Chapter 8 On our Ability to Fix Intended Structures.[1]

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1 Introduction

In this paper I want to discuss the question to what extent we are able to unequivocally fix, by the use of (scientific) language, structures that we want to talk about. In recent times, arguments have been formulated which purport to show that we are able to fix intended structures to a significantly lesser degree than we are inclined to think. Here, I will focus on the so-called *model-theoretic argument* that has been proposed by Putnam.[2] His argument purports to show that we are incapable of absolutely fixing the interpretation of both the physical and the mathematical vocabulary of our best scientific theories to a reasonable extent.

The structure of this paper to some extent mirrors the structure of Putnam [1980]. First, Putnam's model-theoretic argument is briefly reviewed. Subsequently I discuss the options that seem available for constraining the class of intended models of our scientific theories. I will sketch how Putnam would or should respond to these proposals for constraining the class of intended models. I immediately add to this the usual disclaimer that the opinions that are ascribed to Putnam in this paper may be incompatible with his present beliefs, which are notoriously hard to keep track of. Towards the end of the paper, I will argue that the issues raised by Putnam's model-theoretic argument are more properly situated in the philosophy of mathematics than in the philosophy of science. I am unable to give all these issues the attention they deserve and I try to navigate though them by at times making assumptions which really deserve careful consideration. I hope that I have at least marked the places where this is done.

I will be more concerned with the *methodology* implicit in the debate around Putnam's argument than with the *metaphysical* conclusions that we should ultimately draw from it. Indeed, the reader will not be mistaken in reading between the lines (or even, indeed, *in* these very lines) that I am somewhat sceptical that there *are* deep metaphysical lessons to be drawn from Putnam's argument.

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[1] I am grateful to Michel Ghins, Stefan Rummens, Igor Douven, Lieven Decock and Geoffrey Hellman for helpful comments on the subject matter of this paper.

[2] See Putnam [1980].

2 Putnam's challenge

Putnam's argument can be formulated as a *challenge*. It is based on an old theorem in mathematical logic, the Löwenheim-Skolem theorem. In one of its crudest forms,[3] this theorem says that if a theory formalized in first-order logic has any model with an infinite domain at all, then for every infinite cardinal number κ, it has a model with a domain of size κ.

As an example, suppose we want to formalize Newtonian mechanics in first-order logic. There are several ways to go about doing this. But whichever way we do it, we have to at some point write down axioms which postulate Euclidean space and time. This entails that we have to postulate the existence of the field of the real numbers.[4] Perhaps we will even want to postulate the set of functions on the reals. The Löwenheim-Skolem theorem entails that the resulting theory has a model which has a domain the size of the natural numbers. And this theory will also have a model with a domain of a size much larger even than the collection of functions on the reals.

Now we are inclined to say that only a small subset of these models can be *intended* ones. Such intended models are also called *standard* models. The other ones are unintended or nonstandard models of our formalized scientific theory. In the example above, the models which have a domain the size of the natural numbers will definitely be unintended. It seems even that in the final limit, our best scientific theories aim to zero in on a *unique* intended model: the world.

But how do *we* determine which among the models of our formalized scientific theories is (are) the intended one(s)? How can we distinguish the intended models from the unintended ones? This is the form which Putnam's challenge takes.

Putnam himself believes that we are in the end unable to separate intended from unintended models in an absolute, definitive way. If we are indeed unable to distinguish in a definitive way between intended and nonintended models of any given formalized scientific theory, then this is a blow to various versions of scientific realism. Since models of different cardinalities cannot be isomorphic and isomorphism is our standard criterion of structure-likeness,[5] it then seems that it would be a futile endeavor of science to aim at uniquely describing the structure of the world.

Actually, Putnam himself sees this only as a blow to *metaphysical realism*, and continues to see himself as a realist. Many philosophers disagree, and situate

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[3] In more recent times, theorems have been obtained which provide much more detailed information about the *structure* of models of first-order theories. Moreover, Löwenheim-Skolem-type theorems have been proved for languages other than the language of first-order logic. See Barwise and Feferman [1985].

[4] Or something having the structure of the field of the real numbers. I am thinking here of the nominalistic formalization of the theory of the real numbers in Field [1980].

