

Hilary Putnam on Logic and Mathematics, by Geoffrey Hellman and Roy T. Cook (eds). Springer, 2018. pp. x+274.

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Putnam's most famous contribution to mathematical logic was his role in investigating Hilbert's Tenth Problem; Putnam is the 'P' in the MRDP Theorem. This volume, though, focusses mostly on Putnam's work on the philosophy of logic and mathematics.

It is a somewhat bumpy ride. Of the twelve papers, two scarcely mention Putnam. Three others focus primarily on Putnam's 'Mathematics without foundations' (1967), but with no interplay between them. The remaining seven papers apparently tackle unrelated themes. Some of this disjointedness would doubtless have been addressed, if Putnam had been able to compose his replies to these papers; sadly, he died before this was possible.

In this review, I will do my best to tease out some connections between the paper; and there are some really interesting connections to be made. Ultimately, though, my review will be only a little less bumpy than the volume itself.

1 Formal logic and mathematics

Goldfarb's paper is a short gem. In four-and-a-half sides, it provides a nice proof and discussion of one of Putnam's (1965) 'lesser-known but quite interesting' theorems (p.45): if φ is a satisfiable, identity-free, first-order formula, then there is a model of φ which interprets each primitive predicate of φ as a boolean combination of Σ_1 sets. This result is best possible for first-order logic without identity (by Putnam 1957), but it is still unknown whether the result holds for first-order logic with identity.

Friedman also offers us a purely formal paper; but, at 45 sides, it is ten times the length of Goldfarb's paper, and contains no mention of Putnam. Friedman provides us with examples of statements which are independent from ZFC, but more 'concrete' than the two most obvious examples, i.e. Con(ZFC) and the Continuum Hypothesis. (Of course, the MRDP Theorem tells us that we can always find an independent Diophantine sentence, whose 'subject matter' (cf. p.180) is unimpeachably arithmetical; but Friedman presumably regards these independent Diophantines as insufficiently 'concrete', since no one would be likely to entertain one unless they had been motivated to do so by a search for incompleteness.)

Here is perhaps Friedman's most striking example of incompleteness. Let $\mathbb{Q}[0, 1]^k$ be the space of k -tuples of rationals in the range $[0, 1]$. Now (pp.195, 209):

- For $a, b \in \mathbb{Q}[0, 1]^{2k}$, say that $a \approx b$ iff $a_i < a_j \leftrightarrow b_i < b_j$ for all $1 \leq i, j \leq 2k$.
- For $E \subseteq \mathbb{Q}[0, 1]^k$, say that S is an *emulator* of E iff both $S \subseteq \mathbb{Q}[0, 1]^k$ and $(\forall a \in S^2)(\exists b \in E^2)a \approx b$; an emulator of E is *maximal* iff it is not a strict subset of any emulator of E .
- For $S \subseteq \mathbb{Q}[0, 1]^k$ and $a, b \in \mathbb{Q}[0, 1]^k$, say that S is *drop equivalent* at a, b iff both $a_k = b_k$ and $(a_1, \dots, a_{k-1}, p) \in S \leftrightarrow (b_1, \dots, b_{k-1}, p) \in S$ for all rational $0 \leq p < a_k$.

The concrete independent sentence is then as follows (p.209): for all k and all finite $E \subseteq \mathbb{Q}[0, 1]^k$, there is a maximal emulator of E which is drop equivalent at $(1, \frac{1}{2}, \frac{1}{4}, \dots, \frac{1}{k}), (\frac{1}{2}, \frac{1}{2}, \frac{1}{4}, \dots, \frac{1}{k})$.

2 Philosophy of logic

Cook discusses Putnam on quantum logic. As usual, consider a particle in the classic two-slit experiment (with slits unmonitored), and with a formalisation key as follows:

r : the particle hit a certain region of the screen

s_1 : the particle passed through slit 1

s_2 : the particle passed through slit 2

On a certain reading of the experiment, Putnam suggested we will want to affirm $r \wedge (s_1 \vee s_2)$, but not $(r \wedge s_1) \vee (r \wedge s_2)$, and hence reject the (classical) law of distribution.

Cook's main line of response is that experimental data, considered in isolation, can never provide a counterexample to a law of logic, since there is always the question of how to formalise the experimental data. He works through this example in detail, suggesting that an intuitionist might not want to affirm $r \wedge (s_1 \vee s_2)$, and hence see no threat to the law of distribution.

