Putnam on Foundations: Models, Modals, Muddles

Abstract: Putnam has famously offered a sketch of a mathematics without foundations, existing in two equivalent descriptions, set-theoretic and modal-logical. Here his proposal is critically examined, with attention to difficulties surrounding both the modal-logical description itself and especially the notion of equivalence of descriptions.

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1. *Putnam, Logic, and Foundations*

Among the many and varied writings of Hilary Putnam on foundations and philosophy of mathematics, one work holds special prominence. The classic anthology of Benacerraf and Putnam (1964/1983), known to generations of students, was enriched in its second edition by the addition of two papers from each of its editors. But one of Putnam's, "Models and Reality" (1980), though it was a Presidential Address to the Association for Symbolic Logic, and contains theorems on models of set theory, is on its own showing more in the domain of philosophy of language. That leaves the other, "Mathematics without Foundations" (1967b), as perhaps Putnam's most conspicuously displayed publication centrally on philosophy of mathematics. It is that paper, henceforth referred to as MWF, and the issues it raises, that will be my concern here.

MWF has two distinguishable aspects, calling for separate comment. For want of better terms I will call the two the *foreground message* and the *background story*. The foreground message, the ostensible main claim of the paper, vigorously stated in its opening paragraph, is that mathematics is in a safe and flourishing condition; it is not endangered by any foundational crisis; it does not need to be rescued by philosophical heroics. I hope that we can all agree that Putnam is right on this point.

Nonetheless, in the course of developing this message Putnam makes a remark about not taking certain things seriously on which the late Georg Kreisel pounced in a critical review (1972). Kreisel seems to take Putnam to be insufficiently appreciative of

work in mathematical logic and foundations. At the time, there had already been a shift from impassioned advocacy for or against restrictive foundational frameworks such as finitism or constructivism or predicativism to dispassionate attempts to determine the scope and limits of the mathematics that could be developed within one or another such framework if it *were* adopted. More generally the project has become to determine what trade-offs are available in mathematics between, on the one hand, expansion of the power to prove theorems, and on the other hand, contraction of the danger of collapsing in contradiction (as has happened in a few cases: Gottlob Frege's *Grundgesetze*, Alonzo Church's illative λ-calculus, the first edition of W. V. Quine's *Mathematical Logic*, ZFC set theory plus a Reinhardt cardinal). Decades of work has established a detailed scale of "consistency strength", as what might more frankly be labeled "inconsistency risk" is euphemistically called, in which virtually every foundational framework that has ever had advocates has been placed (apart from Quine's "New Foundations", and even that seems to be on the brink of being placed as well).

The best-known part of Putnam's quite extensive early technical work in mathematical logic was closely related to these developments. I mean his work with Martin Davis and Julia Robinson, culminating in their great 1961 paper, pointing the way to the eventual solution of Hilbert's Tenth Problem by Yuri Matijasevich. For what that solution implies is just the following: As one climbs higher and higher up the scale of consistency strength, which near the top means assuming bigger and bigger "large cardinals" in set theory, one continues to get at each step new results in mathematics *of*

the most down-to-earth kind, results asserting the nonexistence of solutions to certain Diophantine equations.

It is, I hope and believe, a misreading of Putnam to take him to be disparaging this kind of research, regardless of what he may say about not taking seriously this or that project. There is really nothing in his foreground message that should be offensive even to a logician like Kreisel, provided one carefully distinguishes "foundations" in a traditional, philosophical, "foundationalist" sense, which Putnam holds that mathematics can do without, from "foundations" as an American Mathematical Society subject classification, in which use it is essentially just a synonym for "mathematical logic".

2.   *A View of His Sketch*

If Putnam's foreground message ought to be uncontentious, the same can hardly be said of what I am calling his background story, expounded in the section of MWF headed "A Sketch of My View". Here Putnam contrasts two "pictures" of mathematics, one being "mathematics as set theory", which he takes to be familiar, the other being "mathematics as modal logic", his own innovation. He also offers a characterization of the relationship between the two: Borrowing a term from his teacher Hans Reichenbach's discussion of wave-particle duality in quantum mechanics, Putnam says the two pictures are "equivalent descriptions".

To be accurate, I should not say that this story is "expounded" in MWF, but rather that it is "adumbrated". For the sketch offered begins by saying that it will be "cursory and superficial", and this disclaimer is as accurate as it is disarming. If in discussions in

later work Putnam seems to attribute great significance to MWF, one probably should understand the significance as being attributed to *the view of which a sketch is given* rather than to *the sketch that is given of the view*.

