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Book Review

Geoffrey Hellman. *Mathematics Without Numbers*. Oxford University Press, Oxford, 1989. ix+154 pages.

1 *Introduction* The present book attempts to arrange a marriage between two traditions in the philosophy of mathematics which have arguably always belonged together. One tradition, now usually termed 'structuralism', is about a century old and originates in Dedekind's 1888 article "Was sind und was sollen die Zahlen" (reprinted in [5]). In this paper, Dedekind argues that the mathematical content of number theory is invariant under transformations defined on its subject matter which preserve arithmetical structure. More generally, and more vaguely, the structuralist view is that mathematics is *about structure*: that the mathematical content of an assertion or theory is invariant under isomorphisms of interpretations of that assertion or theory.

The other tradition, of more recent vintage, is sometimes called 'modalism'. In general terms, this is the view that classical mathematics is (covertly) modal in character; that the language of classical mathematics makes assertions about what *would* hold in any structure of a certain sort, but does not assert the actual existence of any such structure. The view originates, as far as I can tell, with Putnam in [11]. Formulated with reference to set theory, the view is that a statement is equivalent to a modal assertion saying that its first-order representation holds in every possible standard model of the relevant rank (if the statement is of bounded rank; a more complex use of modal notions leads to an interpretation of statements of unbounded rank). The models in question are normally construed as possible *concrete* structures: in Putnam's version, possible physical realizations of certain directed graphs.

The synthesis of these positions leads roughly to the following view. Putnam is clearly not especially interested in physical realizations of graphs in terms, say, of pencil points and arrows. Rather, the thesis is that these are of interest only because they exemplify a certain structure or isomorphism type. The proper formulation of modalism should rather make reference to *all possible realizations whatever* of the relevant isomorphism type. That is to say that the language of classical mathematics

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makes assertions about what would hold in any concrete realization of a certain isomorphism type, but does not assert the actual existence of any such realization. Thus, for example, in the case of set theoretic statements of bounded rank, where α is the relevant bound, the relevant isomorphism type is that of the structure consisting of the sets of rank $< \alpha$ together with the set membership relation restricted to sets of that rank. Hellman's book is by far the most systematic and thorough attempt to spell out the details of this sort of interpretation and to provide a philosophical rationale for it.

My discussion will be structured as follows. I will first sketch Hellman's modal-structural interpretations of number theory, analysis, and set theory. I will then describe some technical problems which arise for these constructions. The problems concern the interpretation of the modality employed and the status of certain second-order devices appropriated by the constructions. I will then discuss a number of advantages claimed by Hellman and others for the modal-structuralist view and argue that in each case the putative advantages have been exaggerated. I then turn to a sketch of Hellman's account of applied mathematics. I shall argue that that account is incompatible with a broadly realistic view of the nature of scientific explanation and is of limited applicability in the context of an antirealist view, though I shall state and prove a general sufficient condition for its positive applicability. I will conclude by sketching an alternative interpretation of the structuralist thesis, and by pointing to some general limitations of the structuralist conception of mathematical truth.

2 A modal-structuralist primer

2.1 Elementary number theory Let us begin with the language $\mathcal{L}$ of elementary number theory. The idea is to construct a translation scheme that maps any first-order number-theoretic sentence onto a modal assertion saying that the sentence holds in any logically possible standard model of elementary number theory. For our target language we take a second-order modal language with 2-*ary* function variables and monadic (class) variables. Let **Q** be the set of axioms of Robinson's arithmetic and let **IND** be the second-order induction axiom

$$(\forall X)((X0 \wedge (\forall y)(Xy \rightarrow Xy+1)) \rightarrow (\forall y)Xy).$$

Define

$$\mathbf{AX} \longleftrightarrow (\bigwedge \mathbf{Q} \ \wedge \ \mathbf{IND}).$$

Given a statement φ of $\mathcal{L}$, at a first approximation the translation of φ will say that

$$\Box(\mathbf{AX} \rightarrow \varphi), \tag{1}$$

where '$\Box$' signifies logical necessity.

The difficulty with this translation is that the primitive number-theoretic devices occur in it schematically; they are provided with no interpretation. What we want to say is that φ holds in *any possible interpretation* of the arithmetical primitives satisfying **AX**. To this end, we introduce quantifiers into the relevant positions, so that in place of (1) we obtain

$$\Box\ (\forall P)\ (\forall f)\ (\forall g)\ (\forall x)\ (\forall y)\ (\mathbf{AX}^p)(f/+, g/\cdot, x/0, y/1) \\ \rightarrow\ \varphi^p\ (f/+, g/\cdot, x/0, y/1)), \tag{2}$$

where 'f' and 'g' are 2-place function variables, 'P' is a monadic set variable, and for any sentence A, A^p indicates the relativization of quantifiers in A to P. Thus, under its modal-structural translation, a sentence of the language of elementary number theory is true if and only if it holds in all possible standard models of arithmetic, for Robinson's arithmetic, in conjunction with the second-order induction axiom, characterizes the standard models categorically.

2.2 Das Kontinuum As a second example, we consider a modal structural interpretation of classical analysis. Let **OF** be the first-order theory of ordered fields. Define

$$Bd^X\ (y) \longleftrightarrow (\exists z)\ Xz \wedge (\forall z)\ (Xz \rightarrow z \leq y),$$

that is, x is nonempty and y bounds each member of X from above; and let LUB be the statement

$$(\forall X)\ (\forall y)\ (Bd^X(y) \rightarrow (\exists z)\ (Bd^X(z) \wedge (\forall w)\ (Bd^X(w) \rightarrow z \leq w))),$$

which asserts the existence for each nonempty bounded set of real numbers of a least upper bound. The theory

$$\Re\ =\ \mathbf{OF}\ \cup\ \{LUB\}$$

characterizes the standard models of analysis categorically. If φ is a sentence of analysis, then a formula analogous to (2), with $\bigwedge \Re$ in place of **AX**, says that φ holds in all possible standard models. It is this formula which the modal-structuralist will take as giving the truth condition of φ.

***2.3* ZF** Here we distinguish statements of bounded from statements of unbounded rank. Let α be a regular cardinal. A statement of $\mathcal{L}_{ZF}$ is said to be bounded of rank α if quantifiers of the statement are restricted to sets of rank $< \alpha$. The idea is to say that a statement φ of rank α is true if φ holds in all possible standard interpretations of rank α.

To implement this idea, let $\mathbf{ZF}^2$ be the axioms of **ZF** minus the instances of the replacement schema, together with the second-order replacement axiom

$$(\forall R)\ ((\forall x)\ (\exists ! y)\ Rxy \rightarrow (\forall z)\ (\exists w)\ (\forall u)\ (u \in w \longleftrightarrow (\exists v \in z)\ Rvu))).$$

$\mathbf{ZF}^2$ is 'quasi-categorical' in the sense that for any two models of $\mathbf{ZF}^2$, one is an end-extension of the other. In fact, we have that if $\Im \models \mathbf{ZF}^2$, then $\Im$ is isomorphic to V_α for some inaccessible cardinal α. As restricted to sets of rank $< \alpha$, an arbitrary statement φ of $\mathcal{L}_{ZF}$ may be accorded the following structuralist truth-condition:

$$\Box(\forall P)\ (\forall R)\ ((\wedge \mathbf{ZF}^2 \wedge A_\alpha)^P\ (R/\in) \rightarrow \varphi^P\ (R/\in)),$$

where A_α is a sentence that holds in any standard model M if and only if M is of rank α. Thus we have said that φ holds in each possible standard model of rank α.

Let us now turn to the problem of interpreting set theoretic statements of unbounded rank. The idea, going back to Putnam, is that if φ is a statement of $\mathcal{L}_{ZF}$ in prenex form, for example, the formula

$$(\forall x)\ (\exists y)\ (\forall z)\ A, \tag{3}$$

where A is quantifier-free, then φ asserts the following modal-structural content: for any possible standard model M_1, and x in $|M_1|$, there exists an extension M_2 of M_1, and a y in $|M_2|$ such that for each possible extension M_3 of M_2 and z in $|M_3|$, the statement $Axyz$ holds in M_3. Formally, then, (1) may be written

$$\Box(\forall M_1)\ (\forall x \textit{ in } M_1)\ (M_1 \models \wedge \mathbf{ZF}^2 \to \Diamond\ (\exists M_2)\ (\exists y \textit{ in } M_2)\ (M_1 \leq M_2 \wedge M_2 \models \mathbf{ZF}^2 \wedge \Box(\forall M_3)\ (\forall z \textit{ in } M_3)\ ((M_2 \leq M_3 \wedge M_3 \models \mathbf{ZF}^2 \to M_3 \models Axyz))).^1$$

3 Some technical problems with modal-structuralism This completes my outline of Hellman's modal structural construal of classical mathematics. Two technical questions of interpretation that immediately arise concern the ontological commitments of the second-order notions and the interpretation of the modality employed. Let us consider these in turn.

