivation of the 'ordinal' properties. It is not surprising that, for instance, in the famous textbook of Mac Lane and Birkhoff [1967] we find in the chapter (II) devoted to the integers, that the cardinal properties of natural numbers are not introduced until more than halfway through, and in a section (§6) starred as optional. But the cardinal properties of natural numbers seem so central to everyday applications of basic arithmetic that it is virtually inconceivable that the non-mathematician's intuitive notion of natural number is one on which their cardinal properties are really the kind of afterthoughts that an exposition based on the Peano axioms, like Mac Lane's and Birkhoff's, makes them appear.

2. Nominalism without Structuralism

Before describing Chihara's nominalization of structuralism or structuralization of nominalism, let me first offer, as Chihara himself does, a summary restatement of the non-structuralist nominalism of his second book [1990]. Chihara's goal in that work was to provide a nominalistic interpretation of the simple theory of types with infinity, which I will call T. This is a theory about sets. Its language L has one style of variable $x, y, z, \ldots$ ranging over individuals, another style $X, Y, Z, \ldots$ ranging over sets of individuals, and further styles of variables ranging over sets of sets of individuals, sets of sets of sets of individuals, and so on, along with symbols for elementhood. An *axiom of infinity*, the technicalities of whose formulation need not detain us, asserts the existence of infinitely many individuals. An *axiom scheme of comprehension* asserts for any formula $\phi(x)$ of L, which may contain 'parameters' or free variables other than the distinguished free variable x, and for any values of any such parameters, the existence of a set X of which will have as an element an individual x if and only if $\phi(x)$ holds for those values of the parameters. An *axiom of extensionality* asserts that sets having the same objects as elements are identical. There are analogous comprehension and extensionality axioms at higher levels, which is to say, for sets of sets, sets of sets of sets, and so on.

As a first step, we may reinterpret T in a theory T^* of *open sentences*. Its language L^* has the variables $x, y, z, \ldots$ ranging over individuals, variables $\xi, \nu, \zeta, \ldots$ ranging over open sentences, not necessarily of L^* itself, with the sole free variable x, further variables ranging over open sentences, again not necessarily of L^* itself, with the sole free variable ξ, and so on. The notion of an object x being an element of a set X is replaced by the notion of an object x satisfying an open sentence ξ, and similarly at higher levels. There is still an axiom scheme of comprehension, according to which for any formula $\phi(x)$ of L^*, which may contain parameters, and for any value of those parameters, there exists an open sentence ξ not necessarily of L^* itself, with the sole free variable x, which will be satisfied by an individual x if and only if $\phi(x)$ holds for those values of the parameters. Such a ξ might consist of $\phi(x)$ itself with the free variables other than x replaced by *names* of the values of the parameters. Such a ξ would not be an open sentence of L^* itself, but only of an extension thereof that includes such names. There are analogous comprehension axioms at higher levels There are no extensionality axioms, since whereas distinct sets cannot have exactly the same individuals as elements, distinct open sentences *can* have exactly the same individuals satisfying them. Such open sentences are said to be *coextensive*. Because of the absence of the axiom of extensionality, we cannot get an interpretation of T in T^* simply by replacing $X, Y, Z, \ldots$ by variables $\xi, \nu, \zeta, \ldots$ and symbols for elementhood by symbols for satisfaction. We need also to replace assertions of identity $X = Y$ by assertions of coextensiveness $\xi \equiv \nu$; and what we have to do at higher levels is more complicated. But it can be done, and we do in the end get an interpretation of T in T^*.