The "mathematics as modal logic" picture directly inspired Geoffrey Hellman's book (1989), advocating a "modal structuralist" version of nominalism, and so indirectly inspired a chapter (IIIC) in the book coauthored by myself and Gideon Rosen (1997), presenting a "mixed modal" strategy for reconstruing mathematic nominalistically. MWF is mentioned in Burgess and Rosen mainly just as a precursor to Hellman, but to treat the paper this way is to focus on only one half of the background story, "mathematics as modal logic", downplaying the other, "equivalent descriptions"; and of this emphasis Putnam himself cannot approve. His attitude can be seen, among other places, in a pair of twenty-first-century works, one a public lecture (2002), the other a book chapter (2012). In both he suggests that insufficient attention to the what he has written about "equivalent descriptions" in MWF and elsewhere has had unfortunate effects.

A theme of the 2012 chapter is that neglect of MWF has led to misunderstanding of another of Putnam's works, the celebrated booklet *Philosophy of Logic* (1971). Putnam can scarcely deny that the rhetoric about the indispensability of set theory in the booklet is superficially Quine-like, but he does deny that it is appropriate to give this rhetoric any genuinely Quine-like reading, given what he was already on record as saying in MWF. Failure to attend to MWF has led to a conflation of Putnam's with Quine's indispensability argument, creating a chimæra, "the Quine-Putnam indispensability

argument". I need not say much about this. That the two "indispensability arguments" are different may be news to many, but it is not news to Burgess and Rosen any more than to Hellman. I think the three of us would all agree that Putnam is right that many commentators have got him wrong.

I will just say, however, that ignorance of MWF may not be the sole cause at work. Even a reader well-acquainted with the position of Putnam in MWF might still have attributed an incompatible, Quine-like position to the Putnam of the later booklet, simply because the Putnam of those years was as famous or infamous as Bertrand Russell for frequent radical changes of views, philosophical and other, and because there is little in the booklet to indicate that it does not just confront us with another of the tergiversations for which its author had become notorious, comparable to the flip-flop over whether the Continuum Hypothesis has a determinate truth value between his paper on Russell (1967a) and MWF later the same year. So far as I can see, there is in the booklet just one late, brief mention of "equivalent descriptions" as a topic not being gone into pro or con.

A theme of the 2002 lecture is that neglect of the lessons of MWF has resulted in consequences so grave as to suggest that analytic philosophy as such may simply be a bad thing; and the book of Burgess and Rosen is cited as the best example of what is wrong with it. As one of the coauthors, I am naturally not happy to see the book thus negatively evaluated by an eminent senior figure; but as Doctor Johnson said, if the reader of a book has not been pleased, it is no use to tell him why he ought to have been pleased. I can at least console myself that I am *only* a target in the lecture. My poor

coauthor Rosen is a *double* target, being not just criticized with me in the public lecture for our joint work, but also further criticized in the book chapter for later solo work. But Rosen is perfectly capable of speaking for himself, if he sees any point in doing so, and I will speak only for myself in what follows.

I will not, however, be speaking for either of us right away. My immediate aim will be to address some of difficulties that arise when one tries to fill in the gaps in the sketch of a view in MWF. These emerge with both components of Putnam's background story, the "mathematics as modal logic" picture, which I will consider first, and the "equivalent descriptions" claim, which I will turn to later. It is in connection with the latter that occasion will arise to look at Putnam's criticisms of Burgess and Rosen, and say a few words in self-defense.

3.   *Modality and Mathematics*

In the 2002 lecture there is early on a section entitled "The modal-logical interpretation of mathematics" in which Putnam enunciates his view thus:

(A)   In mathematics the assertion that objects or structures 'exist' is completely fungible with the assertion that their existence is *possible*.

Formulation (A), considered by itself, is most naturally read as suggesting the following:

(B)   In pure mathematics the assertion than an object exists is equivalent to the assertion that it possibly exists.

(C)   In pure mathematics the assertion than a structure exists is equivalent to the

assertion that it possibly exists.

These formulations are suboptimal. For (C) is redundant, since it is already implied

by (B), mathematical structures being after all just one kind of mathematical object,

capable of appearing as elements in higher structures such as categories. And (B) is too

restricted, applying only to existence assertions what it seems could be applied to *all*

purely mathematical assertions, giving us among other principles the following:

(D)   In pure mathematics the assertion that an object has a certain property is equivalent

to the assertion that it possibly has that property.