3.1 Interpreting second-order quantifiers Why is this a problem? On the standard Tarskian-Platonist interpretation, second-order quantifiers range over the full powerset of the relevant first-order domain. If these sets are just special objects within the relevant possible situations, one wants to know why the operation of set-formation does not apply iteratively in each such situation in such a way as to generate the usual hierarchy. Even if such iteration is not allowed, however, it would seem in any case that on the standard interpretation of the second-order quantifiers, the generality of the structuralist interpretation will be compromised: there will be a residue of unreduced mathematical objects; and so Hellman explores the possibility of alternative, nominalistic interpretations.

Hellman's proposal is that the second-order quantifiers be interpreted as quantifiers over sums, in Goodman's sense, of individuals. I do not think this suggestion comports very well with the intended reading of '$\Box$' as narrowly logical necessity; for, on Goodman's view, sums of individuals are themselves a special sort of individual, and quantifiers over them are ordinary *first-order* quantifiers; the predicative notation 'Xa' would on this reading involve a suppressed mereological relation: it would mean that 'X' signifies an aggregate and 'a' an individual which is *part of* that aggregate. However, if, as on several of the translation schemes above, a sentence φ is mapped onto a sentence of the form $\Box\varphi^*$, and φ^* is first-order, φ will be counted as true if and only if φ^* holds in all first-order interpretations. By the completeness theorem for first-order logic, then, the truth predicate for the relevant language would be Σ_1, which by Tarski's theorem is false for any language containing elementary number theory. One solution to this problem would be to hold the interpretation of the first-order theory of sums fixed; in this case, the intended reading of '$\Box$' would apparently have to be modified. Another would be to interpret the second-order quantifiers involved in Hellman's construction as genuinely plural quantifiers. Boolos [3] has argued that if the quantifiers are so interpreted, they are genuinely second-order and yet free of ontological commitment to sets. If that is right, and the plural quantifiers are genuinely logical devices, then the interpretation of second-order quantifiers can be harmonized with the intended reading of '$\Box$'.

However, both the logic-of-sums idea and the plurality conception afford only an interpretation of *monadic* second-order quantification: there is still a problem about interpreting *polyadic* second-order quantification, of which essential use is made in all of the indicated translations. Hellman suggests that we introduce a primitive notion of pairing to reduce the polyadic case to the monadic one (p. 50); in place of quantification over dyadic relations, for example, we would have monadic quantification pluralities of pairs. Hellman's idea is that the notion of pairing should be *structurally* interpreted, subject only to the requirement that, where P is the pairing functor, the condition

$$P(x, y) = P(x', y') \longleftrightarrow x = x' \wedge y = y', \qquad (*)$$

hold for each x, y, x',y'.[2] In different contexts, different individuals will play the role of 'pairs' and different relations between objects and 'pairs' will constitute the pairing relation.

I do not believe that the present suggestion solves the problem. The difficulty is that the pairing functor is still *uninterpreted*; the principle $(*)$ constrains any acceptable interpretation of 'P' but it does not itself provide such an interpretation. Thus deployed, 'P' occurs schematically in constructions involving the notion of pairing. The difficulty is essentially the same as with the proposal that the modal translate of an arithmetical statement be taken to be the necessitation of the conditional whose antecedent consists of the conjunction of the axioms of second-order arithmetic and whose consequent is that statement itself. In this translate, the arithmetical primitives occur schematically and are provided with no interpretation (though the number-theoretic axioms constrain any acceptable interpretation). The remedy adopted by Hellman in this case is to introduce quantification over functions or relations into the positions occupied by the arithmetical primitives; that gives just the pattern of translation considered above. Notice, however, that the corresponding solution for the pairing function is unavailable in this context, for it would make ineliminable use of dyadic second-order function quantification into the position occupied by the pairing functor. It is just this sort of quantification that needs to be explicated. In the end I believe that Hellman is left without any acceptable construction of polyadic second-order quantification. One suggestion worth exploring, which I shall only hint at here, is to appeal to another sort of interpretation of second-order logic, this time in terms of generalized first-order quantifiers. It has been known for some time that the addition to elementary logic of branching quantifiers such as

$$\begin{pmatrix} \forall x \, \exists \alpha \\ \forall y \, \exists \beta \end{pmatrix},$$

interpreted in the manner described by Henkin [5], considerably enhances its expressive power. In fact, it may be shown that if one allows n-fold branching for any n and n $\forall$-quantified variables in the prefix, the resulting branching logic is equivalent to a strong fragment of second-order logic, sufficient to phrase the translations considered above, and in particular to express second-order replacement. If one could argue that branching quantifiers of this sort are genuinely first-order devices, free of reference to sets or functions, it might be argued that these devices could enable Hellman to produce a simulacrum of a fragment of second-order logic adequate for his purposes; but the ontological issues here have not been sorted out.

3.2 Interpreting modality Hellman repeatedly stresses that the modality relevant to modal-structural interpretations is a "logico-mathematical" one (pp. 8, 15, 17, 28, 36, and 59), but the nature of that modality is never clearly defined. It seems to be Hellman's intention that it be understood primitively, that is, without explanation in terms of more fundamental (or at least other) notions, but I do not believe it is at all clear that we *do* understand it primitively. The modality in question is a rather artifactual notion equivalent, in application to nonmodal sentences, to the second-order validity concept. Some sort of explanation of it is at least highly desirable, either in terms of a reduction to other notions which are plausibly understood independently of platonistic commitments or, alternatively, a direct explanation of *what it is* to understand it primitively. I see no way of implementing the latter strategy; and so I shall consider the former.

We have a notion of necessity, variously called 'metaphysical' or 'broadly logical' or sometimes 'real' necessity, which might be pressed into service here. This cannot be quite the right notion to directly model the required modality, however, for on this reading of '$\Box$' it is necessary that water is H_2O. This identity would not seem to be a 'logico-mathematical' one. We perhaps also have a notion of a proposition's being a priori, or *epistemically necessary*, but this cannot be what is intended either. A sentence of second-order Peano arithmetic, for example, would on the modal-structural interpretation derived from this reading of '$\Box$' say roughly that one can know a priori that the sentence is true under every possible interpretation rendering the axioms of second-order number theory true; but there is no reason to suppose that this is the case in general. Rather, the intended doctrine is the realist one that the sentence may be true independently of our ability to know that it is true, even given knowledge of the second-order axioms.

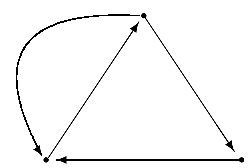
Historically, the possibilities for interpreting the notion of strictly logical necessity divide into proof-theoretic and modal-theoretic interpretations. On a proof-theoretic interpretation, a statement of the form $\Box\varphi$ will be counted as true if and only if the embedded statement φ is provable in a certain formal theory, but a modal-structuralist interpretation based on a notion of provability runs afoul of some fairly obvious limitative observations. First, if the proof predicate is primitive recursive (as it will be if "provability" refers to provability in a fixed formal theory), the relevant provability predicate is Σ_1 and in this case by Tarski's theorem even Hellman's interpretation of elementary number theory will fail to fix the correct truth-values.[3]

Opposed to syntactic characterizations of logical necessity there stand model-theoretic ones. The appropriate characterization for the present context—the one with a clear claim to capture the distinctively *logical* notion of necessity—is that of the second-order model-theoretic validity concept. That is to say, we treat as 'worlds' all structures for, say, $\mathcal{L}_{ZF}$ and provide that a sentence of the form $\Box\varphi$ holds at a world if and only if φ holds in all such structures. It is clear, of course, that this characterization is unavailable in the context of Hellman's enterprise, since it makes essential use of quantification over arbitrary sets.

