What is it to be a solution to Cantor's Continuum Problem?

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This paper is about a problem which arose in mathematics but is now widely considered by mathematicians to be a matter "merely" for philosophy. I want to show what philosophy can contribute to solving the problem by returning it to mathematics, and I will do that by elucidating what it is to be a solution to a mathematical problem at all.

The problem is Cantor's *Continuum Hypothesis* (CH), the assertion that every infinite set of real numbers has the same size as the natural numbers or the entire real line. Classical results of Gödel and Cohen show that the standard axiomatic framework for mathematics, Zermelo-Fraenkel set theory with the Axiom of Choice (ZFC), cannot solve CH. Compounding the problem, Levy and Solovay showed CH is unsolvable using widely accepted extensions of ZFC. Consensus has arisen that the idea of solving CH is such a speculative enterprise that it is a philosophical problem. This contrasts sharply with other famous open problems in mathematics like the Riemann Hypothesis (RH), the Twin Prime Conjecture, or Goldbach's Conjecture.[1] These three problems of analytic number theory collectively constituted the eighth of Hilbert's 23 problems to guide mathematical research in the 20th century, and RH is a Millennium Prize problem.[2] CH was the *first* of Hilbert's problems but—despite being unsolved—is not a Millennium Prize problem.[3]

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[1]The *Reimann Hypothesis* is the assertion that the nontrivial zeroes of the Riemann zeta function are the complex numbers with real part $\frac{1}{2}$. The *Twin Prime Conjecture* is the assertion that for any n there exist prime numbers $p_1, p_2 > n$ such that $p_2 = p_1 + 2$. *Goldbach's Conjecture* is the assertion that any even natural number greater than 2 can be written as the sum of two primes.

[2]The Millennium Prize problems are seven problems to guide mathematical research in the 21st century as chosen by the Clay Mathematics Institute. They come with a $1M reward.

[3]Feferman [10] cited CH's exclusion from the Millennium Prize list as evidence that CH is indefinite and that there's a felt sense in the mathematical community that adjudicating presumed

This is in part because CH is no longer seen as falling within the bounds of ordinary mathematical inquiry. I'm going to argue that CH is an ordinary mathematical problem like RH, the Twin Prime Conjecture, or Goldbach's Conjecture. I'll do this in the course of countering two objections to an alleged solution to CH. This alleged solution, I'll argue, embodies the general form any solution to CH must take. From this we'll see what a solution to CH—if it admits one—must be like, and we'll see it's just like what a solution to RH must be like.

1 Preliminaries

1.1 Aspects of mathematical problems and their solutions

Solutions to mathematical problems take diverse forms. We compute sums, integrate functions, prove propositions using natural language arguments or diagrams, program computers. The mathematical community has settled on a criterion for being a solution: A solution is in principle formalizable in ZFC or some widely accepted extension thereof. Allow me to record a few observations. First, the *open-endedness* of the resources which are admissible to use in a proof is exemplified by Wiles' celebrated proof of Fermat's Last Theorem,[4] which was proved using a framework developed by Grothendieck assuming (the equivalent of) an extension of ZFC.[5] Second, being formalizable does not entail that there is no controversy or iota of doubt about a theorem, although it's intended to put us in such an epistemic position. For example, Hales' 1998 computer-assisted proof of Kepler's conjecture was largely formalized when submitted to the *Annals of Mathematics*, but because of its complexity the team of reviewers could only sign off on it up to a high degree of certainty. Third, "in principle" does a lot of work in our criterion, as almost all mathematical research has not been strictly formalized. One reason for this is that mathematicians have

solutions to CH will require deviation from the usual norms of mathematics.

[4] *Fermat's Last Theorem* is the assertion that no natural numbers $a, b, c > 0$ are such that $a^n + b^n = c^n$ for any $n > 2$.

[5] When asked whether he thought Fermat's Last Theorem is provable in a weaker theory, Wiles has reportedly retorted, "Who cares?" This was related to the author by Menachem Magidor.