(E)   In pure mathematics the assertion that two objects stand in a certain relation is

equivalent to the assertion that they possibly stand in that relation.

The appropriate generalization of (D), (E), and so on, would seem to be this:

(F)   In pure mathematics, adding "Possibly" or "It could have been that" in front of an

assertion makes no difference.

Given the equivalence of *necessarily* to *not possible not* we get from (F) the following:

(G)   In pure mathematics, adding "Necessarily" or "It could not have failed to be that"

in front of an assertion makes no difference.

Note that (F) and (G) do *not* imply that there is anything special about the *logic* of

"exists" in mathematics, that the introduction and elimination rules for the quantifiers somehow differ between mathematical and extramathematical discourse. What (F) and (G) *do* say is that there is something very special about the logic of "possibly" and "necessarily" in mathematics: It trivializes.

I am tempted to suggest that the real principle is after all not (F) and (G) but something more radical, namely this:

(H)   In pure mathematics, we just don't add things like "Possibly" or "Necessarily" in front of assertions.

But I must hasten to add a clarification that perhaps should have been made earlier: In all the above formulations it is to be tacitly understood that we are setting aside modals used in a merely *epistemic* sense. Perhaps the best evidence in favor of the hypothesis that a rule like (H), understood as banning *non-epistemic* modals, is at work in our mathematical language is that when modals *are* used, as indeed they sometimes are, we automatically and spontaneously interpret them epistemically. Surely we take "Possibly Euler's constant is rational, and possibly it is irrational," to mean something like, "Euler's constant isn't known to be irrational, and it isn't known to be rational," rather than anything like "There is an alternate universe where Euler's constant is rational and an alternate universe where it is irrational."

Principle (H) is essentially a negative grammatical rule: The usual distinctions of grammatical mood have no application in pure mathematics. If we try to transpose from

"the formal mode" to "the material mode", we get something like the following:

(I)   Purely mathematical facts are necessary.

But such a formulation, even if it can be understood as merely a rewording of (G), is dangerously misleading. It makes a humble, negative, grammatical observation look like a grand, positive, metaphysical principle. Start talking this way and philosophers are bound to be reminded of Plato. Remind them of Plato, and many will want to deny what you say; and unfortunately, experience shows that some will think they can only deny what you say if they also deny a lot of perfectly ordinary non-philosophical assertions. These include perfectly ordinary mathematical existence theorems, and thus some philosophers fall into nominalism.

All this was in the air when Rosen and I were writing our book. Some of it is hinted at there (article I.A.1.b). I claim no originality for any of it. To illustrate, it will be useful to put down some examples of specific purely mathematical assertions. Though I have claimed that existential assertions have no special status as compared with ascriptions of properties or relations, and that structures are included among objects and hardly require separate mention, I will take an existential assertion as my first example and an assertion about structures as my second, since Putnam puts emphasis on these special cases.

Consider, then, the following:

(1)   There is a counterexample to Polya's conjecture.

(2)   There is a standard model in which "There is a counterexample to Polya's conjecture" holds.

(3)   There could have been a counterexample to Polya's conjecture,

(4)   There could have been a standard model in which "There is a counterexample to Polya's conjecture" held.

The conjecture in question is that most of the numbers less than any given number have an odd number of prime factors; the smallest counterexample is 906,150,257. Never mind, for the moment, just what is meant by a standard model, apart from its being a kind of structure. Never mind, for the moment, exactly how to understand the modal "could have", beyond recognizing that it is not supposed to be merely *epistemic*.

What does our earlier discussion tell us about (1)-(4)? Well, (B) already implies that (1) is equivalent to (3), besides implying (C), which then implies that (2) is equivalent to (4). When the notion of standard model is properly worked out, model theory will presumably tell us that (1) is equivalent to (2); and when the notion of possibility at issue is properly pinned down, modal logic will presumably then tell us further that (3) is equivalent to (4). And so the whole quartet of formulations will be equivalent, at any rate for those who accept the pertinent model theory, which means accepting the background set theory within which it is developed, along with the pertinent modal logic.

4.   *Mathematics as Modal Logic*

This is a pretty picture, but unfortunately our starting point, Putnam's formulation (A), does not accurately represent his actual position, as one can see by attending to what is said beyond the enunciation of (A) in the lecture, or by reading the corresponding material in MWF. A more accurate representation would be this:

(J)   In mathematics the existence of a given *object* is completely fungible with the possible existence of a certain *structure*.