However, there is a possible solution to this problem which seems quite in the spirit of Hellman's project. The remedy makes use of the idea of a *representation* of a structure (say, a standard model of **ZF**). If M is such a model, a representation of M is a concrete realization of M in a possible counterfactual situation. Thus, for example, Putnam's original modal construction of **ZF** made use of the idea of a 'concrete standard model of **ZF**', construed as a concrete directed graph in some possible configuration of space-time. To articulate this idea, it *is* natural to make use of a primitively understood notion of metaphysical possibility. Note that the objection made above to the use of this notion to directly model the concept of logical necessity does not arise here. The problem there was that there seem to be 'logico–mathematical' possibilities which are not metaphysical possibilities. On the present suggestion, our account of logical necessity is the orthodox model-theoretic one, save that the role of abstract structures in the model-theoretic characterization is played instead by their possible concrete representations. Thus, for example, if 'P' signifies the parent-of relation, the sentence

$$(\exists x)\ (\forall y)\ (yPx \to xPy),$$

though arguably not metaphysically possible, is rendered logically possible by concrete realizations of the graph



wherein the relation P is represented by directed connection.

I believe that the present suggestion affords an adequate basis for an interpretation of the required modality if there are *sufficiently many possible* concrete representations. In order to achieve a satisfactory modal version of the mathematical theory of an isomorphism type, we will require at least one possible concrete realization of that type. In particular, as applied to **ZF** under Putnam's scheme, the assumption requires that for each regular limit cardinal α there is a possible concrete graph isomorphic to V_α.

This assumption, explicit in Putnam's version of the modal interpretation of **ZF**, has been criticized by Parsons and myself on rather similar grounds (see [9], n. 23; [10], n. 33). The assumption that the required representations are *concrete* is most naturally interpreted as saying that any V_α is exemplified in some possible configuration of space-time. What reason is there to suppose that this assumption is satisfied? Putnam writes:

> In order to "concretize" the notion of a model, let us think of a model as a graph. The "sets" of the model will then be pencil points (or some higher dimensional analogue of pencil points, in the case of models of large cardinality) and the relation of membership will be indicated by "arrows". (*I assume that there is nothing inconceivable about the idea of a physical space of arbitrarily high cardinality; so that models of this kind need not necessarily be denumerable, and may even be standard.*)[4]

The difficulty is that it is far from clear that the conceivability of such a state of affairs ensures its *possibility* in the required sense. Conceivability is, presumably, an epistemic modality, dual to the epistemic notion of necessity: to say that φ is conceivable is to say that $\neg\varphi$ is not epistemically necessary. For reasons which are now familiar, the conceivability in this sense of a configuration of the world does *not* insure its possibility (see Kripke [8], Lecture I). If the relation of concrete realization implicitly refers to possible configurations of actual space-time and actual space-time is treated substantivally, that is, as an extensive individual, it is at least not implausible that the cardinality of space-time is one of its essential properties. In that case, there is no possible configuration of the world in which space-time has a cardinality other than its actual cardinality, notwithstanding the fact that configurations of space-time of varying cardinality are *epistemically* possible. Unfortunately, in that case there are $V_\alpha s$ for which there exist no possible concrete realization.

In the final chapter of the book, Hellman considers the possibility of relaxing the assumption that the possible structures considered in a modal interpretation of, say, **ZF**, are 'concrete' in the above sense:

> If, in fact, as it presently appears, $\mathbf{RA}^2$ [second-order analysis] is not adequate [for all purposes of physical theory], then even modal nominalism in this sense is doomed. It seems likely that structures for even richer theories need to be entertained, and with these we will have transcended what can even be conceived as part of space-time as we understand it. (p. 116)
>
> Structures beyond $\mathbf{RA}^2$ may not qualify as nominalistic, but we may still entertain such structures hypothetically, dropping any claim to "grasp them" by means of "geometric intuitions". (p. 117)

The suggestion then, seems to be that, contra Putnam, the possible models for, say, **ZF** should be conceived as non-spatio-temporal and, for reasons taken up below, as causally inert.[5] This proposal raises a number of interesting questions; let me call attention to just one difficulty I find with it.

An object is abstract if it is not in space and time and it does not participate in causal relations. Hellman's suggestion would seem to require that the objects in the structures entertained as models of **ZF** be conceived as *abstract* objects. It has sometimes been maintained that if such an object exists in a metaphysically possible situation w, then the propositon that it exists is (metaphysically) necessary in w. If the metaphysical necessity concept respects the modal logic **S5**, it follows that the object in question necessarily, and thus actually, exists. If this reasoning is sound also with respect to the intended logical modality, then the actual existence of the relevant structures would be ensured by their logically possible existence.

It is difficult to see what it is about the indicated argument, assuming it to be sound for the metaphysical concept, that would prevent it from applying to the logical modality considered by Hellman. Indeed, if the suggested construction of logical in terms of metaphysical possibilities is right, the argument *must* apply to the logical modalities; and so the response must either be to deploy an alternative interpretation of the notion of logical necessity and to argue that on this interpretation the purported necessitation principle fails, or to attack the argument for the metaphysical case directly. Neither strategy is attempted. In general terms, the present suggestion seems to be at odds with what has normally been taken to be an important part of the philosophical motivation for the modal conception of mathematics. As Putnam put it, "the conception of mathematics as the study of special objects has a certain implausibility that the conception of mathematics as the study of ordinary objects with the aid of a special concept does not." ([11], p. 57)

On the other hand, it might be suggested that the *epistemic* notion of possibility be substituted for the metaphysical one in carrying out the modal implementation of the model-theoretic framework sketched above. However, the epistemic notion would appear to be too broad for this purpose. We require the relevant possible concrete realizations of $\mathbf{ZF}^2$ to be *standard* models. If φ is a sentence such that $\mathbf{ZF}^2 \cup \{\neg\varphi\}$ is epistemically possible but which holds in all standard models, then any epistemically possible realization of $\mathbf{ZF}^2 \cup \{\neg\varphi\}$ will be *nonstandard*. It is very plausible that such propositions can be found: the epistemic necessity concept applies to a statement relative to $\mathbf{ZF}^2$ only if that statement is, in a more or less strong sense, *a priori relative to* $\mathbf{ZF}^2$, and so the indicated assumption is simply that not all standardly true propositions can be inferred from $\mathbf{ZF}^2$ a priori.

In sum, the notion of logical necessity is a modal analogue of the second-order validity concept. This notion might be explicated either syntactically or model-theoretically. Syntactic interpretations do not yield sound modal translation schemes even for the language of elementary number theory, and model-theoretic ones are at face value encumbered by ontological commitments of just the sort the modal translations are designed to avoid. Modal implementations of the model-theoretic idea suffer from the following dilemma: either the modality employed is *real* (metaphysical) or *ideal* (epistemic). If it is *real*, it is not clear that there are enough possible structures to represent the standard models of, say, **ZF**; if it is *ideal*, there are too many possible realizations of $\mathbf{ZF}^2$ to weed out the nonstandard models.

4 Putative advantages of modal-structuralist interpretations I shall consider the following claims: (1) modal-structuralist interpretations are superior to platonist ones on general semantical and epistemological grounds; (2) they are superior to platonist interpretations of set-theory in enabling a convincing resolution of the paradoxes and in motivating various higher axioms of infinity. Both sorts of claim have been discussed previously in the literature, and both are discussed in Hellman's book. My claims will be that, first, the adjudged advantages of modal-structural interpretations have been exaggerated—as regards both general semantical and epistemological questions and special problems arising in the case of set theory—and, secondly, that modal-structural interpretations require a seriously revisionist stance toward our understanding of classical mathematics.

4.1 Argumenta Benacerrafa There is an attractive argument for the modal-structuralist view stemming from two seminal papers by Benacerraf (see [1], [2]). Both papers raise problems for the Tarskian-Platonist interpretation of classical mathematics, the first on semantical and the second on both semantical and epistemological grounds. In "What numbers could not be," Benacerraf presents Dedekind's point in semantical guise. Benacerraf argues that there is no fact of the matter to settle which of the multiple set theoretic interpretations of arithmetic is the right interpretation, since any interpretation that exemplifies the proper isomorphism type will be as mathematically correct as any other. The later paper "Mathematical truth," on the other hand, stresses problems about knowledge and reference stemming from the ontological status of mathematical objects. The acausal character of the objects of classical mathematics has tended to obstruct philosophical attempts to conceive a workable form either for theory of mathematical knowledge or for a theory of mathematical reference or representation. The problem is explicit if it is assumed that cognitive and referential relations are in some sense causal relations, and this is of course an assumption which has figured prominently in much recent theorizing about knowledge and representation.