This, however, is only the first step towards a nominalistically acceptable interpretation of T. For if 'open sentences' are taken to be abstract *types*, they are presumably unacceptable to nominalists, while if they are taken to be concrete tokens, then the axioms of comprehension are unacceptable, since there certainly do not exist as many concrete tokens as the axioms of comprehension assert. The acceptability of the axiom of infinity is also questionable. So a second step is needed. At this step, the ordinary quantifiers of L^*, 'there exists an individual x', 'there exist an open sentence ξ', and so on, get replaced by *modalized* quantifiers, amounting to 'there could have existed an individual x', 'there could have existed an open sentence token ξ', and so on, to obtain a new language L^{**}. The axiom that there exist infinitely many individuals can be replaced by the axiom that there *could* have existed infinitely many individuals, and, if one is careful, by the weaker hypothesis of infinity that however many individuals could have existed, there could have existed one more.[3] Axioms about the actual existence of open sentences that are satisfied by various individuals that there are get replaced by axioms about the possible existence of open sentences that would, had they existed, have been satisfied by various individuals that there are or that there could have been. Thus the axioms of T^* get replaced by axioms of new theory T^{**}, but the technicalities of their formulation need not detain us. The net result is that we get an

[3] There is a lapse in the book connected with this hypothesis, since a full proof of the Peano postulates as discussed in chapter 7 (pp. 181–184) requires the hypothesis of infinity, which is not mentioned until chapter 8 (pp. 226–227). From private communications with the author I gather that he had intended to insert a discussion of the need for this hypothesis earlier on, together with a reference to the fuller discussion of it in his earlier work ([1990], pp. 68–73), but that this intended discussion was inadvertently left out.

interpretation of T in a theory T^{**} that does not assert the actual existence of anything—that has no ‘ontological commitments’.

3. Chihara’s Commitments

The theory does, however, have ‘ideological commitments’ of two kinds. First, there is a commitment to modalized quantifiers. Second, there is a commitment to a notion of satisfaction that is applicable to all actual and possible languages, and not just to one specific language or a few specified languages. The first commitment was present already in Chihara’s first book [1973]. The second marked a change from his first to his second book, required in order to get an interpretation not just of Russell’s weak *ramified* theory of types, but of Ramsey’s strong *simple* theory of types. Both ideological commitments are controversial. Whether the modal notion ‘there could have been’, as contrasted with ‘there is’, is genuinely intelligible and scientifically respectable has, of course, been a contentious issue in philosophy. Whether we possess notions of truth and satisfaction that are applicable to arbitrary actual and possible languages, or whether on the contrary our semantic notions are initially parochial, applicable only to our home language, becoming extensible to other languages only insofar as we conceive of these as translatable into our home language, has also been a much-debated question.

The scope and limits of Chihara’s ideological commitments do not stand out clearly in his own exposition. As to modality, he makes his commitments seem heavier than they are by habitually, though unnecessarily, using the jargon of ‘possible worlds’. This jargon turns the commonsensical ‘There *could have been* things there actually aren’t’, which is all he is really committed to, into the mystifying ‘There is a possible world in which there are things that there aren’t in the actual world’, which seems to imply ‘There *are* things there actually aren’t’, which in turn seems, if not flatly self-contradictory, at any rate incompatible with nominalism. (Why strain the gnat of numbers if you are going to swallow the camel of unactualized possibilia?) Chihara’s third book [1998] was in large part devoted to dispelling the misimpression of ontological commitment to unactualized possibilia.