Here there is no mention of the possible existence of objects or of the actual existence of structures. Putnam's claim is that (1) is equivalent to (4) directly, and not *via* intermediates such as (2) or (3), of which he makes no mention.

All this makes the issue of the proper formulation of (4) an absolutely crucial one on Putnam's approach, which it was not in my discussion above. One must thus address the questions I postponed earlier, about the proper understanding of the notions of standard model and of the possibility modality. Putnam's hints about these matters in MWF are few and brief. The Hellman book examines, and the pertinent chapter of the Burgess and Rosen book re-examines, these matters at some length. Here I can do no more than mention a few questions telegraphically.

Structures or models. The modal-logical formulation is supposed to contrast with a set-theoretic formulation, yet it speaks of the possibility of certain structures or models, and structures or models are usually explained set-theoretically. Thus it seems that the

modal-logical formulation requires the possible existence of certain sets, which we have just been seeing is arguably the same as the actual existence of those sets. The problem could be avoided if talk of structures or models could be construed in something other than a set-theoretic way. Much of the labor in Hellman and in Burgess and Rosen, involving a good deal of auxiliary apparatus, is directed at just this problem.

Mathematical possibility. Linguists writing on modality (as in Palmer 2001) distinguish three kinds: *deontic* and *epistemic* and *dynamic*. The deontically possible is what may permissibly be done, a notion irrelevant to mathematics and to the present discussion. The epistemically possible is what for all we know may actually be, a notion already dismissed from consideration. The dynamically possible is what (actually is or actually isn't but potentially) could have been, which philosophers perhaps misleadingly call the "metaphysically" possible; and this is the notion I had in mind in my earlier discussion of possibility in mathematics. Putnam insists that *his* interest is in a specifically *mathematical* modality, but he tells us next to nothing about it. There's no denying that we do in ordinary language sometimes speak of "mathematical possibility". For instance, when the prophetess claims to have had a vision of 144,000 saints arranged in a perfect square, we may say, "That's mathematically impossible!" But what we mean here seems to be just the following. First, it's dynamically or "metaphysically" impossible: She couldn't have had such a vision. Second, purely mathematical considerations alone are enough to tell us as much, notably the consideration that the number 144,000 isn't a perfect square.

De re modality. There is a problem, however, about "modalities" such as "purely mathematical considerations alone are enough to tell us that…" The problem is that while this modality seems to make sense *de dicto*, meaning in application to a whole sentence, it seems not to make sense *de re*, meaning in application to a predicate with respect to an object. For instance, it seems mathematical considerations alone can tell us that the following is true: "The set {Hesperus, Hesperus} is a singleton," since any unordered pair $\{a, a\}$ of an element with itself is equal to the singleton $\{a\}$. And it seems mathematical considerations alone cannot tell us that the following is true: "The set {Hesperus, Phosphorus} is a singleton." For *that*, we need the nonmathematical, astronomical information that Hesperus is Phosphorus. But of course, what the astronomical information tells us is that it is the same set in both cases, differently described. What then of the predicate "…is a singleton"? Are mathematical considerations alone enough to tell us that it applies to *that set*, as it is in itself, so to speak, *independently* of how or whether it is described? This kind of difficulty is familiar from critical discussions by Quine. It matters because "quantifying in", or making assertions of the form "There is an *x* such that it is possible that *x*…" only makes sense where the kind of "possibility" involved is one for which *de re* modality makes sense.

Applications of mathematics. The project of developing a modal-logical version of mathematics seems to depend not only on our being able to "quantify into" contexts of the relevant kind of possibility, but on our being able to make cross-comparisons between how things there actually are are, and how merely possible things would have been. We

do in ordinary language make such comparisons with dynamic or "metaphysical modality", as when we say that the building that has been put up on a given site is shorter than the one would have been put up there instead if the architect's original plans had been followed. Standard formalisms of modal logic don't accommodate such cross-comparison well, but that is a defect of the standard formalisms, an issue that gets a fair amount of attention in Burgess and Rosen. We need cross-comparison to provide modal-logical versions of *mixed* as opposed to *pure* mathematical assertions, ones that mention physical as well as mathematical objects in the same sentence; and these in turn are needed to provide modal-logical versions of *applied* as well as *pure* mathematics, as we must if the modal-logical approach is going to provide a genuine alternative.