I fully agree with the main thrust of Cook's paper. However, I am unsure that it much affects Putnam's discussion. First: Putnam himself never suggested that a law of logic would be refuted by experimental data considered in isolation; indeed, he was explicit that we can respond to the two-slit experiment either by changing the logic, or by accepting that our logical connectives do not map straightforwardly onto the algebraic operations of a Hilbert space (Putnam 1968: 179). Second, this is only to be expected: one way to state the Quine–Duhem thesis is that nothing can be falsified by experimental data considered in isolation; and if logic is empirical, then the same thesis applies to laws of logic. Third: I am a little wary of Cook's verdicts concerning intuitionism; as Cook himself worries (p.36), whatever tempts us to see a counterexample to the law of distribution is likely to tempt the intuitionist to assert all of r , and $\neg(\neg s_1 \wedge \neg s_2)$, and $\neg((r \wedge s_1) \vee (r \wedge s_2))$, which is intuitionistically inconsistent.

Shapiro continues his welcome project of bringing Waismann back into the fold of analytic philosophy, focussing on Putnam's early work on the analytic/synthetic distinction. The three-way comparisons between Putnam, Quine and Waismann are certainly rich, and I particularly appreciated the following point: both Putnam and Waismann thought that notion of 'meaning' was usable and useful, but too 'blurred' (Waismann) and insufficiently 'refined' (Putnam) to deliver verdicts to every question of the form 'do these expressions have the same meaning?' I would add that, whilst Shapiro restricts his discussion to Putnam's work from the 1960s, Putnam continued to develop this broad theme in many of his later works.

Let me just register one quibble. To illustrate Putnam's 1962 notion of an analytic-definition, Shapiro suggests that, in 1840, 'marriage' was a one-criterion word (to use Putnam's phrase) with the following criterion: 'marriage is a (holy?) relationship entered into by a man and a woman that is sanctioned as such by a legal body with relevant jurisdiction' (p.122). This seems wrong: plenty of people, in 1840 and before, regarded at least some polygamous and same-sex marriages as marriages (see e.g. Eskridge (Jr.) 1993). 'Marriage' has never been a one-criterion word; rather, as Haslanger (2006: 114) notes, it has always been 'a framework concept that links the institution to a broad range of other social phenomena', and hence was and is just as open-textured as any scientific law-cluster term. (Shapiro [private communication] agrees with this point, and is confident that Waismann would too.)

3 History and philosophy of mathematics

Detlefsen explores Pasch's and Hilbert's different conceptions of mathematical rigour. In brief: Pasch regarded axioms as contentual, but asked us to abstract from their specific contents when aiming at gapless proof; Hilbert regarded axioms as content-less formal objects to be manipulated symbolically.

The body of Detlefsen's paper contains no mention of Putnam; but its postscript presents a poignantly missed invitation for Putnam to have engaged with this material. The background is that Putnam (1984) once presented a position he called *Hilbert's Thesis*: the (formal) notion of *first-order derivability* is extensionally equivalent to the (informal) notion of *deducibility*. Putnam stated that the Completeness Theorem for first-order logic provides good evidence for Hilbert's Thesis; presumably he had in mind Kreisel's (1967: 152–5) squeezing argument. Detlefsen, though, notes that even if Hilbert's Thesis is extensionally correct, we still face the question of how to regard the deductions and axioms: do we follow Pasch, Hilbert, or someone else?

Davis also explores the history of mathematics, considering Putnam's (1975c) contention that mathematics uses 'quasi-empirical' methods. I found it slightly unhelpful that Davis referred to these as 'inductive' methods, not least because Davis offers a splendid explanation of why mathematicians should take very little solace from evidence obtained by mere enumeration of cases computed to date (pp.151–3). But, terminological quibbles aside, Putnam's and Davis's point is that mathematicians have often been willing to go out on a limb, operating fruitfully and successfully in the absence of (what we would now call) firm justification or foundation. The main examples Davis considers are: the use of imaginaries in solving cubic equations with Tartaglia's formula; Toricelli's use of 'indivisibles' in proving the finite volume of his infinitely long trumpet; and Euler's solution of the Basel problem, i.e. $\sum_{n=1}^{\infty} \frac{1}{n^2} = \frac{\pi^2}{6}$.