Experts believe that the elements of Grothendieck's theory which Wile's proof needs localize in a way which does not require the strong extension. (Grothendieck apparently did not find his assumption problematic.)

A more intriguing, less famous example is Richard Laver's use of one of the most powerful extensions of ZFC to prove results about the periods of finite *Laver tables*. It's open whether ZFC suffices to establish these properties of the tables, but it is not doubted that the tables have the properties [8].

gone through what Tao calls the "post-rigorous" stage of their mathematical education, where their training in the formal foundations of their research area has made spelling out proof details, computations, etc. simply routine and understanding the key ideas of claimed proofs sufficient to judge, with a high degree of accuracy, their validity [44].[6] Nonetheless, it is important to the mathematical community that there be an idea of how an argument is formalizable.

It is important because a unifying property of solutions is their *correctness*. An 'incorrect solution' is an oxymoron, and it is this grammar that makes solutions seem necessary. They could not be any other way than the way they are.[7] Supposing otherwise is nonsense. (Think of an assumption in a proof by contradiction.) Another characteristic of solutions that is in a sense naive is that they are *discoverable*. Posed with a problem, there is the sense that there is an answer for us to find. We do not make the answer up according to our preferences. RH is either true or false, and whichever it is will not depend on what number theorists find most elegant.

Theorems and exercises in textbooks and research articles are problems whose solutions are known. Problems for which (as far as we know) no one in human history up to the present has known the solution are *open* problems. Within the class of open problems, there are problems which, if true, "provide the skeletal architecture of a theory" [27, p. 198]. These are *conjectures*. Conjectures can serve as hypotheses in conditional theorems, as if they are promissory notes, in serious research. CH and RH are paradigmatic examples of conjectures.[8]

Finally, there is the question of what resources are required to obtain a solution. For example, one cannot trisect arbitrary angles with a ruler and compass, but one *can* solve the trisection problem if one helps oneself to additional tools, like a collapsible compass. While some problems are specified with reference to a specific set of admissible tools, they need not be. It didn't matter what tools Andrew Wiles availed himself of in solving Fermat's Last Theorem as long as they were widely accepted. The question of what tools are necessary for solving problems leads to

[6] Another is that in general, formal proofs do not help us understand mathematics or develop new proof ideas. Harris emphasizes this in his essay "The Central Dogma of Mathematical Formalism" [18], which denies the 'Central Dogma' that a theorem being proved informally is *equivalent* to its being proved in a formal system.

[7] Within the theory used to get the solution. A theorem is an implication from a theory to a statement in the language of that theory. For example, "Assume Peano Arithmetic. Then there are infinitely many prime numbers." is a theorem. When no 'special' resource is used, the theory being assumed is often suppressed. Hence we say, with no qualification, that Fermat's Last Theorem is true, or has been solved.

[8] 'Hypotheses' were conjectures a sufficient number of special cases of which were confirmed by the time they were put forward.

optimizing hypotheses and theorems of the form “If we could solve problem X, then we could solve problem Y.”

I said at the outset that CH is unsolvable using ZFC or even more powerful, widely accepted resources. A *universe* or *model* of set theory is a mathematical structure satisfying the ZFC axioms. (*The* universe of sets is denoted V.) To show that ZFC cannot solve CH, it suffices to show that there is a universe in which CH is satisfied and a universe in which it is not. Gödel achieved the first task by defining a ‘narrow’—in the sense that it does not contain all sets[9]—universe called L. L is a *canonical* universe, which means that its theory is invariant no matter which ambient universe it is defined in. As a consequence, L has such rich structure theory that we can reasonably expect every mathematical question to be solvable using it. (L shares this feature with the natural number structure, the canonical structure par excellence.) As it happens, CH is true in L. To achieve the second task, Cohen devised a general method for transforming a given universe into one in which CH takes the opposite truth value it began with. The method is called *generic extension.* L can be generically extended to a universe in which CH is false. Thus CH cannot be solved in ZFC.[10]