There are a lot of interconnected issues here. So far as I know, only three writers, of whom Putnam is not one, have struggled at length in print with the complexities involved. And I doubt that any of us (Hellman, Burgess, Rosen) would claim to have gotten to the bottom of all the issues. Nonetheless, I won't press further here issues about "mathematics as modal logic", since the deeper issues seem to me to be those surrounding "equivalent descriptions", the other part of the background story in MWF.

5.   *Equivalent Descriptions*

As the Council of Nicæa declared that the Father and the Son are somehow the same and yet somehow different, so Putnam declares the "mathematics as set theory" and "mathematics as modal logic" pictures, represented in my discussion by (1) and (4), are somehow the same and somehow different. I find the Nicene Creed easier to understand

than Putnam's notion of equivalent descriptions.

Well, I *think* I may after all understand his claim of *difference*. Putnam explicitly says that formulations like (1) and (4) are not *synonymous*. In the jargon of Burgess and Rosen this means that no *hermeneutic* claim is being made, no claim to the effect that the modal-logical formulation reveals what, despite misleading surface appearances, the set-theoretic formulation has deep down meant all along. I also understand that no *revolutionary* claim is being made either, no claim that the set-theoretic picture should be suppressed and superseded by the modal-logical picture. But I don't understand the claim of *sameness*. What consubstantiality is being asserted?

Even here there is one claim that I believe everyone can accept, namely, that there is a mapping back and forth between sentences of the set-theoretic and modal-logical kinds about which one can say the following: *If* one accepts all pertinent set-theoretic apparatus needed to develop model theory, and *if* one accepts all appropriate modal-logical apparatus, *then* in a grand combined background modal set theory one should be able to deduce that the mapping in question is truth-preserving: One should be able to deduce biconditionals on the order of "(1) if and only if (4)".

Putnam wants to say that the two pictures "cognitively equivalent", but whose cognition is at issue here? Presumably not that of the nominalist, who rejects set theory, or that of the extensionalist, who rejects modal logic. *They* cannot accept the grand comprehensive background theory, or the proof that the mapping is truth-preserving. Hence that mapping provides *them* with no bridge that would permit the cognizer to pass

back and forth between the two pictures. And I'm afraid ordinary mathematicians are in about the same position as the extensionalists, not because they *reject* modal logic, but because they know little or nothing about it.

What amounts to Putnam's first attempt to clarify what he means by "equivalent descriptions" consists simply in comparing the relationship between two pictures of mathematics to the relationship between the wave and particle pictures in quantum mechanics. But I'm afraid I see here only a *dis*analogy between the case Putnam wants to consider and the case of quantum mechanics as Reichenbach viewed it. In the latter, *the physicists themselves* were supposed to be aware of the particle and the wave pictures, and used to moving back and forth between them. But mathematicians know little about model theory and less about modal logic. Among them one does not have two pictures in use, but just one, the set-theoretic, worked out in great deal in many books, covering all branches of mathematics. The other picture, the modal-logical, receives a "cursory and superficial" sketch in one paper, MWF, and some elaboration in one book, and along variant lines in one chapter of another, all of which material remains wholly unknown to the mainstream mathematical community.

Putnam also sometimes says that a pair like my (1) and (4) express "the same fact". I find this, too, unhelpful. Such a formulation requires a metaphysics of "coarse-grained" facts at variance with ordinary, unselfconscious, non-philosophical fact-talk. In the *ordinary* usage of "fact", when someone is aware that p and unaware that q, the someone in question may always be said, pleonastically, to be aware *of the fact* that p but not *of the*

fact that *q*. Now mathematicians are aware that (1) holds but, not knowing much about model theory or modal logic, unaware that (4) holds. Hence according to ordinary ways of speaking, (1) is a fact of which they are aware, (4) a fact of which they are not aware. That makes them two different facts, and so any claim they are the same requires what P. F. Strawson would have called a *revisionary* metaphysics, as opposed to a *descriptive* metaphysics that merely follows ordinary usage. And *all* claims of revisionary metaphysics are highly contentious.

6.   *Objections and Replies*

Hints as to what Putnam may mean by "equivalent descriptions" are implicit in his 2002 critique of Burgess and Rosen, so let me turn to that barrage of criticisms, indicating my response to the four objections that seem to me most important, the last of which will bring the issue of "equivalent descriptions" back front and center. I will state the complaints in my own terms, as if made by an anonymous objector, and the responses in the first-person plural, though really I am speaking only for myself.

