REVIEW: GEOFFREY HELLMAN, *MATHEMATICS AND ITS LOGICS*

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1. Introduction

Mathematics and its logics is a collection of Geoffrey Hellman's papers. The volume covers a huge variety of topics: there are essays on the foundational role of category theory, on the nature and value of restrictive programs in foundations of mathematics such as nominalism and predicativism, on modal structuralism, on the Quine-Carnap debate, on intuitionism and smooth infinitesimal analysis, and much more besides. Hellman's important contributions on all of these topics are reflected in the collected papers, and in addition they tend to contain excellent introductions to the relevant debates along the way. As a result, the book would make a valuable addition to the library of any graduate student or professional with a specialization in philosophy of mathematics.

The book is split into three parts reflecting different aspects of Hellman's work. Each part consists of a series of papers with a common theme. Most of the papers are very well known, have been highly influential, and have already been subjects of much discussion in the literature. However, in addition, each part also offers a newly written paper. One of these is a critical piece, responding to recent work by Penelope Maddy. The other two push at the frontiers of the existing work showcased in parts one and two. Since the older works are already so well known and have been so extensively discussed, after summarizing the papers offered in each part, in this review I will go on to discuss the contributions made by the two new non-critical papers.

2. Overview

Part I consists in papers related to Hellman's modal structuralism and surrounding issues. The first paper of part I, *Structuralism without Structures*, is one of the founding works in the theory of modal structuralism; in it, mathematical foundations are developed in detail for many areas of mathematics in modal structuralist terms, and the philosophical credentials of modal structuralism are assessed. (The paper also contains interesting points of comparison between modal structuralism and the predicativst viewpoint, which is itself the topic of scrutiny in part II.) The second paper, *What Is Categorical Structuralism?*, presents a comprehensive discussion of the role of category theory in foundations of mathematics, arguing (against Colin McClarty and Steve Awodey) that ultimately category theory is in an important sense lacking in this regard. Papers three and four, *On the Significance of the Burali Forti Paradox* and *Extending the Iterative Concept of Set*, discuss the set-theoretic paradoxes from the potentialist point of view, making the case that the 'height potentialist' perspective implied by modal structuralism allows for a more satisfying solution to the set-theoretic paradoxes, as well as clearer justifications for certain standard axioms of set theory, than are afforded by the more common 'actualist' view. Papers five and six are related to nominalism, and (among other things) are concerned to defend the particular form of nominalism implied by his modal structuralism.

Part II offers a series of four papers exploring the nature and value of predicative foundations for mathematics. The first paper, *Predicative Foundations of Arithmetic*, is joint with Solomon Feferman. It develops the first-order theory of arithmetic and proves its categoricity using an axiom system they call EFSC, the 'elementary theory of finite sets and classes', which is argued also to be predicatively acceptable. The framework developed presents a beatuifully economic account of the natural number structure from a remarkably austere collection of principles. In addition, the presented results may also be of relevance to contemporary discussions in philosophy. The recent discussion of 'internal categoricity' results and their philosophical significance, for instance, tends to take it as read that *impredicative* comprehension is needed to secure categoricity in this way, whereas the work done by Feferman and Hellman seems to suggest that one can do with less. A little bit – in the form of EFSC – goes further than one might expect.

The second paper of part II, *Challenges to Predicative Foundations of Arithmetic*, is also joint with Feferman. It considers various philosophical objections raised to predicativism in the light of the mathematical work done in the first paper, and also presents some further technical developments contributed by Peter Aczel. The third paper, *Predicativsm as a Philosophical Position*, is sole-authored. Perhaps unsurprisingly, it presents a more critical examination of predicativism as a philosophical position, arguing that ultimately the central contribution of predicative foundations is to emphasize the *dispensability* of the uncountable from the point of view of modern science, rather than providing an independently coherent and plausible foundation for mathematics. The fourth paper, *On the Gödel-Friedman program*, is newly written for the volume. It continues the analysis of the previous article in a more general context, in particular that of Harvey Friedman's program for justifying large cardinal axioms by appeal to their utility in solving undecidable problems at the level of the naturals. Beyond the insightful discussion of Friedman's program, the article moves into novel and interesting territory regarding the epistomology of large cardinals, and in particular sets out the outlines of a Bayesian approach to justification here.