[5] Nevertheless, one might ask oneself whether isomorphism is not too narrow as a criterion for structure-likeness. For a discussion of this question, see Shapiro [1989], p. 163.

Putnam in the anti-realist camp, where there is gnashing of teeth and wailing of souls. However this may be, metaphysical realism seems at least somewhat easier to characterize, at least operationally, than scientific realism. A metaphysical realist is a person who fails to understand the following joke:

> Somebody once asked Motke Chabad, the legendary wit: 'Tell me, Motke, you're a smart fellow. Why is *kugel* called *kugel*?'Motke lost no time in responding. 'What kind of a silly question is that? It's sweet like *kugel*, isn't it? It's thick like *kugel*, isn't it? And it tastes like *kugel*, doesn't it? So why *shouldn't* it be called *kugel*?' (Novak and Waldoks [1990], p. 7)

At least, this appears to be implied by Putnam's statement that 'the metaphysical realist further believes [...] that truth and the correspondence [between language and the world] on which truth is based are totally *non-epistemic*' (Putnam [2000], p. 2). For if naming would involve convention, and con-vention is epistemic, then truth and correspondence would *not* be totally non-epistemic. But presumably one cannot take Putnam literally here. For he repeatedly refers to the metaphysical realist as 'his former self', and it is hard to imagine that Putnam the metaphysical realist would not get the joke. This leaves me a bit at a loss to understand what metaphysical realism *is*. Nevertheless, it does seem that if it does turn out that we cannot even come *close* to separating intended from unintended models in an absolute way, then at least certain some kinds of *scientific* realism (such as structural realism) are also in trouble.

3 *The rules of the game*

Recall what we are asked to do. We are asked to formalize our scientific theories in first-order logic and to consider all and only those models which make this formalized theory true. The notion of model intended here is not the informal notion which for instance physicists use when they say that a certain system 'models' a physical situation or process. Rather, what is intended is the precise but bloodless sense which is given to the notion 'model' in the branch of logic which is called *model theory*, and which goes back to the work of the logician Tarski. A model of a first-order language is any ordered pair consisting of a domain and a collection of assignments of denotations to the nonlogical constants and predicates of the language. Whereas the interpretation of the nonlogical symbols can therefore vary wildly from model to model, the interpretation of the first-order logical symbols ($\neg, \wedge, \vee, \rightarrow, \forall, \exists, =$) is held constant in all models.

One may ask *why* the reference of the first-order logical symbols is held sacrosanct, while the reference of all other primitive symbols is up for grabs. It seems to me that Putnam's motivation for this is his conviction that only our grasp of the meanings of the first-order logical symbols is sufficiently strong to warrant keeping them fixed across models. Of course this conviction can be, and has been, challenged in two ways. First, one may argue that even our grasp of the first-order logical symbols is not sufficiently strong to warrant keeping them fixed. Indeed, constructivistic mathematicians, for instance, would object to the classical interpretation of the first-order quantifiers, especially when one quantifies over an infinite domain. Second, one may try to argue that *more* notions than just the first-order logical ones need to be interpreted uniformly in all models, or at least constrained more than just to the extent of ensuring that the theory can be made true. Of course combinations of the first and the second strategy are also possible.

4 Constraining the interpretation of non-first-order-logical notions

In the sequel I will grant, for the sake of argument, that the interpretation of the first-order logical constants should be held fixed. I will concentrate instead on ways in which the *second* strategy can be pursued. I will distinguish between mathematical notions, logical notions and physical notions.

4.1 Mathematical intuition

Perhaps our intuitive mathematical faculties allow us at least to *partially* identify the intended models among the class of all models that make the given formalized theory true. According to Gödel's version of platonism in the philosophy of mathematics, we stand in a quasi-causal, quasi-perceptual relation to mathematical objects. This relation then allows us to intentionally pick out, among the infinite number of structurally dissimilar models which make the theory true, the ones which contain the intended mathematical objects (the 'real' natural numbers, the 'real' reals). If this is the case, then we succeed in fixing at least the interpretation of the *mathematical* vocabulary of our formalized scientific theory *completely.*

This line of reasoning is dismissed by Putnam. He finds the quasi-causal, quasi-perceptual relation that is postulated by Gödelian platonism utterly mysterious. In this, he follows Benacerraf, who has argued at length against precisely this aspect of Gödel's platonism.[6] Few of the commentators on Putnam's model-theoretic argument take issue with this assessment of the argument from mathematical intuition.