First Objection. Burgess and Rosen claim at one early point to be describing the naive, pre-philosophical view of mathematical existence assertions, but in so doing they speak of numbers as "things", and to do that *is already to import a contentious philosophical position.*

Reply. It is not to import anything, since naive, pre-philosophical subjects themselves *already* speak of numbers as "things". Perhaps the objector means something extraordinary by "thing", but we do not: There is nothing extra in *our* use of "things"

beyond the ordinary use of expressions on the order of "such things as numbers" and "things like numbers", and these phrases are found in unselfconscious, non-philosophical writing. To be sure, one cannot demonstrate this by Googling on those exact phrases for examples, because doing so unfortunately brings up mainly links to discussions of nominalism *vs* platonism, useless for illustrating anything about unselfconscious, non-philosophical usage. However, Googling on "such things as X" and "things like X" does turn up examples of unselfconscious, non-philosophical usage for all the following values of X: Euler's constant, quaternions, triangular numbers, Mersenne primes, measurable cardinals, perfect numbers, the golden ratio, Gaussian integers, Bernoulli numbers, algebraic integers, transfinite ordinals, and p-adic numbers. If one doesn't have to be a philosopher with a hidden agenda to speak of all these kinds of numbers as "things", then surely one doesn't have to be a philosopher with a hidden agenda to speak of other kinds of numbers as "things", either.

Second Objection. Burgess and Rosen, in the same discussion in which they call numbers "things", go on to imply, what is wildly implausible, that the plain sense of such an assertion as

(5)   There are prime numbers greater than a million.

amounts to something like

(6)   There are causally inert things that are not in space and time and which are prime numbers greater than a million.

Reply. We do not claim that the plain sense of (5) amounts to anything like (6), any more than we would claim that the plain sense of

(7)   There is a cat on the mat.

amounts to something like

(8)   There is a furry, four-legged, whiskered creature that is a cat on the mat.

The plain senses of (5) and (7) are (5) and (7), and if you want to say something whose plain sense is (6) or (8), you will have to say (6) or (8). What we *do* claim is that, just as it is implicit in the ordinary understanding of what cats are that they are furry, four-legged, whiskered creatures, so it is implicit in the ordinary understanding of what numbers are that they are not to be described as, for instance, exerting a gravitational attraction on the Milky Way, or being located at the center of the Andromeda galaxy. Philosophers would summarize such negatives using such expressions as "causally inert" and "not in space and time", just as linguists would characterize how English speakers form plurals using such expressions as "sibilant" and "voiced". In neither case is familiarity with the technical jargon of specialists being ascribed to the ordinary people whose thought or practice is being summarized or characterized. Our claim about what is implicit in the ordinary understanding of what sorts of things numbers are is open to challenge, but the objection as stated is not a challenge but a caricature.

The chief evidence for our claim is simply that people don't ordinarily go around

describing numbers as exerting gravitational attraction on the Milky Way, or being located at the center of the Andromeda galaxy, or whatever. Admittedly, our claim does predict something more than this, namely, that if exceptionally some eccentric *did* describe numbers in one of these ways, ordinary people would boggle. Admittedly, it is difficult to test this further prediction, simply because it is difficult to find anyone quite so eccentric as to go around making astrophysical assertions that bring in numbers as alleged *participants* in astrophysical processes. One has to resort to a hypothetical example. Let me do so now.

There is a famous problem in cosmology called that of "missing mass" or "dark matter". Observed gravitational effects suggest that there is something massive at the centers of galaxies that we cannot see because it does not emit light. One hypothesis that has been floated is that neutrinos, originally assumed to be massless, on the contrary have some nonzero mass, and that there are lots of them concentrated in the centers of galaxies. I take it this proposal is still under discussion, but let us imagine it comes to be recognized that it doesn't solve the problem. And suppose some bright young physics graduate student then comes forward with a novel hypothesis: It is not neutrinos but *numbers* that have some nonzero mass, and not neutrinos but *numbers* that are concentrated at the centers of galaxies. The proposal may even include conjectured formulas for the mass $\mu(n)$ and spatial coordinates $x(n), y(n), z(n)$ of the number n. What if you were in the position of the student's dissertation supervisor, hearing this proposal for the first time?

I am pretty sure that my own reaction would run much as follows: "Huh? Nonsense! [or an unprintable synonym] Is it April 1st? Or is this student crazy? Should I call the university counseling services? I hope I won't need to call campus security." Readers can judge for themselves, but those who imagine that they, too, would boggle should not find unreasonable our assumptions about what is implicit in the ordinary understanding of what sorts of things numbers are.