The modal-structuralist conception of classical mathematics seems to offer a means of addressing both of Benacerraf's problems. On that conception, a statement of arithmetic, for example, is accorded an explicitly structuralist truth condition, being counted true if and only if it holds in any possible standard model. The relevance of such a construction to Benacerraf's first problem is evident, for it avoids the apparently arbitrary identification of one standard model as the intended interpretation of arithmetic. Furthermore, since the *objects* of classical mathematics on the modal-structuralist view are (possible) concrete particulars, problems stemming from causal theories of knowledge and representation may perhaps also be eased or at least usefully recast. Let us consider these points in turn.

As we have seen above, the semantical status of the relevant modality is seriously unclear; that being the case, it is not surprising that its epistemology is similarly unclear. In one sort of standard case, there is a second-order categorical axiomatization (**AX**) such that any sentence φ in the relevant language goes over roughly into the claim that φ holds in any possible realization of **AX**. This suggests that the epistemology of the relevant fragment of classical mathematics is no worse off than that of the second-order consequence relation: to discover which sentences hold in the relevant isomorphism type, one investigates what the second-order consequences of **AX** are. However, the second-order consequence relation presents problems of interpretation within the modal-structuralist setting comparable to set theory itself, and in any case we must be able to justify the claim that realizations of **AX** are, in the required sense, *possible*. If we work with the idea that mathematical possibilities are to be recovered from metaphysical ones in the manner suggested above, what needs to be shown is that **AX** holds in some metaphysically possible structure. I suggested before that it is questionable that the required possible realizations can be found; but there is the further question of how one can know that they can if they can. Furthermore, even if the required possibility claim can be justified, its justification may require nonmathematical premises. On the suggested interpretation of logical possibility, on modestly essentialist principles, it is a matter of substantive physical theory, in particular, of our theory of both the fine and the large scale structure of space-time, whether there exist possible standard models of analysis or **ZF**. Although the exact epistemological status of these theories is controversial, it seems implausible that considerations of *this* sort are required for their justification.

4.2 Problems of logical form On what I would take to be the most plausible account of the modality required for the modal-structural interpretation, then, we may have to seriously revise our conception of the epistemological status of classical mathematics. However, the modal-structural interpretation also generates distortions in semantic content which are largely independent of the nature of the modality employed. Benacerraf was aware that the language of classical mathematics appears to refer to a determinate interpretation; indeed, Benacerraf simply assumed that a Tarskian interpretation was the only semantic description that captures what we are saying when we employ that language. Consider, for example, the following assertions:

(1) there are two recursively ennumerable sets of incomparable degrees of unsolvability;
(2) the zeros of the Riemann zeta function in the critical strip all have the same real part.

One way of motivating the Tarskian interpretation of classical mathematics is to compare (1) and (2) with two assertions which clearly require such an interpretation and which appear to have a similar structure; for example,

(1*) there are two logic students at the dissertation stage neither of whose advisers have heard of the other;
(2*) the dissertation students in the philosophy department all have the same adviser.

It seems very implausible that the respective pairs of sentences do not exemplify a common structure, but the advocate of the modal-structuralist view appears to be committed to just this claim. The quantificational devices appearing in the respective pairs of sentences appear to function in the same way, but according to the modal interpretation this is simply an illusion.

This divided policy seems objectionable on general methodological grounds; and it leads to trouble: the modal-structuralist account misses certain entailment relations in which mathematical notions figure essentially. Consider, for example, the following inference.

(a) There are infinitely many primes.
(b) There are only finitely many persons located in the city of Los Angeles.

(c) There are infinitely many things which are not persons located in the city of Los Angeles.

By use of the generalized quantifier Q_0, "for infinitely many," we might give something like the following representation of this inference.

(a*) $(Q_0x)(Nx \wedge (\forall y)(\forall z)((y > 1 \wedge z > 1) \rightarrow \neg\mathrm{Prod}(x, y, z))$
(b*) $\neg(Q_0x)(Px \wedge \mathrm{Loc}(x, \alpha))$

(c*) $(Q_0x)\neg(\mathrm{Loc}(x, \alpha) \wedge Px))$.

On this representation, the inference is model-theoretically valid. That is as it should be, for the inference is indeed valid. The difficulty is that it is not so construed on its modal-structural representation. It would have been if (b) and (c) were to be modalized in the same way as the modal translate of (a), but that would be absurd: (b) does not claim that in any possible situation containing a standard model of arithmetic, say, only finitely many people can be found in the city of Los Angeles, but that there are actually only finitely many such inhabitants.[6]

I believe that considerations such as these somewhat undermine the claim of the modal-structural interpretation to constitute an accurate description of what our mathematical discourse literally means. At the very least, the modal-structuralist view leads us to assign very different structures to sentences which on general semantical grounds one would expect to exemplify similar structures, and it systematically distorts certain entailment relations. In the end, the modal-structuralist should probably say that he is not really trying to frame a description of the language of mathematics that completely captures what we take it to say. Rather, the modal-structuralist's claim seems to be that our theoretical purposes in speaking that language are to some extent *subverted* by what we literally mean, and that these purposes would be better served if we meant something else instead. According to the modal-structuralist, a fragment of classical mathematics should be taken to describe invariants of a class of (possible) structures, and that isolating a single representative of that class as the intended interpretation of the fragment is neither required for, nor conducive to, this end. The success of a reconstructive enterprise based on this picture is to be judged in terms of its consonance with the aims of mathematical practice and its success in giving an account of the application of mathematics to the nonmathematical realm. Both of these dimensions of appraisal are touched on below.

4.3 Higher axioms of infinity and limitation of size Beginning with Putnam ([11], p. 58), advocates of modal translations of set theory have wanted to claim that the modal view affords a satisfactory resolution of the paradoxes and a convincing justification of certain large cardinal axioms. The idea is that it should be *possible* to extend any concrete standard interpretation of **ZF** to another such interpretation in which additional ranks are represented. The bearing of this idea the modal version of certain large cardinal axioms is evident; in the presence of the assumption that there exists a possible standard model, it leads, for example, to a justification for the axiom of inaccessible cardinals and, suitably ramified, for other large cardinal axioms as well. The same idea can be used to motivate the thesis that the classes comprising all sets, all ordinals, or all cardinals are not sets.

It seems to me that the advantages of the modal conception in dealing with the limitation of size problem, on the one hand, and the reciprocal problem of motivating large cardinals on the other, have been exaggerated by its advocates. I noted above that it is not clear how the required modal existence assumptions are to be justified: let us grant that problem and set it aside. In the best possible case, for each V_α, α inaccessible, there will be a possible concrete standard model isomorphic to V_α. In this case, it seems to me, there will exist a natural correspondence between the modal existence assumptions required to motivate a modalized axiom of infinity and literal existence assumptions required to motivate the corresponding platonist version.

Consider, for example, the suggested justification of the modalized axiom of inaccessibles. The idea was that, given any possible concrete standard model of **ZF**, it should be *possible* to extend that model by adding additional ranks. I do not know whether this justification for the modal axiom should be considered successful or not, but to the extent that it is convincing, using ordinary principles of set theory, it can be translated into a justification for the corresponding unmodalized axiom by appealing to an assumption implicit in both Hellman's discussion and in Putnam's. The assumption is that the collection of concrete individuals existing in any single possible situation forms a set. This assumption is equivalent to the condition that any fixed possible space-time is of bounded cardinality (even if there exist possible space-times of arbitrarily large cardinality). The assumption is integral to the whole modal-structural approach to motivating large cardinal axioms: without it, there could be no reason why one should not entertain the possibility of a world with a domain of concrete individuals maximal with respect to inclusion or cardinality, which would of course contradict the modal extendability assumption alluded to above.