4.2 More logical notions?

4.2.1 Denumerability and finiteness

Quine at one point suggests, in a discussion of the Löwenheim-Skolem theorem,

[6] The classical reference here is Benacerraf [1973].

that we should perhaps treat 'denumerable' as an elementary concept.[7] We could then formalize 'denumerable' as a primitive predicate of sets, and stipulate that it should be uniformly interpreted as being true of denumerable sets and of nothing else. Such a stipulation presupposes that we have a strong grasp of the notion of denumerability. Otherwise the stipulation would just be a meaningless instruction. This amounts in effect to treating denumerability as a *generalized quantifier*, in the technical sense of the word.[8] Our formalized scientific theory will then no longer be a first-order theory and we can no longer appeal to the crude form of the Löwenheim-Skolem theorem that we have given above to argue that our formalized scientific theory will still have models of all infinite cardinalities.

But it is not at all clear that this solves the problem completely. The theory of elementary arithmetic, Peano Arithmetic (PA), not only has nonstandard models with uncountable domains, but also has *denumerable* models which are structurally different from the standard natural numbers structure: it suffices to consider any denumerable model for PA + the negation of the gödel sentence for PA. So the notion of denumerability cannot be used to single out the indended model of PA even up to isomorphism. For the theory of the real numbers the situation is worse, in fact. For in a first-order context the notion of denumerability does not even suffice to pin down the *cardinality* of the set of real numbers.

A somewhat more attractive option might be to concentrate on the notion 'finite'. The property of finiteness of collections is not definable in first-order logic. But we may contemplate viewing the notion of finiteness as a primitive notion, to be formalized as a primitive predicate which in any given model is true precisely of the finite collections in the domain of the model in question. After all, it seems that we have a fairly strong grasp of the notion of finiteness. Feferman and Hellman have shown how using the notion of finiteness as primitive, a (predicative) theory of arithmetic can be constructed which is what is called *categorical:* all its models are isomorphic to the 'intended' model.[9]

From the notion of finiteness, or from any other 'generalized quantifier' that has been considered in the literature, more than categoricity for the natural numbers cannot be obtained. For it is a basic fact of model theory that isomorphic structures make exactly the same sentences of any given formal language true. In technical terms, one says that isomorphic structures are *elementary equivalent*. Many philosophers of mathematics believe nowadays that this remaining indeterminacy resulting from the fact that for logical and mathematical purposes, any two isomorphic structures serve equally well, is simply a fact of life that we have to learn to live with.[10] Mathematical structures just *are* not determined sharper

7 See Quine [1952], p. 115. Lieven Decock has drawn my attention to this passage.

8 The standard reference for generalized quantifiers is Barwise and Feferman [1985].

9 See Feferman and Hellman [1995].

10 The classical reference here is Benacerraf [1965]. For a recent, mature endorsement of Benacerraf's position, see e.g. McGee [1997], p.36-39.

than up to isomorphism. In the sequel, I will assume that this is essentially correct.

Despite this, it seems that the notion of finiteness will not be sufficient for our purposes. Our formalized scientific theory will undoubtedly intend to postulate, among other things, the real numbers. The notion of finiteness does not by itself suffice to determine the structure of the real numbers up to isomorphism. Nevertheless, it is not excluded out of hand that some other 'reasonably clear' generalized quantifier, or a combination of several 'reasonably clear' generalized quantifiers, will be successful in pinning down the structure of the real numbers, or even the structure of the functions on the reals.[11] Alternatively, one can try to reconstruct as much of scientifically applicable mathematics as possible accepting only the natural numbers as given. Thus one is led to the program of *predicative mathematics* that was initiated by Poincaré and Hermann Weyl, and has over the past decades been pursued by Feferman.

4.2.2. Tennenbaum's theorem

Another way to argue that we are at least able to fix the structure of the natural numbers, one that has so far not, as far as I know, been considered in the literature, is based on a theorem by Stanley Tennenbaum.[12] This theorem says that in any denumerable nonstandard model of the first-order theory of the natural numbers, if we take the domain to consist of the natural numbers (which we can, without loss of generality), the addition and multplication relations of this model are highly nonrecursive,[13] i.e. highly noncomputable. In other words, the requirement that addition and multiplication should be *computable* functions narrows the class of denumerable models of PA down to models isomorphic with the standard model.