Part III is a little less homogeneous than the previous two parts. It presents five papers in the domain of philosophical logic. The first, *Logical Truth by Linguistic Convention*, offers a defense of the Carnapian thesis that first-order logical knowledge is in a significant sense 'analytic'. The second, *Never say "Never"!*, argues that intuitionism is an important sense not self-contained, since it turns out to rely on a 'non-intuitiontistic' understanding of infintary quantification. The third paper, *Constructive Mathematics and Quantum Mechanics: Unbounded Operators and the Spectral Theorem* continues the assault on intuitionism by arguing that constructive mathematics is incapable of proving results needed in the foundations of quantum mechanics, especially the so-called 'spectral theorem'. Paper four, *If "If-Then" then what?*, discusses Maddy's recent attempts to revive (and extend) the classical position of 'If-Then-ism' in foundations of mathematics. Finally, the fifth paper, *Mathematical Pluralism: The Case of Smooth Infinitesimal Analysis*, discusses the issue of how to understand the logical connectives in SIA, and argues that ultimately they must be understood as only as implicitly defined relative to a presupposed classical meta-theory, thus amplifying the theme in this part that intutionism and intuitionistic theories are, taken independently of a classical meta-translation, ultimately inadequate.

As this summary makes clear, the book addresses a great selection of topics, too many to sensibly discuss here. Instead, I will now turn to a discussion of two points of interest arising in two of the newly written papers published in the volume.

3. On the justification of infinity

One of the central claims of the fourth paper of part I, *Extending the Iterative Concept of Set*, is that the 'height potentialist' in set theory is able to offer a better justification for the axioms of infinity and replacement than is available on the standard 'height actualist' framework.

The stage is set with Boolos' famous paper, *The iterative concept of set*, as the backdrop. There, Boolos shows how certain axioms of standard set theory can be motivated by appeal to the idea that sets are to be thought of as 'formed in a well-ordered series of stages', wherein at each 'stage' of the process of set-formation one forms all possible sets of things formed at prior stages.

However, not all axioms of standard set theory were derivable from Boolos' axioms. Saliently, Boolos abandons the axiom of replacement altogether; more subtly, in order to secure infinity, he has to write in the existence of an infinite stage as a new axiom in the stage theory.

Like all standard axiomatic set theories, Boolos' stage theory is *(height) actualist.* This means that it doesn't make room for a substantive notion of a *merely possible* set or stage of set formation. Rather, there simply are the sets and stages of formation that there are; there is no sensible question to ask about whether or not there might in addition be more possible sets or stages.

The *(height) potentialist* framework, on the other hand, says that no matter what sets there are there could always be ones of higher rank; in terms of stages, this says that no matter what stages of construction have been carried out, more such stages are possible. The idea is one of a never ending, 'potential hierarchy' of sets, as opposed to the standard actualist idea of The Cumulative Hierarchy.

The paper at issue makes the striking suggestion that height potentialism might be able to do better when it comes to justifying infinity and replacement than Boolos or the height actualist more generally is able to. Now as a matter of fact I agree with Hellman's claim here; but I am not convinced by his reasoning. Let me explain why. For the sake of simplicity, I will focus my discussion on infinity; analogous though more tedious-to-state points stand to be made about replacement as well.