It turns out that CH can fail in infinitely many ways, and these ways are realized by generic extension.[11] Generic extension can be iterated to achieve all of these

[9]The widely accepted resources extending ZFC which I've gestured at imply this. The statement that this narrow universe is the universe of sets—the statement $V = L$—historically had some proponents. Devlin wrote in 1977: “It is a natural axiom, closely bound up with what we mean by “set”. [...] its assumption leads to the solution of many problems known to be unsolvable from the Axiom of Choice alone. Time alone will tell whether or not this axiom is eventually accepted as a basic assumption in mathematics” [9, p. IV]. Time told: the majority of the 24 articles in the *Handbook of Set Theory* [12]—articles by experts and intended to get one to the research frontier—are about structure and methods incompatible with $V = L$. The evidence is that we are on the “$0^{\#}$ exists” side of the Covering Lemma (see below). Jensen wrote in 1995: “[$V = L$] makes a clear statement about the nature of the mathematical universe. It is mathematically fruitful in that it solves many problems and leads to interesting new concepts and theories. It is philosophically attractive for adherents of “Ockham's razor,” which says that one should avoid superfluous existence assumptions. I personally find it a very attractive axiom. Nevertheless, it has been rejected by the majority of set theorists, beginning with Godel himself” [21, p. 398]. But Ockham's razor does not apply: There is mathematical structure which cannot exist in L. The hypotheses that enable investigation of this structure are not superfluous.

[10]Independence proofs are another reason, beyond correctness, why formalizability is important. If we want to show that a proposition is independent of our standardly accepted mathematics, we need to know what “our standardly accepted mathematics” encompasses, and we need to put it in a form that is amenable to the tools of mathematical logic.

[11]While our attention is on CH, there are infinitely many problems which are unsolvable using ZFC and its widely accepted extensions.

starting from an initial universe, generating the *generic multiverse* of universes which generically extend the initial one.[12]

Already suspecting prior to Cohen's work that CH is unsolvable, Gödel in his paper "What is Cantor's Continuum Problem?" [15] initiated a research program of identifying new, well-justified axioms that solve CH. Compounding the Continuum Problem further, many alleged solutions to CH inspired by Gödel's program have been put forward, and they are almost all mutually inconsistent.[13] It seems to me that this program got us off on the wrong foot—and invited the "relegation" of CH to philosophy—by emphasizing justifications of axioms (1) without directing attention to what conditions have to be in place for something to be a solution to a mathematical problem to begin with and (2) at the expense of their content. The present paper will hopefully close this gap and turn focus towards the *content* of axioms, not merely their purported justifications.

1.2 Ordinary mathematical inquiry

In ordinary mathematical inquiry, mathematicians believe mathematically precise statements have truth values, and they take themselves to be in the business of discovering those truth values. In particular, they do not arbitrarily decide them. Usually, their discoveries are uncontroversial. Everyone agrees that what should qualify as a solution to CH is controversial. This by itself does not mean CH is not an 'ordinary' problem like RH—Hales' proof of the Kepler conjecture already shows that problems solvable using our standard tools can be controversial.

What would make CH extraordinary would be if it admitted a solution but the research program leading to that solution did not treat CH according to ordinary mathematical inquiry. Lingamneni [26] claims that, while some alleged solutions to CH are arrived at in accordance with ordinary mathematical inquiry, the only alleged solution I believe might make a successful claim to being *the* solution is not. Similarly, Feferman claims

> the usual idea of mathematical truth in its ordinary sense is no longer operative in the research programs [leading to the alleged solution] which, rather, are proceeding on the basis of what seem to be highly unusual (one might even say, metaphysical) assumptions. And even though the

[12] There are two notions of "the" generic multiverse in the literature, see [47] and [39].