But addition and multiplication *are* computable functions. As children, we *learn* how to calculate the sum and the product of natural numbers. We are taught an algorithm for calculating sums and products. Therefore we are entitled to restrict the class of *denumerable* models of PA to those in which addition and multiplication are recursive. On the other hand, computability does not even make sense for uncountable sets. Therefore models with *uncountably* large domains are definitely ruled out as well. Combining these two facts, we see that we are able to fix the natural numbers up to isomorphism. Of course this argument relies on our having a good grasp of the notion of algorithm.

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[11] Stefan Rummens has rightly pointed out in conversation that this nevertheless seems doubtful, since the question of the intended models of the theory of the real numbers is wrapped up with the question of the continuum hypothesis. See also the remarks related to this in section 4.2.3.

[12] For a clear discussion and an elegant proof of this theorem, see Boolos and Jeffrey [1989], chapter 29.

[13] These operations on the nonstandard domain are not even *definable* in the language of first-order PA.

Up to isomorphism is again the best we can get here. And, as with the strategy of taking the notion of finiteness as primitive, it is again not at all clear how this or a similar move could allow us to pin down the structure of the real numbers.

As far as I know, Putnam has not commented in print on the suggestions for fixing the natural number structure using the notion of finiteness as a primitive or using the notion of algorithm as basic. But it seems unlikely that Putnam would find either of these arguments persuasive. The reason for this will be pointed out in section 5.

4.2.3 Second-order logic

Second-order logic is like first-order logic except that beside the first-order quantifiers, it also contains second-order quantifiers. Just as in first-order logic the first-order quantifiers range over *all* objects of the domain, the second-order quantifiers are in second-order logic taken to range over *all subsets* of the domain. This is not the case if one considers so-called *Henkin semantics* for the language of second-order logic. But one ought to insist that in *that* case one does not work in second-order logic but in a two-sorted first-order logic. There is a considerable amount of confusion about the distinction between first- and second-order logic in the literature. Most of it is a result of terminological ambiguities. Take for instance the title of Simpson's recent book *Subsystems of Second-Order Arithmetic*, which deals exclusively with first-order systems! For proof theorists this may be quite harmless, since they are usually able to sort out the confusion when pressed to do so. But ambiguities like this hamper the communication of results and ideas from mathematical logic to the philosophical community.

The Löwenheim-Skolem theorem does not hold for second-order logic. In fact, the structure of the natural numbers, the structure of the real numbers, and all other mathematical structures one is likely to encounter in daily empirical science can be defined up to isomorphism in second-order logic.

Putnam, and several eminent philosophers and logicians with him,[14] would be sceptical of the contention that the second-order quantifiers have been given a clear interpretation. In their most lucid moments,[15] critics of second-order logic tend to reason along the following lines. In the explication of the interpretation of the second-order quantifiers, mention is made of the set of all subsets of the domain. For any finite set, the set of all its subsets is a perfectly determinate object. But it is by no means clear that the collection of all subsets of an *infinite* set is a well-determined object, and we have seen that the domain of the intended interpretation of our formalized scientific theory *will* be of infinite size. In sum,

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[14] Quine is one of them.

[15] In their less lucid moments they formulate weaker arguments against second-order logic. These arguments should either be passed over in silence, or be gracefully taken apart, as is done in Boolos [1975].

or so it is contended, second-order logic is inextricably wrapped up with transfinite set theory, and transfinite set theory contains too much indeterminacy.

The question whether the set of all subsets of an infinite set is a well-determined object is related to the status of Cantor's *continuum problem* about the *size* of the set of all sets of natural numbers. Many set theorists indeed think that this question can only be settled by a convention. But a significant number of set theorists think that Cantor's question has a determinate answer.

Others, most notably Boolos, have tried to resist the critique on second-order logic in another way. Boolos has devised an interpretation of second-order logic which is equivalent to the usual interpretation and yet does *not* overtly involve set-theoretic notions (such as the notion of arbitrary subset).[16] His critics contend that it *covertly* involves set-theoretic notions. The debate continues. I will not pursue it here.