Suppose, then, that this assumption is satisfied, and that we have succeeded in motivating a modalized axiom of infinity by arguing that there is a possible situation w in which a cardinal of the relevant type $T(x)$ has a concrete realization. By the assumption, the collection $n(w)$ of all concrete individuals in w is a set, whence within **ZF** supplemented with the relevant modal claims we can establish the existence of a cardinal number $\lambda(w)$ of $\Omega(w)$. (The argument is the familiar one: using replacement and choice, we obtain an ordinal α isomorphic to a well ordering of $\Omega(w)$; $\lambda(w)$ is then the smallest ordinal similar to α.) Then $\lambda(w)$ bounds all cardinals represented in w, so that if a cardinal of type T is represented in w, then $(\exists x)T(x)$ holds in $V_{\lambda(w)}$. In other words, modulo the indicated assumption, the *ordinary* principles of set theory applied in the relevant modal context, allow us to lift any reason for supposing that there is a possible concrete representation of a certain cardinal to an argument for its actual existence.[7]

The other problem concerns limitation of size. Classically, the question is why certain large totalities (such as the collection of all sets, or all ordinals) do not form sets. Of course, the **ZF** axioms imply that these totalities do not form sets; but this has seemed to some not to provide a satisfactory explanation of why such totalities are not *setlike*. The feeling seems to be that sets are totalities formed by assembling well-defined individuals in well-defined ways. The property of being a *set* itself is, in the Cantorian view of the world, a well-defined attribute: it is determinate of any possible object whether or not it is a set. The question is why sets do not form a setlike totality; and if, as Boolos has claimed, set theory is our overall theory of all the setlike objects there are, it has seemed to some to be unclear, notwithstanding the **ZF** axioms, why that totality does not form a *set*. Some sort of explanation is called for, and the modal-structuralist purports to provide one, in terms of the modal extendability principle. As Putnam put it, even God could not create a possible standard model of **ZF** which it would be *mathematically impossible* to extend.

As I noted above, the extendability assumption requires the condition that the domains of the possible worlds are themselves setlike, at least as restricted to concrete objects, and this is an assumption which has seemed to many to be quite plausible; but I do not think it is in the end any more plausible than the corresponding classical limitation of size principle. Argue, in analogy to the previous paragraph, as follows. Let us think of the concrete standard models of **ZF**, à la Putnam, as being made up of possible pencil points (or their higher-dimensional analogues) and "arrows." It is not clear, given the determinacy of the relevant concrete objects, why these cannot be assembled together in some single possible situation. That is to say, given a class (not necessarily a set!) of worlds, why shouldn't there exist a world whose domain comprises copies of the concrete particulars from all of the domains of the given worlds?

It seems to me that the force of this question, to the extent that it has any, is about the same as that of the corresponding question about sets, and that answering it will require essentially the same sort of reasoning. Instead of talking about extendability in the cumulative hierarchy, one will talk about extensions of domains of worlds. In both cases, there is an amalgamation principle that has an intuitive hold on us; and it is rather plausible that an argument that defeats one would suggest one that defeats the other.

Let us sum up: I have argued that the modal-logical justifications for modalized large cardinal principles can be translated, assuming the indicated limitation of size principle for domains of worlds and the *ordinary* principles of set theory, into set theoretic justifications for the platonist versions of those principles. I have further suggested that the problems affecting the justification of limitation of size principles in set theory have counterparts in problems affecting the rationale for corresponding principles for domains of worlds.

5 Applied mathematics

5.1 Measurement and modality I now turn to Hellman's account of applied mathematics within the modal-structuralist framework. That account is contained in the final chapter of the book and raises some interesting metaphysical issues. The difficulty is that the previous translation pattern will not work as applied, for example, to a measurement context such as

(1) The length of a is n centimeters,

for on that pattern (1) would say that x has length n centimeters in each possible situation containing a standard model of arithmetic or analysis; but there presumably are such possible situations in which a has a length other than n; claims of this sort would thus never be counted as true. To avoid this outcome, we shall have to assume that the deviations from actuality which generate the relevant possible situations leave the actual measure of a fixed. Generalizing on this theme, Hellman is led to a constraint he terms the *non-interference proviso*, that the possible worlds considered be restricted to those which contain copies of the actual physical world (or the relevant part of it) in which actual objects have all of their actual physical properties. The 'mathematical' objects in these possible worlds, then, must be conceived to be causally inert at least with respect to actual events and objects.

Different questions about the non-interference proviso arise from different metaphysical orientations in the philosophy of science. In the first place, the suggestion seems to be committed to the existence of a well-defined physical configuration of the world that obtains objectively and independently of mathematical characterization. From an antirealist point of view, it can be questioned whether the notion of such a configuration is intelligible at all, whether or not it essentially involves a mathematical architecture. Hellman takes this antirealist worry to constitute the most serious challenge to his view of applied mathematics; for the idea here is to modify the modality in the original translation scheme to restrict the possible worlds considered to those which incorporate the actual physical configuration of the world. If the idea of such a configuration is unintelligible, then so too is the suggested modification.

However, even on what deserves to be called a ‘realist’ interpretation of scientific theories, Hellman’s proposal is not entirely unproblematic; for it is possible to question whether the actual physical configuration of the world obtains independently of the mathematical facts. The question is whether the instantiation of a physical state by a system might *consist in* the correctness of a mathematical description of the system. If so, and if reference to mathematical facts is handled as in the original translation scheme, the whole problem at issue will arise again: for there would be no assurance that actual objects retain their actual physical properties in the possible worlds invoked to model the relevant mathematical structures.

The present worry does not concern a requirement that numerically specified magnitude-properties be held fixed from world to world. Hellman is surely on strong ground in supposing that the property we are ascribing to an object in attributing a magnitude to it does not involve reference to any particular *number*. The realist idea is that there are properties or states which constitute, for example, the mass or energy content of an object in the actual world, which is what we are attributing to the object when we describe it as having a mass of X grams or an energy of Y electron volts; but the same property could be specified in various other ways. The question I wish to raise is rather whether the exemplification of even such an ‘intrinsic’ magnitude-property might be a partly mathematical matter.

Let us consider what is involved in ascribing such a property to an object. In general, there will be a set of constraints imposed on the acceptable representing functions for the magnitude in question by a substantive theory of that magnitude, and one shows that, to within a certain equivalence relation (for example, uniqueness up to a scalar transformation) there is just one function defined for all causally possible systems of the relevant sort satisfying these constraints. Call such a function *admissible* for the magnitude. As far as I can see, we have no way, in general, of specifying the relevant intrinsic properties, and thus of characterizing the required modality, without reference to the relevant collection of representing functions; and these functions may be constrained in mathematically nontrivial ways. In this case, our conception of *what it is* for an object to fall under such a property may be bound up with specific mathematical commitments.

In general, it is possible for a realist to maintain that there is an objective physical architecture which underlies a metrical characterization of a system, but that this architecture essentially involves mathematical facts of one sort or another. The assumption required for the deployment of Hellman’s modality, then, is not simply *realism* with respect to the physical states of the systems in question, but realism *in combination with* a nominalist thesis about what it is for a system to instantiate such a state. However, no justification for such a thesis has been provided.

5.2 Explanation and realism Even if we help ourselves to the relevant modality, it is not clear that, on broadly realist assumptions about explanation, the replacement of a piece of applied mathematics with its modal-structuralist counterpart preserves explanatory relations. Let N represent the modality in question and H the conjunction of a set of statements involving the axioms of number theory or analysis which, standardly construed, explain a nonmathematical sentence P. If the transition to the modal-structuralist interpretation is to preserve explanatory relations, it must be the case that the sentence NH explains P. The sentence NH says roughly that the hypothesis H obtains in each superstructure of the actual world containing a standard model of second-order analysis (say). Thus we can explain, *in such a world*, why P is true; but what does this circumstance have to do with our ability to explain why P obtains *in the actual world*?

The question can perhaps be put into sharper focus by exhibiting it as an instance of a more general situation. This time let H represent an arbitrary hypothesis which, if true, *would* explain P; but suppose that H is in fact false. In analogy to N, consider a modality M that applies to a sentence S to yield a statement MS which is true if and only if S obtains in each superstructure of the actual world in which H obtains. Thus the sentence MH is true; but it is quite unexplanatory of the actual truth of P. The reason, of course, is that H is in fact false: even though we can explain P in terms of H in any situation in which H holds, this fact seems quite irrelevant to explaining P in situations in which H fails to hold. The modality N above is the special case of M in which H is, for example, a formulation of mathematical physics in terms of a nonmodalized (and thus, for the modal-structuralist, literally false) mathematical theory.

Field has made a similar point but with a slightly different emphasis (see [6], pp. 256–60). The modal-structuralist translate of applied mathematics is supposed to show us how to *dispense* with reference to (actual) mathematical objects by enabling us to derive predictions about the actual world by showing that the predictions obtain in every structure arising from the actual situation by adding copies of the relevant mathematical structures. If this is so, however, why shouldn't an analogous argument allow us to dispense with all *theoretical* architectures, by deriving statements about the actual world by showing that they obtain in any model arising from the *observable* situation by adding entities playing the relevant causal-explanatory role? The reasoning involved seems to be identical in the two cases.