[13] A non-exhaustive list of proposals: forcing axioms and related Baire category principles, the $\mathbb{P}_{\max}$ axiom $(*)$ and its variations and generalizations, the Axiom of Constructibility $V = L$, $V =$ Ultimate L, generic elementary embeddings, real-valued measurable cardinals, Freiling's Axiom of Symmetry.

> experts in set theory may find such assumptions compelling from their experience of working with them...the likelihood of their being accepted by the mathematical community at large is practically nil. [10, p. 8]

I'm going to argue that [26] and [10] misconceive salient aspects of ordinary mathematical inquiry. Once we see how the conception of [26] and [10] goes wrong, we'll see that CH is an ordinary mathematical problem like RH, the Goldbach conjecture, and the Twin Prime conjecture. With the clarified picture I will argue that the *canonical models solution* embodies the only way to proceed in solving CH. In particular, (1) I will be concerned with the form of the canonical models solution, not with its content, and (2) none of the other alleged solutions could constitute the solution (without significant supplementation).

In the next section, I will sketch the canonical models solution to CH and Lingamneni's criticism that it is incapable of delivering an ordinary, non-arbitrary solution to CH. In §3 I will begin the clarification. The general form of the solution to CH—*if there is one, which this paper leaves open*—is given in §4.

2 The canonical models solution

The canonical models solution to CH is to define *Ultimate* L, a universe of sets that admits a structural analysis so detailed that every question about the universe is determinate and which is not 'narrow' like L. Then one uses the structural analysis to solve CH in Ultimate L. CH is true in Ultimate L.

Then one must argue that CH being true in Ultimate L shows that CH is true simpliciter. The immediate obstacle to this is that the truth value of CH varies across the universes in the generic multiverse. But what if the generic multiverse contains a universe from which all of the other universes are accessible by generic extension? That universe would be the uniquely definable member of the generic multiverse, a privileged universe, and the theories of all generic extensions would reduce to its theory. Wouldn't the unique definable universe in the multiverse just be V? (Or isn't that what we mean when we use 'V'?) Usuba [45] showed that there is such a universe. And Woodin showed that Ultimate L, if it exists, is the unique universe. Thus Ultimate L satisfying CH is what it means for CH to be true, or to hold in V. Even though CH is false in other universes in the generic multiverse, that fact is irrelevant—they are not the privileged universe.

So the argument—which is not unanimously endorsed by set theorists—goes.

2.1 Objection 1: Reframing

On Lingamneni's reading, the foregoing argument "immediately belies its own claim to have resolved CH" [26] because it admits that there are universes of set theory in which CH is false. When RH is solved, on the standard view of ordinary mathematical inquiry, there will be no models of arithmetic in which RH has the opposite truth value. Reframing CH as a problem in the special universe in the generic multiverse is just that—a reframing. Why accept that it supersedes the original question? To accept it is to tacitly admit that there is no fact of the matter about CH.

Moreover, in other fields of mathematics, there are similar reframings, but answers to reframed questions are not taken to be solutions to the original problems![14] The reframing maneuver being legitimate would lend further credence to the claim that CH lies outside the bounds of ordinary mathematical inquiry.

2.2 Objection 2: Arbitrary Decision

Lingamneni's second objection is that there is insufficient reason to believe the universe of sets is *fine structural*,[15] i.e. admits a detailed, quantifier-by-quantifier analysis, even if fine structure is the only known mechanism with which to answer open questions:

> The problem is that on this methodology, just as long as we get an answer, any answer will do... There is a real and salient epistemic possibility that despite our hopes, the Riemann Hypothesis could actually be false — so much the worse for us! If there is no such fear to restrain us with regard to CH, it must be because we do not really believe that there [is a fact about it—we are free to **adjudicate** or even dismiss the question, **rather than being forced to discover its solution**...]
>
> Put another way, what distinguishes the answer-maximizing justification for "V = Ultimate-L" from other extrinsic justifications is the lack of accountability to an external criterion of set-theoretic truth. [26, p. 615, my emphasis]

The canonical models approach provides answers to problems, but no guarantee that its answers are *solutions.*

[14] They cannot be solutions in general. A salient example is the theorem [2] that relativizing the Millennium Prize problem whether polynomial time is nondeterministic polynomial time to oracles cannot solve it: there are oracles A, B such that $P^A = NP^A$ and $P^B \neq NP^B$.