When beside scientifically applicable mathematics also set theory is taken into account, then even accepting second-order logic does not by itself give a *complete* solution to the fixing problem. The standard set theory is Zermelo-Fraenkel set theory with the Axiom of Choice, ZFC^2 for short.[17] ZFC^2 is not categorical: not all its second-order models are isomorphic. But there is a so-called *quasi-categoricity* result for ZFC^2: if one takes any two non-isomorphic second-order models of ZFC^2, then one of them is isomorphic to an initial segment of the other, or more precisely, one of them is isomorphic to a strongly inaccessible ordinal rank of the other. Recently, McGee has shown in an intriguing piece of work that if an apparently reasonable axiom is added to ZFC^2 with 'Urelemente', then the collection of the pure sets of any two models of the resulting theory *are* isomorphic to each other.[18] The 'apparently reasonable' axiom in question says that the collection of Urelemente form a *set* and not a proper class, or, in other words, that in comparison with the amount of sets there are, there are only 'relatively few' Urelemente.

4.3 Sense perception

Let us now concentrate on the *physical* vocabulary of our formalized scientific theory. The argument from mathematical intuition was dismissed as an implausible account of how the interpretation of the mathematical vocabulary is

[16] See Boolos [1985]. See also the *Appendix on Pairing* by Burgess, Hazen and Lewis, in Lewis [1990].

[17] The superscript indicates that we consider the second-order formalization of set theory, which differs in certain points from the first-order formalization of set theory.

[18] See McGee [1997]. He shows that the same result obtains if one works not in second-order logic but in the framework of *schematic* logic, which is just like first-order logic except that the axiom schemes of the theories are interpreted as ranging over all expressions of all *extensions* of the language in which the schematic theory is formulated. Schematic logic can be seen as a *fragment* of full second-order logic.

fixed (cfr. section 4.1 above). But perhaps the strategy *behind* this move is more promising at least for a part of the physical vocabulary.

We stand in a perceptual relation to certain objects, properties and relations for which we have names in our formalized scientific theory. Therefore at least *these* reference relations ought to be kept constant across different models. It would be a violation of common sense realism about the observable that appears to be widely accepted both in analytic and in continental philosophy if the interpretation of observable vocabulary would be allowed to vary.[19]

Putnam expresses some willingness to go along with this line of thought.[20] But both the mathematical and the unobservable physical (and the mixed) vocabulary remain unaffected by this concession. Putnam shows that because of this, we are still left with a plethora of cardinally and structurally dissimilar models.

4.4 The causal theory of reference

But perhaps the strategy of the preceding subsection can be extended to the unobservable. The causal theory of reference, due to Kripke and Putnam,[21] tells us (in a nutshell) that nonobservational physical vocabulary obtains its meaning (at least in part) in acts of initial baptism, where such terms are stipulated to refer to all the things that are of the same *natural kind* as what causes the relevant sense impression present during the act of baptism. The notion of 'belonging to the same natural kind' is then explicated in terms of structural similarity. In this way, an explanation is given of the *causal connection* between the unobservable structure of the world and the nonobservational vocabulary of our scientific theories. Now it has been claimed that this causal link *fixes* the reference relation, so that as with the observational vocabulary, we ought to keep the interpretation of the nonobservational vocabulary constant across the models.

Putnam rejects this line of reasoning, for two reasons. First, there are signs that Putnam has in recent times come to doubt the causal theory of reference. But I must confess that it is not completely clear to me what his present position on this matter precisely amounts to. Sometimes it seems that Putnam is only rejecting *physicalistic interpretations* of the causal theory of reference. Second, he rejects appeal to the causal theory of reference because it is 'just more theory'. He contends that the causal theory of reference must *itself* be formalized in first-order logic and added to our given formalized scientific theory. Then the class of models of the augmented formalized scientific theory must be considered. The Löwenheim-Skolem theorem applies to this theory, even when interpretation of the observational vocabulary is kept fixed, which leaves us just where we started

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[19] Nevertheless, it ought to be kept in mind that Quine has toyed with indeterminacy of reference-arguments that purport to undermine even this argument.

[20] But cfr. infra, section 5.

[21] The classical reference here is Kripke [1980].

from.