As to satisfaction, Chihara makes his commitments seem lighter than they are by habitually, and misleadingly, using an idiom of ‘constructibility of open sentences’, which naturally suggests writing down or typing up sequences of symbols, or perhaps assembling them out of blocks with symbols on them, an example Chihara himself sometimes uses. What gets obscured by this mode of expression is the fact that the notion of satisfaction he needs cannot be construed as a two-place relation between objects and certain special physical bodies, tokens of open sentences, as the following example should make clear. So far as I know, ‘$\wr\circ\wr\ x$’ is not an open sentence or open formula of any actual natural or artificial language, and it certainly is not part of standard English. It may occur to me, however, to introduce a slight extension of English, to be called *Evenglish*, in which '$\wr\circ\wr\ x$' abbreviates the following: 'x is a string of strokes having two parts which (a) each consist of consecutive whole strokes, (b) do not overlap, (c) together make up the whole of string x, and (d) are like-shaped'. Then '$\wr\circ\wr\ x$' is in effect an open sentence of Evenglish satisfied by things like '||' and '||||' and '||||||' and not by things like '|' or '|||' or '|||||'. Now while all this is occurring to me, it may be occurring to you, off in another room, to introduce a slight extension of English, to be called *Oddenglish* in which '$\wr\circ\wr\ x$' is to abbreviate an open sentence just like the one above except for having 'does *not* have two parts' in place of 'has two parts'. As an open sentence of Oddenglish, '$\wr\circ\wr\ x$' is *not* satisfied by '||' or '||||' or '||||||', but *is* satisfied by '|' and '|||' and '|||||'. Clearly it makes no sense to ask whether a given row of strokes satisfies '$\wr\circ\wr\ x$' or not, without mentioning whether '$\wr\circ\wr\ x$' is being considered as an open sentence of my language or yours.

Thus we seem to need a *three*-place relation, 'object x satisfies ξ when the latter is considered as an open sentence of λ', where $\lambda, \mu, \nu, \ldots$ are variables ranging over languages. But languages are presumably not concrete objects, so this will not do. Since apart perhaps from rare examples of multilingual puns, the same writer never uses the same expression as an open sentence of two different languages on the same occasion, perhaps the best solution for the nominalist would be to work with a two-place relation between objects and *tokenings* or acts of producing tokens, rather than between objects and the tokens produced by such acts. Tokening acts are arguably physical *events*, if not physical objects, and may not be too much for a nominalist to swallow.

There are indications that Chihara himself may have had something like tokenings in mind. Such indications can be found in an interesting passage (p. 210), where Chihara replies to a certain objection of Resnik's, running roughly as follows. Part of the justification, sketched above, for the axiom of comprehension in Chihara's set-up involved the assumption that *names* can be given to individuals as needed. Resnik's objection is that it may not be possible to give some individuals names because they are 'too small, too fast, or too fleeting'. In responding to this objection Chihara says two intriguing things. First, he tells us that to construct an open sentence 'it could be enough that some intelligent being performs some act', and a bit later he mentions more specifically 'hand signals'. This makes it seem to be tokening events rather than token objects that matter on Chihara's view. Second, Chihara also tells us, by way of alleviating Resnik's worry, that by 'it is possible to construct' he does not mean 'it is possible for *humans* to construct': The signals may be given by non-human hands.

Naturally one would like to be told something more about the nature of the non-human intelligent beings Chihara has in mind, beyond the fact that they have hands to signal with, and that no object is too small or too fast or too fleeting to be apprehended and named by them. Chihara does not tell us much, but since he is a nominalist, it is probably safe to assume that it is material rather than spiritual beings he has in mind, something more like extraterrestrials with superpowers than like angels or djinn. Needless to say, Chihara is not, like a disreputable UFO cultist, asserting that extraterrestrials actually exist; he is not even, like the more respectable SETI scientists, actively trying to discover whether they do. Now there are any number of persons who write about extraterrestrials without involving themselves with the question of their actual existence, and there is a name for the genre of writing they produce: *science fiction*. Chihara takes offense (p. 157) at any suggestion that there is some kind of link between this genre of writing and his own work; but I think it is not too hard to see why someone might make the connection.