[15] Fine structure theory originates in work of Boolos and Putnam [4] analyzing when new reals appear in L. Jensen [20] developed and applied the fine structure theory of L.

3 Truth in mathematics

I will now rationally reconstruct the notion of mathematical truth operative in the canonical models research program and its solution to CH. The notion is, I believe, just the ordinary one, the one operative in analytic number theory or algebraic topology. My reconstruction will draw on elements familiar from philosophy and logic.

The first element is Tait's observation that *proof is our criterion for truth in mathematics*; holding in an intended model is not [43]. In the sequel I will treat the arguments of [43] as sound and the resolution to the Truth/Proof problem—"what has what we have learned or agreed to count as a proof got to do with what obtains in the system of numbers?" [43, p. 341]—as given:

> This problem arises because there seem to be two, possibly conflicting, criteria for the truth of a mathematical proposition: that it hold in the relevant structure and that we have a proof of it. The first step of the resolution is to see that the first criterion is not a criterion at all. The appearance that it is arises from the myth of the Model-in-the-Sky, of which we must—but do not seem to—have some sort of nonpropositional grasp, with reference to which our mathematical propositions derive their meaning and to which we appeal to determine their truth. The fact is that there are no such Models; there are only models, i.e., structures that we construct in mathematics. Our grasp of such a model presupposes that we understand the relevant mathematical propositions and can determine the truth of at least some of them - e.g., those whose truth is presupposed in the very definition of the model. Thus, rather than saying that holding in the model is a criterion for truth, **we would better put it the other way around: being true is a criterion for holding in the model**. [43, p. 355, emphasis mine]

Tait's observation concentrates attention on the assertability conditions operative in mathematics: One is warranted to claim that a proposition is true just in case one can prove it. To return CH to mathematics—to treat it as an ordinary mathematical problem, as I see the canonical models researchers as doing—we cannot deviate from these assertability conditions when it comes to CH. In particular, justifying an answer to CH by appealing to anything external to mathematical activity (like a Model-in-the-Sky) against which that activity is to be compared so as to determine its legitimacy violates the assertability conditions.

The second element is Carnap's internal/external question distinction, about which he writes:

> If someone wishes to speak in his language about a new kind of entities, he has to introduce a system of new ways of speaking, subject to new rules; we shall call this procedure the construction of a linguistic framework for the new entities in question. And now we must distinguish two kinds of questions of existence: first, questions of the existence of certain entities of the new kind within the framework; we call them internal questions; and second, questions concerning the existence or reality of the system of entities as a whole, called external questions. Internal questions and possible answers to them are formulated with the help of the new forms of expressions. The answers may be found either by purely logical methods or by empirical methods, depending upon whether the framework is a logical or a factual one. [5]

External questions are "philosophical questions concerning the existence or reality of the total system of the new entities. Many philosophers regard a question of this kind as an ontological question which must be raised and answered before the introduction of the new language forms" [5]. But external questions do not admit of answers; they are really pragmatic questions about whether to accept new language forms. "The acceptance cannot be judged as being either true or false because it is not an assertion. It can only be judged as being more or less expedient, fruitful, conducive to the aim for which the language is intended" [5].