However, if Putnam rejects the causal theory of reference, then it is not clear that he is expressing the majority opinion among philosophers of language. It is certainly not the case that *all* philosophers of language have given up on the causal theory of reference. Moreover, the thesis that the causal theory of reference is 'just more theory' in the sense explained above, has been widely rejected as an unacceptable rethorical move.

5 *Just more theory*

Igor Douven has recently reconstructed Putnam's argument in such a way that the just more theory-move is avoided.[22] I cannot here go into the details of Douven's reconstruction.[23] All I can do here is to express my scepticism that any such reconstruction can be carried out without introducing new, nontrivial premises - thus generating a *new* argument. So I will assume here, along with most of the commentators on Putnam's argument, that the just more theory-move is an *essential* ingredient in his line of reasoning.

I do not want to repeat here in detail the misgivings about the 'just more theory'-move that have been expressed in the literature. But note that if this move is permissible at all, then it ought to be equally applicable to the argument from mathematical intuition (see section 4.1), the argument from perception (see section 4.3) or indeed against *any* attempt to narrow down the class of intended models of our formalized scientific theory. Putnam appears to some extent to welcome this conclusion, though at the same time he appears hesitant to apply it at *every* juncture. More concretely, he expresses sympathy for its application to the argument concerning sense perception (see section 4.3), but not to the arguments concerning mathematical intuition (see section 4.1) nor to the arguments

[22] See Douven [1999]. Putnam himself now endorses a version of Douven's reconstruction. See Putnam [2000].

[23] But I cannot resists making the following brief remarks. Avoiding the just more theory-move has a *price*. In Douven's reconstruction, this takes the form of an *extra premise* which must be assumed to make the argument go through. This premise can roughly be expressed as (Douven [1999], p. 482, my gloss): 'If no naturalistic theory of reference is true, then a theory is true if and only if it is satisfiable.'It is assumed that since this statement must be taken to be a *conceptual truth* by the (metaphysical) realist, it must be *necessarily true*. But I simply fail to see the conceptual connection between the antecedent and the consequent of this thesis. I do not see why the realist must assume this to be a necessary truth. It must immediately be added here that Douven expresses an awareness of the fact that the assumption of the necessary truth of this statement is nontrivial.

Another debatable assumption of Douven's argument is that the possibly infinite disjunction of possible naturalistic theories of reference counts itself as a naturalistic theory of reference. See Douven [1999], p. 488.

concerning second-order logic (see section 4.2.3).

I think that what is behind all this is the following. We have a strong command or grasp of the meaning of some concepts, and a weaker command or grasp of the meaning of other concepts. For instance, we may want to assume that we perfectly understand the Tarskian explication of the truth-conditions of conjunction, whereas the notion of an arbitrary subset of an infinite set, perhaps, is less clear to us and may even be inherently indeterminate. Such unclear or perhaps even indeterminate notions ought not to be taken as understood in our explication of how we fix the interpretation of our scientific theories. For the indeterminacy or unclarity of these notions would carry over to the interpretation which is formulated in terms of them, and in the end no clear interpretation is given.

Which notions are sufficiently clear or determinate to function in a philosophical account of the models intended by our theories should be the subject-matter of careful case-by-case philosophical scrutiny. It was misleading for Putnam to suggest that there is a blanket strategy (a logical trick) for refuting attempts to restrict the class of intended models. Nevertheless, if a notion is to some extent unclear or indeterminate, then perhaps the best thing we can do is to tie it to notions of which we have a better understanding, for instance by writing down a list of axioms concerning this notion. This list then indeed functions as 'just more theory'. Of course we may, over time, obtain a stronger grasp of some of these notions. In this way, there may be notions which we previously regarded with suspicion, but now feel comfortable using in explications of how the intended interpretations of our scientific theories are fixed.

So I suspect that Putnam should ultimately rest his case on the claim that the notions that need to be used to limit the class of intended models of our formalized scientific theory are not sufficiently clear. 'Denumerable' can for this reason not serve the purpose for which Quine wants to use it, he should say. Similarly, the notions 'algorithm' and 'finite' cannot be assumed to be understood at the outset (he should say).[24] These evaluations are of course open to philosophical debate. In the case of the notion of algorithm, we are then led to Kripke's interpretation of Wittgenstein, and of course to Wittgenstein himself on following a rule.