4. Combining the 'Isms

Chihara's account of how structuralism is to be merged with nominalism is concise, and my summary of it will be curt. A statement of arithmetic or analysis ostensibly about 'the natural numbers' or 'the real numbers' can always be interpreted, structuralistically, as a statement in a background set theory about 'all models of the Peano postulates' or 'all models of the complete ordered-field axioms'. If the simple theory of types rather than standard Zermelo-Fraenkel set theory (ZF) is taken as the background theory, the structuralistic interpretation will be a bit more difficult. This is not because type theory is logically weaker than ZF, for nothing like the full strength of ZF is needed to establish the non-vacuousness of the interpretation (that is, to establish the existence of models of the Peano postulates or the complete ordered-field axioms). Rather, type theory is more difficult to work with because in type theory we cannot say anything about 'all structures', since we cannot say anything about 'all sets'. We can only make more restricted statements, about 'all sets of individuals' or 'all sets of sets of individuals' or 'all sets of sets of sets of individuals' and so on. Perhaps the easiest approach would be to use the fact that a weakened version of ZF, still strong enough to establish the existence of models of the relevant axioms, is well known by logicians to be interpretable in type theory. We can then get a structuralistic interpretation of our statement of arithmetic or analysis in type theory in two stages, the first being an interpretation of the statement in a weakened version of ZF, the second being an interpretation of the latter in type theory. Whether one proceeds in this two-step manner or in some more direct way, one further step will get us where Chihara wants to go. It is enough to combine a structuralistic interpretation in type theory with a nominalistic interpretation of the latter, so as to get a structuralisticonominalistic interpretation.

5. The Van Inwagen Problem

Chihara claims that his interpretation has five advantages, in that it is able to suggest solutions to five puzzles. In some cases the proposed solution depends more on the structuralist aspects of his interpretation, and in other cases more on the nominalist aspects. The two puzzles that get the most attention are those commonly called ‘The Van Inwagen Problem’ and ‘The Benacerraf Problem’. Structuralism is more relevant to the former, and nominalism to the latter.

The Van Inwagen Problem runs as follows. Consider some mathematical relation. Usually the relation of set to element is chosen as an example, but it may be more instructive to consider the relation of an ellipse to its eccentricity (the ratio of the length of its major axis to that of its minor axis, which is a real number greater than one). For instance, if E is an ellipse with a major axis twice as long as its minor axis, then E stands in this eccentricity relation to the real number 2. The neo-Scholastic metaphysician then asks, ‘Into which of the three categories admitted by neo-Scholastic metaphysics (internal, external, extrinsic) is this relation supposed to fall?’ To be an internal relation, it would have to hold in virtue of the intrinsic properties of E and of 2, each considered separately. It is easy to see what would be the relevant property of E: the fact that its major axis is twice as long as its minor axis. But, it is claimed, we have no idea what are the intrinsic properties of real numbers like 2. So we cannot categorize the eccentricity relation as intrinsic. And in fact, ‘We have no idea what are the intrinsic properties of real numbers’ is equally given as the reason why the eccentricity relation cannot be categorized external or extrinsic, either. The inability to fit the eccentricity relation into any of the three categories is supposed to show that there can be no such relation, and the fact that, while we have plenty of ideas about the relations of real numbers to each other, we have no idea of their *intrinsic* properties, is supposed to show that there can be no such objects.

On a structuralist interpretation, statements ostensibly about ‘the’ real numbers are really generalizations about all complete ordered fields, and the *only* statements about real numbers that make sense are those that make sense regardless of which model of the complete ordered-field axioms one is considering. Regardless of what model one is considering, there will be an object playing the role of 2, and in each model that object will have some intrinsic properties. But the intrinsic properties of the object playing the role of 2 will differ form one model to another, and therefore no statement about the intrinsic properties of ‘the’ real number 2 will make sense. This explains why, though we can legitimately say *some* things about 2, namely, those things that are equally true for any model of the object playing the role of 2, we cannot answer, and indeed cannot even legitimately ask, questions about the intrinsic properties of 2. Similarly, while for each model there will be an eccentricity relation between ellipses and the objects in the domain of that model, what kind of relation it is may differ from model to model, and this explains why we cannot answer, or even legitimately ask, questions about the proper categorization of the eccentricity relation. Such, in brief, is the structuralist solution to the van Inwagen problem.