How might height potentialism help with deriving the axiom of infinity? First, it is important to note that in one very mundane (but important) sense, it necessarily cannot help. For the standard axiom of infinity says, simply and unapologetically, that there *exists* an infinite set. And many height potentialist theories, Hellman's own brands included, tend not to have this as a consequence, and inded they tend not to even imply there exist *any sets at all.* On potentialist theories, in general, sets are possibilia, and so the relevant 'axiom of infinity' does not assert that infinite sets *exist*, but rather only the 'potentialist translation' of the standard axiom, that infinite sets *are possible*. This latter is really the principle whose justification is at issue in the paper.[1]

Now is probably a good time to explain why *I* believe that the height potentialist has available a justification for the axiom of infinity not available to the actualist. That is simply because their axiom is a *weakening* of the corresponding actualist principle. The height potentialist only needs to secure the *possible* truth (in whatever sense is at issue) of the actualist's axiom. So obviously routes of justification will be open to one but not the other, though exactly how this plays out will depend on the brand of potentialism (and the sense of possibility) at issue.

But this simple thought is not Hellman's. According to Hellman, there are principles statable in the language of his modal stage theory – essentially, that of modal, plural logic with vocabulary for stages and sets – that are at once significantly more self-evident than the potentialist's axiom of infinity and that nevertheless imply it as a consequence. It is this claim I am somewhat doubtful of.

Hellman considers one axiom of this kind that he ultimately rejects on grounds of its being 'too close' to the sought conclusion to really gain any ground. This is the assertion:

> **A:** It is possible that there are some stages ss such that the empty set is formed at one of them, and every stage in the ss is succeeded by another stage in the ss.[2]

In the context of the rest of the stage theory, and in particular the axiom that every stage contains all possible sets from among things at the previous stage, this readily implies there is a least stage with infinitely many stages before it, and with it many infinite sets. But, according to Hellman, this axiom is

> "Too close for comfort to the conclusion sought. Once you have an infinity of compossible things... not surprisingly you generate a set of those things at the next stage."

What then is Hellman's alternative? To introduce it, we need the following definition. Say that a property $P(ss)$ that applies to stages ss is *Indefinitely Extendible* (IE) iff each of

> **B:** $P(ss)$ is possible,
>
> **C:** given any possible stages ss with $P(ss)$, it is possible to find a t after all ss with $P(ss+t)$

are true. In the latter of course $ss+t$ is just the ss together with t (which will be supplied by our plural comprehension axioms). The *rough* idea is that P is an IE property when it is always possible to find further stages to which it applies: so no matter how far you go on in the process of set formation, you will always eventually find new series of stages that satisfy P.

Now, let N be the predicate

> Every stage in the ss is one at which a natural number is formed

Then the axiom Hellman proposes is

> **HA:** N is not IE

[1]Hellman does not this in the article, but in a rather off hand way; I think perhaps it might have been emphaszied more, and earlier.

[2]Following Hellman, I will use double letters xx to range over pluralities, and will tend to use s, ss when the intended range is stages.

Let us see what **HA** amounts to. Substituting N for P in IE and negating we get the negation of one of B or C with $N = P$. The resulting negation of B is demonstrably false given the rest of Hellman's stage theory, so we are left with (up to trivial grammatical reshuffling)

HA$^+$: Possibly, there are stages ss where every one of ss witnesses the formation of a new natural number, and such that no later stage witnesses the formation of a natural number.

Using the fact that the rest of Hellman's theory implies (i) it is possible for there to be a stage s_n at which each particular natural number n is formed, and (ii) that the stages of set construction all have successors, one can easily go on to show that there must possibly be a stage at which the set of all natural numbers is formed. But this is the potentialist version of the axiom of infinity we set out to secure.

Nevertheless, Hellman claims that **HA** as more plausible than **A**. Indeed, he thinks of the just cited derivation af **A** from **HA** as a *non-trivial proof* of the potentalist's axiom of infinity. As a result he believes the stage-theoretic potentialist has a special advantage to offer when it comes to the justification of that axiom (and the replacement scheme).