Davis then goes out on an interesting limb himself, suggesting that these considerations undercut certain pictures of mathematical knowledge:

If presented with a proof that Peano Arithmetic is inconsistent or even that some huge natural number is not the sum of four squares, I would be very very skeptical. But I will not say that I *know* that such a proof must be wrong. (p.155)

Now, surely Davis would say that every natural number *is* the sum of four squares; that, after all, was proved by Lagrange. But why, then, not say that he *knows* that any putative proof to the contrary 'must be wrong'? Davis's answer is as follows: since we humans have only little brains, our theorising about natural numbers is (of necessity) a kind of idealized exploration of 'simple austere worlds' (p.155); but this gives rise to the worry that some incomprehensibly big number might behave in ways that we cannot even imagine.

This worry strikes me as almost—but not quite—intelligible. For my money, anything which would count as a natural number sequence must obey induction, and Lagrange's proof holds for any sequence which obeys induction. But this short reply does not rob Davis's worry of significance. On the contrary, its almost-intelligibility tells us something important about our attitude towards mathematics.

To see what it tells us, compare Davis's worry with Putnam's (1979: 432) description of a situation in which you rationally (but mistakenly) come to believe that someone has proved the inconsistency of Peano Arithmetic. Putnam's point was not that the possibility of such a situation threatens our present knowledge of Peano Arithmetic's consistency. What it does show,

though, is that ‘we can make sense of the question “What would you do if you came across a contradiction in Peano Arithmetic?” (“Restrict the induction schema”, would be my answer.)’ But we could not make sense of this question, if we regarded arithmetical truth and consistency as merely conventional.

McCarthy’s paper revisits Putnam’s famous (and I think decisive) rebuttal of the Lucas–Penrose argument against mechanism. McCarthy’s first novel move is to provide a more plausible anti-mechanist argument, the *Soundness Argument*, which I formulate as follows (see pp.96–7):

- (a) I am represented by T (some formal axiomatic theory).
- (b) I am warranted in appealing to T ’s axioms and rules of inference.
- (c) I can recognise that T only proves truths.
- (d) I can recognise that T does not prove ‘ $0 = 1$ ’.
- (e) I can recognise that T is consistent.

Plainly, (a) is version of mechanism. But, since I do not simply churn out uninterpreted symbols in the language of arithmetic, the notion of ‘representation’ used in (a) should be somehow *normative*. This normativity is supposed to entail (b). Then (b) apparently entails (c), by ‘the standard inductive definition of proof in T ’ (p.96), and (d) and (e) follow in turn. But Gödel’s second-incompleteness theorem shows that (e) and (a) are in conflict. What has gone wrong?

McCarthy’s first observation is that there is a potential gap between (b) and (c). After all, I could be able to say, of each of T ’s axiom and inference rules, that they are sound, without being able to say that T in its totality is sound. Reflecting further on this gap, McCarthy is led to a deep, limitative result on frameworks for considering how (what we treat as) axioms might change over time. In particular, no consistent frame can be all three of:

- *effective*: i.e. the relation for revising axioms is computable.
- *solvable*: i.e. for any states, some state is accessible to both, by the revision-relation.
- Π_2 -*complete*: i.e. each true Π_2 sentence is assertable.

Effectiveness is a weak thesis of mechanism; and McCarthy argues convincingly that solvability is both plausible and desirable (p.106ff); so mechanists should reject Π_2 -completeness.

As I see it, this ties neatly into one aspect of Davis’s paper. If crude enumerative induction were an acceptable way to obtain arithmetical knowledge, in general, then any Π_2 truth should be (in principle) assertable, so that the appropriate frame would be Π_2 -complete (cf. p.103). Continging, if solvability is desirable and we want to allow for the possibility of mechanism, then we have another reason to refuse to regard enumerative induction as a generally acceptable way to obtain arithmetical knowledge.

4 Putnam’s model-theoretic arguments

Two papers in this volume concern Putnam’s model-theoretic arguments; but from rather different points of view. Kanamori provides a very nice, self-contained paper on Putnam’s construction argument. The gist of Putnam’s (1980) original argument is this :

- (a) we can treat all of the physical magnitudes that we ever measure as a countable set of real numbers; let r be a single real number coding this set;
- (b) invoking some model theory, we can find an ω -model of $ZF + V = L$ which contains r ;
- (c) so no amount of physical evidence could suggest to us that $V = L$ is ‘really’ false.

And this leads to a rhetorical question: what does a moderate platonist even mean, if they say that $V = L$ is 'really' false?