The following picture emerges. We have very clear notions, the first-order logical constants and the notion of a first-order model for instance, which allow us to characterize every given finite set up to isomorphism. If perception and perhaps even causality allow us to pin down reference further,[25] then we might even be able to *uniquely* fix concrete finite structures. We also have notions which are perhaps more complex, such as 'finite' and algorithm'. If our grasp of them is nevertheless sufficiently strong, then we can fix the natural numbers

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[24] Incidentally, it seems that the notion of algorithm *presupposes* the notion of finiteness.

[25] I believe that the two are tied together. Ultimately, the notion of causality seems to play a role in our confidence in our ability to fix the meaning of observational expressions.

structure up to isomorphism. And if even our explication of second-order logic is definite, then we may be able to characterize all mathematical structures that are needed in empirical science up to isomorphism.

6 Physical and mathematical theories

Even if second-order logic allows us to fix up to isomorphism all mathematical structures needed in science, this provides us with no guarantee that the complete state of our (four-dimensional, say) universe can in principle be described up to isomorphism in second-order logic. Nor does it even guarantee that the class of all models in which all the true fundamental laws of physics (if there are such things) hold can be characterized up to isomorphism.

But it would give us some hope that something approaching the latter would be the case. For one would assume that the fundamental laws of physics are somehow smooth and in some sense simple. Otherwise, there would be an inclination to conclude that there are no fundamental laws at all. It may be that in the true fundamental laws *constants* occur, real numbers, say, which take on a very specific value which is impossible to describe exactly even in second-order logic. But in that case we might still be able to write down expressions in second-order logic which give a good *approximation* of the exact true fundamental laws. This is then the best we can hope for.

7 Methodological morals

To conclude this paper, I will attempt to draw some morals from the debate concerning Putnam's model-theoretic argument.

I have not discussed the bearings of Putnam's model-theoretic argument on the question of the viability of *scientific realism.* I have not discussed Putnam's own preferred solution to the philosophical paradox which he has created and which led him to his *internal realist* position. As far as I can tell, all I have said in this paper is *compatible* with Putnam's overall metaphysical outlook - although it does not *entail* that outlook either. The same holds, presumably, for other positive solutions to Putnam's paradox, such as the one advanced by van Fraassen.[26] And something similar can be said for most versions of scientific realism. All I have done is to draw some *methodological* morals from the debate concerning Putnam's argument.

One moral of this paper is that while they are certainly *relevant* for the investigation of our ability to refer unequivocally to structures, theorems from mathematical logic do not by themselves determine the correct solution to the

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[26] See van Fraassen [1997].

problem. Benacerraf is in this context absolutely right when he stresses that nontrivial philosophical premises are also needed.[27] When these are made explicit, they turn out to touch on *many different* areas of deep philosophical dispute. For instance, we really have to sit down and conduct a philosophical inquiry into the nature of causality. Such shifts of focus bring the realism discussion back to where it had been located for *decades*, and where the battle *should* be fought: on familiar philosophical territory, outside mathematical logic.

Another point is that, as I have attempted to show, many of the interesting problems raised by Putnam's model-theoretic argument are properly situated in the *philosophy of logic and of mathematics*. Therefore it seems to me entirely appropriate that Putnam's article on the model-theoretic argument was soon after its first appearance reprinted in Putnam and Benacerraf's classical anthology on the philosophy of mathematics. At the same time I am amazed at the large amount of discussion that Putnam's argument has generated in the philosophy of science and in general metaphysics, compared to the much smaller amount of discussion it has given rise to in philosophy of logic and mathematics.

Mathematics forms an integral part of our physical theories. But mathematical logic studies in the first place logical aspects of *mathematical* theories. And I cannot escape the feeling that the structure of the mathematical universe is not *as directly* relevant for the realism debate in the philosophy of science as Putnam appeared to think in his [1980]. It is not clear to me exactly *how* it would be relevant for forms of *scientific* realism if mathematical structures turn out to be significantly underdetermined.

It is regrettable that until now philosophers of science and philosophers of mathematics have not had many interesting things to say to each other. But until truly deep connections between the physical universe and the mathematical universe(s) are clearly explicated, I suggest that it is better to let these two fields continue their separate lives. The time for contemplating a unified formalization of scientific theory has not yet come.

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[27] See Benacerraf [1996].

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