One (fairly minor) issue with this argument is that it is not altogether clear that distinctively stage-theoretic ideas are essential to it; in fact, they may serve to complicate the underlying point. For instance, in the stage-free setting of Øystein Linnebo's modal set theory, it is natural to define indefinite extendability of a condition φ to mean that (a) at least one thing φs, and (b) for any plurality xx of φ-ers, it is possible to find a φ not among the xx. And using this definition, inspection of Linnebo's axiom of infinity reveals it is effectively just the assertion 'N is not IE', where N is a formula defining the finite ordinals. Thus, insofar as Hellman has a case here, there seems to be a corresponding case to be made in the simpler stage free setting. (Although perhaps the argument is still best given in the staged setting, if talk of stages allows a cleaner justification for the other axioms.)

Another more substantive issue is that it is somewhat unclear whether there is in fact signficant 'cognitive daylight' between **A** and **HA**; that is, whether one of these principles might plausibly be easier to justify than the other.

Let me explain why. Hellman's stage theory is committed to

E: For each natural number n, it is possible to find a stage s_n at which the finite Von Neumann ordinal n is formed.[3]

The question at issue is whether in addition it is possible to form all such stages 'at once'. **A** says 'yes' quite straightforwardly: it asserts that it is possible to run through all the finite stages of the set construction process. In the context of **E** on the other hand, **D** seems just another way of saying 'yes' but more bentbackwardly, by saying that eventually the stages that witness formation of natural numbers *give out*, even though (as the stage theory requires) the stages continue. But (given **E**) this really seems a trivial restatement of the claim that eventually you get all of them. You can say eventually you get all possible stages at which natural numbers are formed; or, that eventually you necessarily won't get any more stages at which natural numbers are formed. But in either case, so long as you also have that every natural number is formed at some possible stage, it seems to me you are

[3] This is a slightly loose and tendentious way to put things, since it appeals to natural numbers in the metatheory. But the same points can be made, in a more subtle way, without this crutch.

saying essentially the same thing, and it is therefore rather hard to imagine anyone uncertain of the one statement being reassured by appeal to the other.

It thus seems that the axioms **A** and **HA** come to more or less the same thing, so if one is 'too close' to the sought conclusion to really advance the debate then so is the other. This is not to say, though, that the potentialist is no better off than the actualist, since as I have already stressed they will in general have a simpler task in justifying their weaker form of the axiom of infinity. All they need is the *logical possibility* (say) of an infinity of sets existing together, which is surely a lighter weight to lift than the *actual existence* of the same.[4]

4. On Bayesianism and Large Cardinals

The second theme I wanted to pick up on in this review concerns an interesting strand of argument in the book that plays out in part II concerning justification of belief in the existence of large cardinals.

One possible path to justification here, that has its roots in ideas of Gödel, is that one may be able to justify belief in large cardinals by a kind of intra-matheamtical indispensability argument. The idea is to show that there is a significant class of important 'low level' mathematical problems (say at the level of the integers) that are unsolvable except by appeal to large cardinals, and then to defend belief in large cardinals on grounds they are needed to solve the relevant problems (or even just allow for simpler arguments than are otherwise available).[5] In its initial Gödelian form, this idea was developed using consistency statements for theories of interest as the 'important mathematical problems' left open by set theory without large cardinals but solvable in set theory with them. This initial form is open to natural concerns about whether or not consistency statements are 'mathematical' enough to constitute 'important mathematical problems'; but, since Gödel, many further results have been found that do better on this score, including e.g. the Paris-Harrington theorem, problems like the 'mortal matrix' problem in linear algebra, and lots of results in topology and analysis. At the cutting edge of these we have various combinatorial results about the integers and rationals which Harvey Friedman has shown to be equivalent to fairly strong large cardinal axioms; these include a case discussed by Hellman in detail, in which a statement about the integers in the field of Boolean relation theory is shown to (in a sense!) require the existence of an n-Mahlo cardinal for each natural n for its proof to go through. The 'Gödel-Friedman', as Hellman calls it, aims to marshal such results as evidence in favor of the existence of the corresponding large cardinals.