Third Objection. Burgess and Rosen, in their defense of the supposed ordinary belief in causally inert things outside space and time, raise an objection on grounds of complexity against proposed modal-logical alternatives to set-theoretic formulations. But objections on grounds of complexity would rule out virtually every analysis offered by philosophers from Frege onwards.

Reply. This objection overlooks the distinction we make between two kinds of nominalist. In contemporary linguistics it is common to associate with quite short sentences fairly elaborate structures that are supposed to be "psychologically real", though unconscious. Complexity in itself is no objection to analyses in linguistics, nor is it any objection to analyses in philosophy. But if a philosopher puts forward an alternative to a set-theoretic formulation as an *analysis*, then we call that philosopher a *hermeneutic* nominalist, and our objections against *that* kind of nominalist are not primarily based on complaints about complexity, but on complaints about the lack of *evidence* in favor of nominalistic analyses comparable to kind the evidence linguists advance for their hypotheses. Complexity is relevant only secondarily, insofar as more

complex hypotheses require more compelling evidence. We advance complexity as the *primary* objection against a *different* kind of nominalism, *revolutionary* as contrasted with hermeneutic, the kind of nominalism that proposes to *replace* current formulations with novel ones. And what could be more legitimate, when faced with such a proposal, than to protest that the novel formulations are very elaborate and awkward, if they are so?

Fourth Objection. Burgess and Rosen are not alive to the possibility of equivalent descriptions. The assumption that

(K)   Reality determines just *one* privileged language that we are to use when our interests are theoretical.

is so deeply ingrained in them, as in other analytic philosophers, that they are not even aware that they are making a potentially contestable assumption here.

Reply. Disambiguation is called for. The formulation (K), whose wording is taken *verbatim* from the 2002 lecture, admits of two readings:

(L)   Reality picks a language for us and requires us to use this one language in all theoretical contexts.

(M)   Reality requires us to pick a language for ourselves and use this one language in all theoretical contexts.

Here (L) is, nearly enough, the denial of the view of Quine, according to which theory forms a web on which reality impinges only at the boundary, through the evidence

sensory experience, leaving indeterminate how the web should be woven in the middle, both as to content and as to language, if indeed one can separate the two. No one but a dunce could read our approving quotations from Quine in the conclusion to our book and imagine that we subscribe to (L). Since the objector is not a dunce, we must assume that (K) in the objection is understood in the sense of (M).

I thus take it that the suggestion is that we do not accept the modal-logical formulation because we (rightly think that it cannot be accepted as an analysis and) wrongly think that if we did accept it we could only accept it as supplanting rather than supplementing the set-theoretic formulation. The charge seems to be that we overlook the option of accepting the modal-logical formulation while *keeping* the set-theoretic formulation, varying which we use from occasion to occasion, letting two if not a hundred flowers bloom, filling the world with pluralistic or at least dualistic sweetness and light. And all this tells me something about the content of the doctrine of equivalent descriptions, which has otherwise eluded my efforts to grasp it: Whatever exactly it amounts to, it is a doctrine subscription to which would make the foregoing seem a cogent objection. But *is* it a cogent objection?

7.   *Contrasting Pictures*

Do we overlook the option of just accepting the modal-logical picture *alongside* the set-theoretic? No, I say, because there is not really any such option for us to overlook. To articulate this more fully, let me contrast my view with a view that is "Putnamian" at least in the sense that a reader might well come away with it from reading MWF, whether

or not it is what the author of that suggestive but cryptic discussion had in mind, a matter about which I make no exegetical claims. But before presenting the rival views or pictures of mathematics, let me offer my own analogy with physics, and begin with two rival views or pictures of that science.

A first picture of physics is as follows: *Our best theory of gravitation is general relativity, and our best theory of the other fundamental forces is quantum field theory. These theories and their languages are incompatible or incommensurable. Yet we do not discard one and adhere to the other. Rather, we make use of both, the one in one set of contexts, the other in another set of contexts.*

This picture is false, and the true situation is as follows: Our best theory of gravitation is a body of expert opinion *about* general relativity, roughly to the following effect, that it provides the best available formalism for dealing with gravity, but needs a quantum correction no one at present quite knows how to give it, though fortunately the error is small, and the formalism remains usable in a wide range of applications. Similarly our best theory of the other fundamental forces is a body of expert opinion *about* quantum field theory. Our best theories are not incompatible or incommensurable, nor are they formulated in different languages. They are formulated in one comprehensive language in which we can both develop various formalisms and also express various opinions about them.