Compared with Putnam's permutation argument, or his use of the completeness theorem, the constructivisation argument has received relatively little commentary, but Kanamori takes it all in rather nicely. He focusses mostly on Bays's (2001) criticism of the argument, which runs roughly as follows. At step (b) of the argument, Putnam should be clear on the theory, T , within which he proves the existence of the model. On Gödelian grounds, T must be stronger than ZF itself. But in that case, the moderate platonist will not be troubled by some strange model of ZF; she should only be troubled by some strange model of T ; and, on Gödelian grounds again, T itself cannot prove the existence of any models of T .

Kanamori replies to Bays by suggesting that all Putnam needs is the conditional 'if there is a model at all, there is an unintended one' (pp.240, 242, 245). As Kanamori notes, I had used this line to defend Putnam myself (2011: 330); but I had mistakenly thought this conditional could be used only to defend Putnam's permutation argument and his use of the completeness theorem, and not his constructivization argument; I embrace Kanamori's correction of my error. In broad brush strokes: if the moderate platonist accepts that there is a model of her favourite theory, T , then we can simply follow Putnam's argument to the conclusion that there is a model of $T + V = L$ containing r ; if she denies that there is a model of T , then she had better stop advocating T anyway. (And, for the sake of completeness, let me add: if the moderate platonist refuses to subscribe to any particular theory, T , then she gives up on being a *moderate* platonist; see Button and Walsh 2018: 175.)

Hodesdon's paper considers the way in which Putnam's model-theoretic arguments were intended to show that metaphysical realism (*tout court*) is 'empty'. After some interesting reflections on the connection between internal realism and transcendental idealism, Hodesdon suggests that Putnam's complaint boils down to the following: 'when the metaphysical realist makes a claim, she can't guarantee that her interlocutor, who holds a different theory of truth, will interpret her claim as she intends' (p.87). I cannot, though, believe that this is the correct reading of Putnam, since it is straightforwardly question-begging. Indeed, the metaphysical realist would be right to reply to this complaint as follows: 'It doesn't matter what you, or I, *take* to be the theory of truth. If metaphysical realism is right, then there is a correct theory of truth. So if you interpret me using some other theory, you *misinterpret* me.'

Now, Hodesdon (pp.85–6) offers her reading of Putnam having discussed and rejected mine; but she somewhat misunderstands my position, so it might help if I briefly clarify it (see my 2013: pt.A). Consider a claim like 'causation fixes reference'. That may indeed be part of an ideal theory of the world; but, as such, it is up for model-theoretic reinterpretation. (It is just more theory.) To fix reference, then, we might insist that 'causation fixes reference' is not merely ideal, but also true. External realists, though, think that there is a deep gulf between what is ideal and what is true. So, by their own lights, insisting on the truth of 'causation fixes reference' would go beyond anything for which anyone could possibly have any evidence. (They might as well say 'magic fixes reference'.) This forces external realists to worry that reference might be radically indeterminate. But this worry is empty(/incoherent), for one literally could not make sense of the worry if it obtained. And this is why external realism itself is empty(/incoherent).

5 Modal logic and set theory

To close this review, I will consider the three papers which discuss Putnam's 'Mathematics without foundations' (1967). There, Putnam argued that we could approach set theory either modally or non-modally; but that these were 'equivalent'; so that there is no need to choose one or the other picture as a foundation for mathematics. This is a particular instance of a general Putnamian theme; on similar grounds of 'equivalence', he elsewhere argued that there is no need to choose between:

- (a) treating sets or functions as foundational;
- (b) treating points or lines as fundamental;
- (c) formulating Newtonian physics with or without fields;
- (d) mereological nihilism or universalism (for references, see my 2013: 199).

However, all three of the papers which discuss 'Mathematics without foundations' focus only on the case of set theory. Moreover, all three disagree with Putnam, explicitly favouring modal over non-modal formulations of mathematics.

The subtitle of Burgess's paper, 'Models, modals and muddles', sets his tone: he is not happy. His main complaint concerns the sense in which Putnam holds that modal and non-modal set theories are 'equivalent'. The best sense he can make of it is that, if 'one adopts some grand comprehensive background theory, one might be able to see that the two theories are equivalent; but to adopt a grand theory would then be to advocate a (different) form of monism after all' (p.141). Indeed; but I take it that Putnam was not advocating for such 'grand theory' monism. Instead, Putnam is best read as suggesting that each picture can interpret the other, and that the adequacy of each interpretation can be seen from within each picture, with no need to ascend to some 'grand theory' (cf. my 2013: chs.18–19).