The present difficulty for Hellman's construal of applied mathematics arises on a realist understanding of the nature of scientific explanation. It would not, as far as I can see, bother someone who thought of a theory merely as a device for generating predictions about the observable realm. What Hellman has done very roughly is to identify the truth condition of a theory in mathematical physics with the condition that the actual world behaves *as if* there were mathematical structures of the sort postulated by the unmodalized theory; a prediction registered by an observation sentence is derived by showing that the sentence holds in any situation containing the relevant part of the actual world together with the relevant mathematical structures. Such a prediction is obtainable as well on the modal version of the theory as on the nonmodal one. The problem arises only if the mathematical content of a theory in physics is viewed as playing an ineliminable explanatory role, and the explanations it generates as collapsing if their mathematical content is 'modalized away', just as they would collapse if other parts of their theoretical content were 'modalized away'.

5.3 Let's be unrealistic So it is appropriate to briefly consider the fate of Hellman's proposal within an antirealist framework. The difficulty here is that the modality involved in Hellman's transcriptions of the claims of mathematical physics, for example, doesn't make any obvious sense within such a framework. The idea is to consider possible situations alternative to actuality in which the physical states and relations of actual objects are held fixed, but from an antirealist point of view many of these states and configurations are out of play. In response to this difficulty, Hellman attempts to provide a reconstruction of the relevant modality without the assumption that it makes sense to talk about a 'determinate physical configuration of the world'. There are two versions of the reconstruction.

5.4 Finite synthetic bases The general idea is to locate a *synthetic basis* for the magnitudes in question, in the first version, a *finite* set of nonmathematical predicates which are taken for the purposes at hand to completely describe a given magnitude. The translation schemes given for analysis or **ZF** are then conditionalized on the hypothesis that all actual objects exist in the relevant possible worlds and fall under precisely the same predicates in the synthetic basis as they do in the actual world.

To state this condition, Hellman makes use of an actuality operator '@' that applies to a statement φ to give a sentence $@\varphi$ that holds in any world if and only if φ holds in the actual world. If

$$\Omega \;=\; \{R_1, \ldots, R_n\}$$

is the relevant synthetic basis, then the required stability condition may be given by the formula

$$(\exists X)\ (@\forall y X(y) \wedge$$
$$(\forall y_1), \ldots, (\forall y_n)(\bigwedge_{i=1}^{m} Xy_i \to \bigwedge_{i=1}^{n}(R_i y_i, \ldots, y_m \longleftrightarrow @R_i y_i, \ldots, y_m))),$$

where by adding superfluous argument-places we may assume that each R_i is m-ary for a fixed m.[8]

Hellman says little about why one should generally expect there to be such a finite synthetic basis for a given physical magnitude; in the end, he appears to relax the requirement of a finite synthetic basis in a somewhat problematic way, as we shall see. It is worth exploring for a moment some assumptions that would ensure the existence of such a basis.

If f is a rational-valued function that describes a given magnitude, a generally sufficient condition for Ω to constitute a synthetic basis for that magnitude is the following.

($*$) For each rational number r, there is a predicate $\varphi_r(x)$ in $\mathcal{L}(\Omega)$ such that for each object α, $\varphi_r(\alpha)$ holds iff $f(\alpha) = r$.

There is in turn a simple sufficient condition for ($*$):

(C) (i) there is a unary predicate $u(x) \in \Omega$ such that $(\forall x)\ (u\ (x) \longleftrightarrow f(x) = 1)$;

(ii) there is a 3-place predicate $S(x, y, z) \in \Omega$ such that $(\forall x)\ (\forall y)\ (\forall z)\ (S(x, y, z) \longleftrightarrow f(x) = f(y) + f(z))$;

(iii) $(\forall r)(\exists x)\, f(x) = r$.

Condition (i) says that Ω contains a unit predicate, and condition (iii) says that f is onto the rational numbers, that is, that every rational value is exemplified in the actual world. Condition (ii) says that Ω contains a predicate expressing metrical difference, the fact that the magnitude of one object differs from that of another by that of a third.

Let us see briefly why (C) implies $(*)$. First, for any natural number n, we shall assemble a predicate $\varphi_n(x)$ in $\mathcal{L}(\Omega)$, expressing that $f(\alpha) = n$. For $n = 0$, take $\varphi_n(x)$ to be $S(x, x, x)$. Proceeding inductively, assume that $\varphi_n(x)$ is given and define

$$\varphi_{n+1}(x) \longleftrightarrow (\exists y)\ (\exists z)\ (\varphi_n(y)\ \wedge\ S(x, y, z)).$$

Then by induction on n using (C), it follows easily that $\varphi_n(\alpha)$ holds if and only if $f(\alpha) = n$ for any n and α. Now take $r = n/m$, where $m > 0$ and m and n are relatively prime. Define

$$\varphi_r(x) \longleftrightarrow (\exists x_1), \ldots, (\exists x_m)(x_1 = x \wedge \bigwedge_{i<m} S(x_{I+1}, x_i, x_1) \wedge \varphi_n(x_m)).$$

Then for any α we have $\varphi_r(\alpha)$ if and only if $\beta_1, \ldots, \beta_m$ can be chosen such that $\beta_1 = \alpha$ and $f(\beta_{i+1}) = f(\beta_i) + f(\alpha)$ holds for each $i < m$, with $f(\beta_m) = n$. Thus we have $mf(\alpha) = n$ so that $f(\alpha) = r$, as required.

It is clear that the above construction makes essential use of the condition C(iii), requiring each rational magnitude to be exemplified by some actual object. It is equally clear that the range of cases in which one can expect this condition to be met is very limited and rather special. If, for example, there is an integral value of f that is *missed* in the actual world, in the sense that for no actual object α do we have $f(\alpha) = n$, then $\varphi_k(\beta)$ is false for every β and every $k > n$, as is every statement of the form $\varphi(\beta) = n/m$ with n and m relatively prime. The variations on the above construction which naturally present themselves make use of analogous assumptions about the actual exemplification of magnitudes.[9]

5.5 Infinite bases In order to get around technical and philosophical problems about finite synthetic bases, Hellman considers the possibility of an infinite synthetic basis (p. 131). For each rational number r, we introduce a *primitive* predicate $\varphi_r(x)$ which expresses that x has the value r with respect to the magnitude in question. The covariance claim, that the biconditionals

$$\varphi_r(x) \longleftrightarrow @\varphi_r(x)$$

hold for each actual value of x in the possible worlds considered, is expressed by means of a satisfaction relation for the new class of predicates. Let $S(\varphi_r, \alpha)$ say that α satisfies the predicate φ_r. Then we stipulate that the sentence

$$(\exists X)\ (@(\forall y)Xy \wedge (\forall y)\ (Xy \rightarrow (\forall q)\ (S(q, y) \longleftrightarrow @S(q, y))))$$

holds in each of the relevant worlds, where 'q' ranges over the predicates $\{\varphi_r\}$.

The difficulty here is that the new predicates cannot really be primitive from the standpoint of a theory of understanding and use for the language in question; else, there would be no explaining how we could come to understand the totality of them. Rather, it seems to be Hellman's intention that the φ_r's be interpreted *operationally*:

> We could employ predicates of the form "x and y are separated by distance bearing ratio r to standard length ℓ", where 'r' is a rational constant and 'ℓ' a constant designating a preselected fixed standard (e.g., a well-isolated metre-stick). Such predicates do not involve quantification over numbers; and our understanding of them…can perhaps be explained operationally, without quantifying over numbers or other mathematical objects. By invoking sufficiently many such predicates, one may hope to supply the required 'fixation of the material facts' without circularity, and without strong hypophysical commitments. (p. 131)

Hellman does not say how such an operational interpretation leads to the required satisfaction predicate for the $\{\varphi_r\}$ but the answer is not too difficult to come by. One imagines that there is an effective map

$$\varphi_r \mapsto \pi_r,$$

where for each r, π_r is an operational procedure which, when applied to an object α, gives an outcome 1 if α falls under φ_r and gives 0 otherwise. Then the satisfaction relation $S(\varphi_r, \alpha)$ will be explicated in terms of a counterfactual that says that if π_r were applied to α, it would give the outcome 1. Thus the covariance condition for the φ_r will say that for each φ_r, any actual object α falls under φ_r in the relevant possible situations if and only if π_r would yield the outcome 1 if it were to be applied to α *in the actual world*.