There is, however, a simpler solution, which I take it (on the strength of a footnote (p. 23) of Chihara's) is due to Catherine Elgin. It is simply to say, 'If this mathematical relation cannot be accommodated by the categories of neo-Scholastic metaphysics, then so much the worse for neo-Scholastic metaphysics.'

6. The Benacerraf Problem

The Benacerraf problem is the puzzle, 'How could we come justifiably to believe anything implying that there are numbers, given that it does not make sense to ascribe spatiotemporal location or causal powers to numbers?' All nominalists agree on the following solution: 'We *can't* come justifiably to believe anything implying that there are numbers.' This 'solution', needless to say, raises some further questions, which different nominalists answer in different ways. Chihara thinks *his* answers are better than other nominalists' answers to these further questions, and also better than various anti-nominalists' answers to the original question, including my own.

My solution—I mean 'mine' in the sense that I subscribe to it—runs as follows. If you can't think how we could come justifiably to believe anything implying

(1) There are numbers.

then 'Don't think, look!' Look at how mathematicians come to accept

(2) There are numbers greater than 10^{10} that are prime.

That's how one can come justifiably to believe something implying (1).

When I first began thinking about nominalism twenty-odd years ago, I thought the nominalist would have only two possible replies. The first option would be for the nominalist to maintain that (2) doesn't really imply (1). Since (2) undeniably at least *appears* to imply (1), to take this line would be to commit oneself to the view that there is major difference between what (2) appears to imply and what (2) really does imply. Given the close dependence of implication on meaning, it is hard to see how one could then avoid a commitment to the view that there is a major difference between what (2) appears to mean and what (2) really does mean. And it is hard to see how such a view could be plausibly maintained without offering at least the outline of some positive account of what it is that (2) does mean, if it does not mean that there are numbers, some of which are both greater than 10^{10} and prime. Some descriptive interpretation would have to be offered on this option, which I have elsewhere called the *hermeneutic* alternative.

The second option would be to concede that (2) does imply (1), committing oneself to the claim that belief in (2) is unjustifiable. In that case many further questions would arise, of which I will mention just two. Is the aim of mathematicians, in deciding what results to accept, that of arriving at justified beliefs, or is it something else, perhaps that of devising useful fictions? If mathematicians *are* aiming to arrive at justified beliefs and are failing to do so, should philosophers attempt to get them to recognize their failure and take corrective measures? Different answers to these questions give rise to different suboptions under the second option, in connection with which Rosen and I and others have elsewhere used such terms as *instrumentalist* and *revolutionary* and *alienated*.

As I said above, for many years I thought a nominalist would have to adopt either the first, hermeneutic option, or some suboption under the second option. I was wrong, and for a reason that by hindsight seems obvious: It is perfectly possible for a philosopher to be committed to a disjunction while dodging commitment to either disjunct. I first became aware of this possibility at a public event early in the last decade, where Chihara and I were both speakers. During the discussion period after our talks, I asked him, since as a nominalist he holds (1) to be false, and since the true cannot imply the false, whether his position was that (2) is false, or that (2) does not imply (1). Chihara declined to state an opinion then, and he still declines to state an opinion today: For in his latest book he neither puts forward his interpretation as a hermeneutic account of what (2) really means despite contrary appearances, nor concedes that his nominalism obliges him to deny or doubt (2). I was very surprised when I first encountered Chihara's professed agnosticism about the meaning of ordinary mathematical examples like (2), and am even more surprised now, because Chihara now explicitly claims (p. 249) to be able, through his interpretation, to 'validate our ordinary mathematical reasoning'. It is this ability, he tells us, that distinguishes his work from science fiction.