A first picture of mathematics is as follows: *There is no need to fix on a single language for thinking about mathematical reality. There are two available, set-theoretic*

and modal-logical, and a translation between them that lets one go back and forth to suit the occasion. If listening to nominalist rhetoric has raised qualms, one can tell oneself, "Oh, don't worry about the apparent ontological commitments of set theory. We could put everything modal-logically." If listening to extensionalist rhetoric has raised qualms, one can tell oneself, "Oh, don't worry about the apparent ideological commitments of modal logic. We could put everything set-theoretically." The key is to think of the two theories as complementary and not as in competition.

This picture is false, and the true situation is as follows: On the one hand, as already in effect indicated in my earlier objections to talk about "cognitive equivalence", if one just has the two languages and the mapping of sentences of one to sentences of the other, with no guarantee that this mapping is truth-preserving, then one has nothing that should allay anyone's qualms about anything. On the other hand, the only place to go for a proof that the mapping is truth-preserving seems to be a grand comprehensive background theory, combining all the commitments of set theory and modal logic. And if you go *there*, you no longer in any serious sense have a dualism of two languages: You have a monism of one great, big one.

As for what we accept, we accept set-theoretic mathematics, and reject modal-logical mathematics, as a *nominalist* might offer it, as an analysis of or replacement for set-theoretic mathematics. But we might be persuaded to accept modal-logical apparatus *in addition to* what we now accept. (We do at places in the book at least suspend disbelief in it for the sake of argument.) In that case, we would be accepting a grand composite

background theory, a modal set theory, and *it* would be our mathematics. The set-theoretic and modal-logical pictures would be merely components, together making up less than the whole, as synthetic geometry and pure algebra are merely components, together making up less than the whole, of coordinate geometry. But I cannot see us accepting set-theoretic mathematics and modal-logical mathematics *just side by side* without modal set theory in the background to provide a bridge between them.

The long and the short of it is this. Given two descriptions, if we have a comprehensive background incorporating both, we may be able to see that they are equivalent, but we do not then have in the two descriptions two languages or theories, but only two fragments of one comprehensive language and theory, together making up less than the whole. By contrast, if we have no such background, it is unclear that or how or in what sense we would be able to see that the two descriptions are equivalent. If Putnam has some clever way to evade this dilemma, it has escaped me.

Postscript.

Though the frequency of major reversals in Putnam's views lessened with the passing decades, he still showed a remarkable intellectual flexibility to the end. A few weeks after the deadline for contributions to this volume, and the submission of the present paper, one of the editors wrote to me to point out a couple of posts on Putnam's blog (putnamphil.blogspot.com) from late 2014 (dated 12 and 13 December) in which, repyling to an as yet unpublished criticism by Steven Wagner (unavailable to me at the time of this writing), he retracted his doctrine, upheld for well over forty years, that the set-theoretic and modal-logical pictures of mathematics are "equivalent descriptions". In particular, Putnam had come to acknowledge that the comparison with wave and particle pictures in quantum mechanics is not apt for reasons including one of those noted above, that whereas physicists are aware of *both* alternative pictures, mathematicians are only aware of *one*. He had come to prefer to describe the modal-logical picture as a "rational reconstruction" of the set-theoretic picture, preferring now to compare the relationship of the set-theoretic with the modal-logical pictures to the relationship of confused seventeenth and eighteenth century ideas about imaginary numbers with improved understandings that came in during the nineteenth century (complex numbers as simply pairs of real numbers). There are, nonetheless, several reasons to hope that the foregoing discussion of Putnam's former view may remain of some interest. For one thing, the retraction of the doctrine of equivalent descriptions was not accompanied by any retraction of the criticisms in his 2002 lecture. For another thing, the retraction was

limited to the mathematical case, while the above critical discussion, if cogent, applies more broadly. For yet another thing, the retraction appeared only in the blogosphere and not in the print literature, and to that extent it may be said that it was not fully "official". But mainly, any view maintained by a philosopher of Putnam's stature for the better part of a half-century must remain worthy of discussion even if its author eventually moves beyond it. For such reasons I am allowing the above text to stand unamended. I would have been greatly interested to see Putnam's response. Now, sadly, I can only wonder how his lively and original mind might have surprised us one more time.

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