Aside from general worries about operationism—set aside for the purposes of this subsection—the most serious question facing this suggestion concerns the existence of the required operational bases. As far as I can see, there is no reason whatever to expect a priori that for each rational magnitude (for concreteness, let us say *mass*, in grams) an operational specification can be given for what it is for an object to instantiate that mass. One would require, for example, for each n, distinct operational or procedural conditions governing the ascription to a system of a mass of $1 + 10^{-n}$ grams; indeed, for the construction of the satisfaction predicate required above, the association that maps each positive integer onto a suitable coding of the corresponding operational procedure must be effective (or, at least, definable). There seems little reason to suppose that such procedures exist for each n; still less plausible is the claim that such a procedure can be specified effectively and uniformly for each n. Such a thesis would seem to require operational optimism on the scale of Bridgman's *The Logic of Modern Physics* [4].

It emerges, however, that nothing quite so strong as an exact operational criterion for each rational magnitude is required for a satisfactory synthetic determination argument (p. 133); all (!) that is required is that for each positive rational number r and each positive integer n, a predicate $\varphi_{r,n}$ can effectively be found which gives an operational explanation of what it is for an object to have had a mass in the interval $[r - 10^{-n}, r + 10^{-n}]$. But again, there seems to be no reason at all to suppose that this requirement can be satisfied. The changes in instrumentation required even for an order of magnitude improvement in the measurement of a physical quantity (corresponding, e.g., to the transition from $\varphi_{r,7}$ to $\varphi_{r,8}$) may involve entirely novel technological discontinuities and sometimes substantive advances in theory. At the present time, in fact, we simply have no conception of how to set up such a specification for arbitrary r and n for *any* physical magnitude.

6 Rethinking structuralism The general claim underlying structuralism in all of its variants is that mathematics is the study of structures, or isomorphism types, independently of any special interest in particular realizations of those structures. However, the evidence from the history of mathematics and its current practice adduced to support this sweeping thesis seems rather thin. Perhaps the most frequently pointed to facts are the alternative set-theoretic interpretations of the classical number systems: the natural numbers, the integers, the rational numbers, the real numbers, and the complex numbers. Let us take the case of the natural numbers as typical. Something like the following argument has seemed persuasive to many philosophers:

(a) All standard interpretations of arithmetic are equally correct (one cannot be correct to the exclusion of any other).
(b) No nonstandard interpretation is correct.
(c) Two Tarskian interpretations of arithmetic which assign distinct references to a number-theoretic term cannot both be correct.
(d) The class of standard interpretations of arithmetic is closed under isomorphism.

(e) Therefore, no Tarskian interpretation of arithmetic can be correct.

Here 'standard interpretation' refers to any Tarskian interpretation of the proper isomorphism type. The argument is valid: Suppose that $\mathcal{A}$ were a correct Tarskian interpretation. Then $\mathcal{A}$ is either standard or not. By (b), no nonstandard interpretation can be correct; therefore, $\mathcal{A}$ must be standard. By (d), then, there exists an isomorph $\mathcal{L}$ of $\mathcal{A}$ such that $\mathcal{L}$ is a standard interpretation of arithmetic that disagrees with $\mathcal{A}$ on some term ζ. By (a), $\mathcal{L}$ is also correct, but since $\mathcal{L}$ diverges from $\mathcal{A}$ on ζ, this contradicts (c). The structuralist response to this contradiction is to replace reference to particular standard interpretations by quantification over all standard interpretations; the differences between the modalist and the 'actualist' versions of the structuralist view depend upon whether these quantifiers are regarded as modalized or not. In either case, the response is, in effect, to deny premise (b): the structuralist interpretations are nonstandard, because they are non-Tarskian. However, it is possible to question both the premise (a) and the premise (c), and doing so will lead us to an alternative construal of the structuralist view.

Premise (a) is very implausible if it is understood to say that all standard interpretations are equally acceptable for mathematical purposes; and I see no other way of understanding it. Different set-theoretic interpretations will have different mathematical claims (of simplicity, explanatory adequacy, and so on) to be acceptable: it is a condition of the mathematical acceptability of such an interpretation that it enable derivations of the laws of arithmetic, and these derivations will be more or less good as judged in terms of these criteria. It seems simply false that an interpretation in terms of say, an ω-sequence of binary stars would be regarded as mathematically acceptable.

Another problem with (a) is that whatever plausibility it may have for certain classical theories such as number theory or analysis, it is not at all well supported by evidence from other mathematical domains. The analogue of (a) for classical set theory, for example, seems quite unsupported. There simply is no plurality of isomorphic interpretations of **ZFC** each of which is actually regarded as mathematically acceptable. (Although there are alternative set *theories*, and perhaps also alternative *conceptions of set*.)

Premise (c), on the other hand, says that we cannot correctly ascribe more than one reference to a number-theoretic term. But there is a familiar model of reference on which one *can* do this which deserves some consideration.

In recent mathematics, there are many good models of the number concept. In different situations, we operate with different models, but in a situation in which a determinate model has been introduced, questions about the identities of particular natural numbers have definite answers. Similarly, while it is a matter of indifference for (most) mathematical purposes whether real numbers are identified with Dedekind cuts or with equivalence classes of Cauchy sequences of rational numbers, in the context of a particular development of analysis, facts about one or the other construction come into play: for it is part of the aim of the development to explain the data codified in an axiomatic characterization of the real number system. Let us call a situation in which a particular model of number theory (or analysis, etc.) is deployed for this purpose a *constructional context* for that theory.

Questions such as 'is 2 equal to $\{0, (0)\}$, or to $\{\{0\}\}$?', then, though without sense outside of a constructional context, may be meaningful within one. Such assertions may be viewed as having an indexical component: their truth condition depends upon the constructional context in which they are evaluated. On this view, the truth-condition of an assertion of this type in a context of utterance depends on our ability to coordinate the context uniquely with a constructional one. We may do this if the utterance is part of a mature mathematical enterprise which is reasonably self-conscious about foundations, for in such a situation, a particular standard model will have been deployed. In a great many situations, however, no *particular* standard model is at issue, and in these situations, sentences which presuppose a particular construction of the natural numbers will lack a determinate truth-value. However, since a sentence of pure number theory holds or fails rigidly across standard models, any such sentence will fall under an associated necessity concept if it is true in some standard model: it is true relative to each admissible set-theoretic reduction, and so falls under the necessity concept generated by allowing the models provided by the totality of all possible constructional contexts to play the role of indices (or 'worlds', in some generalized sense).

The present indexical picture affords an alternative interpretation of the structuralist thesis which allows us to explicate the structuralist character of arithmetical truth while allowing definite standard models to be associated with arithmetic in particular contexts. This interpretation resolves some apparently paradoxical aspects of the linguistic behavior of the mathematical community. Switching from arithmetic to analysis for a moment, different members of that community, or even the same member on different occasions, may make apparently incompatible claims about the identities of particular real numbers. When working with the Dedekind construction, for example, she may assert

$$e = \{r \in \mathbb{Q} \mid (\exists n) r < \sum_{k=0}^{n} (k!)^{-1}\},$$

and on another occasion, on a version of the Cauchy construction, that e is identical to collection of all sequences $\{r_n\}$ of rational numbers such that the inequalities

$$\mid r_n - r_m \mid < n^{-1} + m^{-1},$$

and

$$\mid r_n - \sum_{k=0}^{n} (k!)^{-1} \mid < 2n^{-1}$$

hold for each pair n, m of positive integers. These identifications are flatly incompatible if the reference of real constants such as 'e' is held fixed from context to context. On the suggested indexical interpretation, however, these apparently contradictory assertions are reconciled in a straightforward way: the apparent contradiction is generated by conflating indexical contexts.

The present suggestion fits nicely into an explanation of why mathematicians do not regard reductions of one number system to another as *pointless*. If analysis is the study of all structures isomorphic to the real line, the point of the subject will be to investigate the consequences of a categorical description of that isomorphism type, independently of any attempt to regard the objects in these structures as being constructed in a particular way. If this is so, why should a construction which identifies real numbers with Dedekind cuts or equivalence classes of Cauchy sequences of rational numbers be considered to be mathematically significant? Similarly, if rational arithmetic is the study of all structures satisfying categorical description of the rational field, why should a repesentation of such a structure as the field of quotients of an underlying integral domain be considered especially illuminating?