Linnebo embraces this reading of Putnam. However, he first argues (very convincingly) that Putnam's point can be better made by formulating modal set theory along the lines of Parsons (and subsequently developed by Linnebo and Studd). In this setting, Linnebo points out, we can set up modal and non-modal set theories which are mutually faithfully interpretable. (To say that theories are mutually interpretable is to say that each can interpret the other, in such a way that translations of theorems are theorems, and translations of non-theorems are non-theorems.) This provides a precise sense in which the two pictures are 'equivalent'. But Linnebo then rightly notes that, in general, theories can be mutually faithfully interpretable, but distinct enough for us to want to choose between them. And, in particular, Linnebo claims that modal set theory deals better with the set-theoretic paradoxes than non-modal set theory.

Linnebo's paper is thoroughly enjoyable, but let me say a few words on Putnam's behalf. In the case of modal versus non-modal approaches to set theory, we can find a tighter equivalence than (mere) mutual faithful interpretability: the right theories can be shown to be (something like) definitionally equivalent (see Button MS). Now, the general point remains, that theories can be equivalent in this formal sense, but distinct enough in some informal sense for us to want to choose between them. In the particular case of modal versus non-modal set theories, however, our grasp of the subject matter is so thoroughly dependent upon our formal theories, that it is not implausible (to me, at least) that there really is nothing to choose between these theories.

Like Linnebo, Hellman & Cook think that modal set theory handles the set-theoretic paradoxes better than non-modal set theory. They then use this as a springboard to the idea that we might of-

fer a modal treatment of the liar paradox. The general idea is that there is no most-encompassing interpretation of a language, since necessarily any interpretation can be extended (p.66ff).

All such approaches to paradox encounter a familiar concern, which we can state as follows: *When someone attempts to resolve paradoxes by saying 'there is no all-encompassing interpretation', how should we interpret that claim?* Hellman & Cook's (pp.68–70) response to this concern is that, when they attempt to talk about all possible interpretations, they are merely mentioning all possible interpretations, without using them, and that this is unproblematic.

I worry, though, that Hellman & Cook are in fact committed to the (mind-boggling) view that they cannot even mention all possible interpretations (not even if they say 'we cannot mention all possible interpretations'). Here is why. Fix any interpretation, \mathcal{M} , for the claim: 'necessarily, any interpretation can be extended'; so \mathcal{M} provides us with a potential hierarchy of interpretations. But Hellman & Cook hold that, necessarily, no interpretation is maximal. So they must accept that \mathcal{M} is not itself maximal. Now, a strictly richer interpretation than \mathcal{M} would have provided a strictly richer potential hierarchy of interpretations. So \mathcal{M} fails to mention all possible interpretations. Since \mathcal{M} was arbitrary, we seem to arrive at the general conclusion: by their own lights, Hellman & Cook cannot mention *all* possible interpretations.

A similar worry arises for Linnebo. Linnebo's suggested resolution of the set-theoretic paradoxes is similar to Hellman & Cook's, since he advocates 'an interpretational understanding of mathematical modality' (p.264). But Linnebo is committed to the view that, necessarily, no interpretation is maximal. So no interpretation of his favoured modal set theory is maximal. Generalising, it seems that his modal set theory must fail to talk about *all* possible sets.

These objections carry force, though, only if we assume that our languages stand in need of (model-theoretic) interpretation. One response, then, is just to deny this assumption, and to maintain instead that a language which we know how to use is a language we already understand. That would be pleasingly consonant with the lessons Putnam (1980: 481–2) drew from his model-theoretic arguments.

As forewarned, my review of *Hilary Putnam on Logic and Mathematics* has been somewhat disjointed. To close, perhaps I should offer an overall verdict on who should buy this book. In all honesty, I am not sure. This is not because of an absence of good philosophy; on the contrary, I hope it is clear that I have very much enjoyed many of the individual papers, and have learned a great deal from attempting to join some of the dots between them. But I am simply not sure who will take enough from the book, as a (disconnected) whole, to justify Springer's hefty price tag. (That price tag is especially unreasonable, given the high number of copy-editing mistakes, and the distressingly poor typesetting of formal logic and mathematics. Springer needs to up its game.)*

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