But contrary to Chihara's claim, it is quite impossible to validate ordinary reasoning without adopting some hypothesis about what ordinary people mean. For reasoning does not consist in emitting certain sounds, but in doing so with a certain meaning attached. The fact that Humpty

Dumpty could attach to the sounds a person emits a meaning that would turn those sounds into valid reasoning is in no way sufficient to establish that the person emitting the sounds is reasoning validly. To bridge the gap one needs the descriptive, hermeneutic assumption to the effect that the hypothetical meaning that would make the reasoning valid is the actual meaning attached by the person in question to the sounds.

7. What Science Teaches Us

Chihara not only rejects the label 'hermeneutic', but also rejects each of the labels 'instrumentalist' (pp. 154–155) and 'revolutionary' (p. 165) and 'alienated' (p. 159). And in the course of arguing why his position should not be called 'alienated', in the sense in which Rosen and I have used the term, he tells us that his nominalism is motivated, not by appeal to some suprascientific philosophy (such as neo-Scholastic metaphysics), but rather by consideration of 'what science teaches us about how we humans obtain knowledge'. Chihara unfortunately does not say just what teaching of science he has in mind. But nominalists typically cite problems about the possibility of *knowledge* of propositions implying the existence of abstracta as a way of side-stepping the issue of the *truth* of such propositions; and it is not Gettierological problems about the gap between justified true belief and knowledge that concern them; so presumably the relevant teaching of science should be something about justified belief. Presumably the alleged teaching of science should be something like this: 'We humans cannot justifiably believe anything implying the existence of abstract objects.' But is this a teaching of *science*, or of a simplistic and Procrustean epistemological theory that may be scienti*st*ic but is far from scienti*f*ic?

Let us consider an example. Chihara seems to hold (pp. 176–177), in common with other nominalists, that expression *types* are objectionably abstract, and he certainly avoids them in his positive project. Now when I opened this review by asserting that there are now four books by Charles Chihara, I clearly did not mean, 'There are now four book *tokens* by Charles Chihara.' For there are not just four but hundreds or thousands of such tokens, scattered through various institutional and personal libraries, including my own; and this is not something that has just now recently become true, but rather is something that has been true for three decades and more, ever since the printing of Chihara [1973]. If you asked me, or a librarian or bookseller, for evidence to justify the belief that there are now four books (*not* in the sense of book *tokens*) by Charles Chihara, we could point to four book tokens, each with the name 'Charles S. Chihara' on the title page, and each, apart from that one common feature, quite unlike every other. Does *science* teach that this is insufficient evidence?

The nominalist who does not wish to stonewall faces the choice between saying that such evidence is insufficient for any assertion implying the existence of books (except in the sense of book tokens), and saying that 'There are now four books by Charles Chihara' doesn't imply that there are such things as books (again except in the sense of book tokens). Likewise, such a nominalist faces the choice between the option of denying or doubting other assertions made in my opening paragraph, assertions made using such words as 'exposition' and 'criticism' and 'nominalism' and 'structuralism', and the option of maintaining that all these assertions, too, are really about tokens or other concrete objects or events. Michael Dummett ([1991], p. 273) writes as follows about a similar paragraph he finds in a newspaper:

> Ordinary literate people readily understand such paragraphs; few would be easily able to render them in words involving reference only to concrete objects, if indeed they can be so rendered, or even to understand such a rendering if presented with it. An ordinary reader's comprehension of the abstract terms does not consist in the grasp of any such procedure of translation, but in a knowledge of how those terms function in sentences . . .

Like Dummett I find implausible the claim that the meaning ordinary literate people attach to something like the newspaper clipping he quotes, or like my opening paragraph, is some complicated nominalistic version mentioning only concrete objects. I find even more implausible the claim that *unless* that is what ordinary people mean, the beliefs ordinary people express in language like that used in such paragraphs are unjustifiable. But I find the claim that *science* teaches that those beliefs are unjustifiable least plausible of all. Dummett has been willing to characterize nominalism as a 'superstition'. I won't go so far as to say that, but I will say that nominalism is no teaching of *science*.

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