On the suggested interpretation of the structuralist thesis, the mathematician operates, in a certain range of situations, in a manner analogous to a natural scientist dealing with an entrenched but unexplicated set of laws: she looks for a reduction. The familiar strategy is to produce a representation of the subject matter of the laws that construes these objects as structured configurations of more basic objects, and to derive the laws in question from a theory of these basic entities in conjunction with the relevant 'bridge' principles. On the picture being suggested here, much the same sort of thing was going on in the alternative constructions of the real numbers in nineteenth century analysis. There is a body of familiar laws which categorically describe the real line and which are explained in terms of the theory of the rational field, in conjunction with the identification of real numbers with, for example, equivalence sets of Cauchy sequences of rational numbers and the induced construal of the field operations on the real numbers. The differences between the two sorts of cases are, *inter alia*, first, that the standard of explanation in the second is a mathematical one, requiring a certain sort of explicative proof; and, secondly, that the reductions in the second case are essentially multiple. The only constraint on an acceptable reduction is that it enable an explanation of the target laws: but this condition will obtain if the 'bridge' principles facilitate an explanation of why some member of the relevant isomorphism type satisfies those laws. The multiplicity of acceptable reductions is accommodated by the indexical character of the truth concept for the target theory, which allows many constructions of the subject matter to be literally correct—in context.

I cannot pretend to have said enough about the present 'indexical-structuralist' conception of mathematical truth to have characterized it adequately. It is intended to apply to those fragments of classical mathematics which appear *both* to be about a definite system of objects *and*, in some sense, to be *about structure*; these, I submit, are precisely the branches of classical mathematics which are normally thought of as possessing multiple set-theoretic reductions. But not all of classical mathematics is plausibly viewed in this way. For reasons some of which were indicated above, I do not believe that set theory itself can, without quite serious distortion, be construed in this way, notwithstanding the fact that set theory is our most general instrument for *describing* structure.

I will conclude by making brief mention of another way in which mathematics appears not to be purely structural in character. This concerns what are generically termed *representations*. Mathematicians are frequently interested in quite special ways in which an isomorphism type can be presented or described. For example, it is considered significant when one has a homomorphism mapping a group onto a set of invertible $n \times n$ matrices for a fixed n over the field of complex numbers, or, equivalently, a set of invertible linear maps defined on complex n-space. There are similar 'privileged' representations for other classes of structures, for example, Boolean Algebras, Lie Algebras, and Topological Groups. A different sort of representability concerns the possibility of *effectively* describing the structure in question, that is, the problem of constructively presenting a copy of the structure. In all of these cases, a representation consists of a homomorphism mapping the structure in question onto a more 'concrete' structure. It is the content and significance of this notion (or notions) of relative concreteness which seem to me to be difficult to capture in purely structural terms. The required concepts of relative concreteness are notoriously unpreserved by isomorphisms of the structures to which they apply; but they seem nonetheless to be mathematically significant notions, and this circumstance by itself points to an obstruction to a fully general identification of mathematical notions with structural ones.

7 Conclusion Hellman's book presents an attractive synthesis. It is attractive, because it arrives simultaneously at a representation of mathematics that goes to the core of part of what is actually involved in the elaboration of the mathematical domain, viz., a preoccupation with the structural properties of that domain, and also to the heart of a number of serious philosophical problems about the nature of mathematical truth. I think many of the problems raised above for Hellman's particular attempt at this synthesis are likely to arise in one form or another for any attempt, and so I have treated them in some detail, and, to some extent, in abstraction from the particulars of Hellman's presentation. However, *Mathematics without Numbers* must surely stand as the definitive presentation of the modal-structuralist point of view. It rewards close study.

NOTES

1. Here we have used the following natural abbreviations:

    '$(\forall M_i)$' for '$(\forall X_i)(\forall R_i)$'
    '$(\exists M_i)$' for '$(\exists X_i)(\exists R_i)$'
    '$(\forall v$ in $M_i)$' for '$(\forall v)(X_i v \rightarrow \_)$'
    '$(\exists v$ in $M_i)$' for '$(\exists v)(X_i v \wedge \_)$'
    '$M_i \models A$' for '$A^{X_i}(R_i / \in)$'
    '$M_i \leq M_j$ ' for '$(\forall x)(\forall y)((X_i y \rightarrow x_j y) \wedge (R_i xy \rightarrow R_j xy))$'.

2. On p. 50, Hellman uses a three-place pairing relation rather than a function symbol, but this difference is inessential.
3. These considerations are generalized in my [9], to which the reader is referred for additional discussion.
4. Putnam, [11], p. 57. Italics mine.
5. Non-spatio-temporality seems by itself sufficient for one sort of causal neutrality, for an object which does not exist in space and time cannot in the ordinary sense be an individual constituent of the events that comprise the field of the causal relation. But for the purposes of his account of applied mathematics, Hellman must require also that the constituents of the relevant possible mathematical structures do not causally interact with actual events; see Section 5.1.
6. Alternatively, it might be suggested that on the modal-structuralist view (b) and (c) should be treated in the manner described below for assertions of applied mathematics. In that case, (b) would say roughly that there are finitely many people inhabiting Los Angeles in any world containing a standard model of arithmetic and which is otherwise sufficiently like the actual world. There are both technical and philosophical problems with this idea which are taken up below. But in any case, the use of this strategy in the present context leads to some rather bizarre results. The sentence (b), for example, would on this view have to be held to be radially ambiguous. In normal nonmathematical contexts, (b) would be assigned the form (b*). In any mathematical context in which it occurs, (b) would be assigned a semantic interpretation in terms of a conditional involving reference to possible standard models of the mathematical theory in question; and these interpretations would differ from context to context. There seems to be no independent reason to postulate such ambiguity.
7. The present argument for the satisfiability of $T(x)$ is a capsule of a set-theoretic argument which, when fully formulated, would make use of a suitable modal extension of **ZFC**. Note that the present use of the **ZFC** principles is not *circular*. We are not attempting to give a justification of the **ZFC** axioms, but to motivate certain large cardinal principles within **ZFC** augmented with whatever additional assumptions we can independently justify. The classical set theorist is free to use any modal existence assumptions in this enterprise that can be adequately justified in modal terms. In this, her position is unlike that of the modal-structuralist, who must prescind from use of ordinary set theory in justifying required modal principles.
8. The present condition embodies a slight technical correction to Hellman's Condition 3.4. That condition requires the covariance of $@R_i x_1, \ldots, x_m$ in a possible world w only for those actual values of $x_1, \ldots, x_m$ which exist in the possible worlds considered. Another technical problem is that if 'f' is the term denoting the magnitude in question, there is nothing in the condition stated by Hellman to coordinate the synthetic predicate corresponding to a particular rational magnitude r with the condition $f(x) = r$, interpreted

in the relevant possible standard models. In order to be able to apply mathematical constructions involving f to actual magnitudes, we require a way of passing from sentences of the form '$f(x) = r$' to the corresponding synthetic characterization of what it is to have the magnitude r and back again. To this end, we can require, if $\varphi_r(x)$ is the relevant synthetic predicate, that the equivalence

$$(\forall x)(f(x) = r \longleftrightarrow \varphi_r(x))$$

hold in the possible worlds considered. There are, of course, infinitely many such equivalences, one for each rational number r. We may absorb them into the antecedents of Hellman's translations in a finitary way by making use of satisfaction relations for the predicates φ_r and for predicates of the form '$f(x) = r$' saying, in effect, that these pairs of predicates are co-extensional. This device of semantic ascent will make another appearance in Section 5.3.2.

9. In a certain range of cases, it may be possible to specify a finite synthetic basis for a magnitude that does not fulfill the indicated conditions in terms of a finite synthetic basis for another magnitude which does fulfill these conditions. Suppose that Ω is a finite synthetic basis for a magnitude represented by a function f satisfying (C), and it is regarded as intelligible to ask, relative to given choices of units, of any objects α and β whether α has the *same magnitude* with respect to f as β with respect to another magnitude-describing rational-valued function g. Let us say in this case that f and g are *synthetically comparable*. Let $Eq_{f,g}(x, y)$ be a synthetic predicate expressing this equivalence relation, and for a given rational number r, let $\varphi_r(x)$ be the synthetic predicate expressing $f(x) = r$. Then, we have

$$g(y) = r \longleftrightarrow (\exists x)\ (\varphi_r(x)\ \wedge\ Eq_{f,g}(x, y)),$$

so that $\Omega \cup \{Eq_{f,g}\}$ is a finite synthetic basis for the magnitude described by g. Perhaps the problem could be solved generally by arguing that for each relevant g there exists an f satisfying condition C such that f and g are synthetically comparable.

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