Now I parenthetically remarked 'in a sense' above, for the reason that it is only in terms of *consistency strength* that large cardinals are needed. In the case I just mentioned, for example, it is true that the proposition in Boolean Relation theory follows from the assumption of the existence of an n-Mahlo cardinal for each n, and that it will not follow from a weaker large cardinal axiom because the

[4] In his comments on this review, Hellman agrees that there is an issue here, and has suggested that a more refined argument, stated in a richer vocabulary and employs a broadly Dummettian ideology of indefinitely extendible concepts, can be used to better explicate the key idea here. It would be very interesting to see this idea developed in full detail, and I hope we shall see something along these lines in the future.

[5] Of course Hellman himself, like Putnam in fact, ultimately finds such arguments dubious on grounds that abstract mathematical objects are not needed anywhere in light of the possibility of a modal-structural translation of mathematics.

proposition itself implies the consistency of the schema (in n) to the effect that an n-Mahlo exists. This gives a *sense* in which the relevant large cardinal hypothesis is necessary in proving the proposition. But of course this *does not* mean the large cardinal hypothesis really is a necessary condition for the proposition, since in fact it is enough simply to assume that the relevant hypothesis *does not have any false arithmetic consequences*, which is a much weaker assumption and itself a statement at the level of the integers. And it is completely obvious that this situation is general: whenever you have some statement at the level of the integers that seems to need a large cardinal axiom to prove, one will be able to get by with the much weaker assumption of conservativeness over the arithmetic fragment. Large cardinal hypotheses don't need to be true to be useful, they just need to not mislead. In his work, Solomon Feferman repeatedly appealed to such observations as a way of 'defanging' the Gödel-Friedman program.

The last two papers of part II, and especially the last one, contains the germ of an idea for how to overcome this kind of problem by appeal to Bayesian confirmation theory. The central suggestion is that the famous 'Bayes rule'

$$P(H|E) = \frac{P(E|H) \cdot P(H)}{P(E)} \tag{1}$$

may somehow be invoked to give a 'boost' to the subjective probability of large cardinal hypotheses, even given the foregoing observations.

Hellman's idea is to appeal to results like those of Friedman to 'increase the posterior rational credence' of the hypothesis that large cardinals exist, *via* Bayes rule. According to Hellman, in the role of evidence (the E in Bayes' rule) we may take syntactic claims like (say) the schema (in n) asserting that the existence of an n Mahlo cardinal only has true arithmetic consequences; this, says Hellman, is a proposition to which we have 'epistemic access', 'arising in a variety of contexts' in such a way that plausibly we can hope to 'confirm... it through its own fruitfulness in other applications'. (This is in contrast e.g. to Friedman's proposition from Boolean relation theory, which Hellman does not think has these properties.) Hellman adds that besides this consistency claim, in the case at hand, we may throw in additionally 'consistency properties of other cardinals whose existence is derivable from the Mahlos' – including inaccessibles, hyper-inaccessibles and so on – to further enrich the evidence. In the role of the hypothesis (H) of course we will just have the large cardinal hypothesis in question, namely the existence of an n-Mahlo cardinal for each n.

Hellman doesn't run through how the application of Bayes rule in such cases is supposed to go in detail, but I think it is illuminating to do so – and reveals why application of the rule in mathematical cases in particular is perhaps more problematic than traditional empirical applications. In this case, it is clear that $P(E|H)$ is 1, so this drops out. We are left with the claim that one's degree of belief in the claim that the n-Mahlo cardinals exist, given they are arithmetically conservative, should be at least the result of dividing one's degree of belief that they exist by one's degree of belief that the corrsponding scheme is arithmetically conservative. (Technically one could add to the E side the other consistency properties mentioned by Hellman, but these would be redundant for the obvious reason that they are all implied by the consistency property already mentioned, and so conjoining them will not alter the probability of E.)

At this point we run into a bit of a problem. If one's degree of belief in the arithmetical conservativeness of the relevant scheme is high – which for most people of course, it is – then updating on this will make little difference to the posterior on their existence. Few people then will have their credences in Mahlos altered by this line of reasoning. Certainly, a skeptic like Feferman already willing to concede consistency and conserativeness, but doubtful only of existence, will be little moved.

Now to *some* extent this might be chalked up simply to the problem of 'old evidence', familiar from more general discussions of Bayseianism. This is the problem that for Bayesian update to have *any* effect, there must be a change in view on the E; after you have attained your evidence, the formula will no longer work to tell you how to update. Consider for example E = 'the die comes up 6 6 times', H = 'the die is loaded'. Suppose the die has been rolled six times, and came up each time 6; and you are basically certain of this. Then applying Bayes rule will yield the wrong results; really you want $P(E) = 1/6^6$, not close to 1, to get an appropriate update. In this case, as with such mundane cases more generally, it is obvious what to do: just imagine yourself in a situation before you were certain in E, before you got the relevant evidence, and reason from there to work out how you should update now. Perhaps then we ought to do the same in the case of these consistency statments?

It is a little unclear how this would go: after all, it is not as though we can all remember a time before we thought Mahlo cardinals were arithmetically conservative, along with an 'ah ha!' moment where the evidence was revealed. This points to a general issue in using Bayesian techniques for *a priori* questions; it is not clear in these circumstances that we have a relevant notion of 'evidence'. If Hellman's proposal is to work, something more subtle will have to be invoked.

One natural and interesting idea here, suggested (in my opinion all too) briefly by Hellman, is instead of thinking in terms of *an indvidual's* priors and posteriors we think in more general terms about something more nebulous, like 'the community of mathematicians', considered as a group over time. Here we can think of this group as accruing evidence for the consistency & conservativeness of large cardinals gradually and over time through experience of giving proofs in the relevant systems. On this way of thinking of things, if there were a case where the community as a whole were first rather skeptical of the consistency/conservativeness of some large cardinal statement, and then over time gradully came to be confident in the consistency, this could be taken to warrant Bayesian update among members of the community giving a boost in favor of the *existence* of large cardinals, by a pattern of reasoning not dissimilar to that sketched in the case of the die.

The extent to which this sort of thinking *should in fact* lead to a boost in confidence in large cardinals – even if only under rational reconstruction – is an interesting question. By dint of the way it is set up, it will be sensitive to the question of who gets to count as part of the 'community'. (If we start with Aristotle, we may get one set of results; if we start with Cantor, things may look quite different.) It will also be sensitive to the large cardinal axiom. Plausibly, the arithmetical conservativeness of Mahlos has been a shock to few (since Mahlo). But there are certainly interesting cases here: for example, it is currently considered (so far as I know) a *very* open question whether the choiceless cardinals are consistent/conservative;[6]

[6]This question is bound up with deep issues at the frontiers of contemporary set theory, including the so-called 'HOD dichotomy'. See CITATIONS.

so if over time, inductive familiarity led to a belief in consistency, this Bayesian machinery would predict we should be more confident in the *truth* of the choicless cardinal axioms as well.

This is an interesting potential result and the general framework of Bayesianism is in my opinion interesting ground in the epistemology of set theory, ground that has been underexplored. Hellman's paper is a welcome remedy to this, but still (naturally) leaves many questions unanswered and much work to do. Despite what seem to me to be unresolved issues in how to really carry through the application of Bayesianism in this area, Hellman deserves huge credit for raising the idea and charting out the basic terrain.

Summary

Mathematics and its Logics is an excellent collection of papers: it serves as very useful reference point for Hellman's *oeuvre* and beyond that makes an enjoyable read. The debates of course will go on, but to borrow a turn of phrase from Hellman himself, in his review of Burgess and Rosen's book on nominalism, the volume at issue is evidence of professional academic philosophy practiced at its best.