I want to use the internal/external distinction because it is intended to apply in the context of formal systems, and set theorists tacitly use the distinction when answering questions about problems independent of ZFC. When asked "Do Suslin trees exist?",[16] a set theorist will respond by internalizing the question to different theories. Internal to the theory $\mathsf{ZFC} + V = L$, there are Suslin trees; internal to the theory ZFC+ Martin's Axiom, there are none. There does not seem to be a sense in which this question could be intended prior to internalizing to *some* theory. A lot of theoretical apparatus has to be in place to even formulate the question.[17] On the other hand, it is trivial to prove in set theory that there are sets. Asking "But do

[16] A Suslin tree is a tree of height ω_1 whose branches and antichains are all countable.

[17] Mathematics has been embedded in our ways of life for millennia. I view the mathematical activity of the Babylonian surveyors, the human computers of ancient China, the Islamist mathematicians in the middle ages, the Italian merchants of the Renaissance, the 19th century analysts, and so on as contributing to pinning down and developing theories of arithmetic, geometry, analysis, etc. which are now formalizable in ZFC. Their mathematics, often computational rather than proof-based, was carried out in informal theories which their problems were internalized to. The theories may have evolved, but the correctness of their solutions seems not to have changed. This stability of solutions gives mathematics its peculiar cumulative character, and distinguishes the informal theories from theories in other fields like e.g. astronomy, geoscience, or literary criticism.

sets *really* exist?" is to insist on the meaningfulness of an external question.

Finally, what theory should we internalize mathematical questions to by default? We have come to internalize number theoretic questions to Peano Arithmetic, whose intended interpretation[18] is the natural number system. One reason we do this is that intended interpretations are characteristically fully determinate. For example, the (non-recursively enumerable) theory *True Arithmetic* is the set of sentences which are true in the intended interpretation of Peano Arithmetic, and problems which are formally independent of Peano Arithmetic are nonetheless believed to have truth values when evaluated in the intended interpretation—they or their negations are included in True Arithmetic. Evidence that we understand the intended interpretation of Peano Arithmetic includes the fact that there are no divergent arithmetics and our recognition of the correct extensions of Peano Arithmetic (like Second Order Arithmetic) that solve independent-from-Peano-Arithmetic problems (like the Paris-Harrington principle[19]).

What if we were to do the analogous thing, internalizing set theoretic questions to the theory of the intended interpretation of set theory? (We'd like to treat CH as ordinarily as any number theoretic problem, after all.) A key point—crucial to Tait's observation above and to refuting Feferman's claim that the canonical models solution rests on metaphysical assumptions—is that *interpretations are specified within mathematics*.[20] For example, Gödel's universe L is an interpretation of the language of set theory,[21] and it is rigorously specifiable within an ambient set theory.

The idea I would like to pursue is that we are looking for a theory to internalize CH to, and the mark of the right theory is its ability to specify the intended interpretation of the language of set theory.

[18] The *intended interpretation* of a formal theory is the interpretation—the assignment of meaning to the symbols of the theory's signature—that motivates the study of the theory.

[19] For any integers $n, k, m > 0$ with $m \geq n$, there is $N > 0$ such that for any coloring of the n-element subsets of $\{1, 2, 3, \ldots, N\}$ with k-many colors, there is $Y \subseteq \{1, 2, 3, \ldots, N\}$ of size at least m such that the coloring is homogeneous on Y and the number of members of Y is at least the minimum element of Y. Paris and Harrington proved that this principle is independent of Peano Arithmetic.

[20] The emphasis on *theory* keeps clear that we are not concerned with a Model-in-the-Sky, or anything which we would require nonpropositional grasp of, only with models that can be built in the theory. The theory the canonical models researchers are building both allows one to define the intended interpretation and is satisfied in that intended interpretation. Since theories are ultimately what mathematics is done in, that there may be e.g. countable models satisfying the theory of the intended interpretation is immaterial under this methodology. It brackets philosophical questions of determinate reference of mathematical language.

[21] Indeed, L is the intended interpretation of ZFC + $V = L$.

3.1 A feedback loop

The only way to proceed in determining the intended interpretation is by working from within, with set theory being our "Neurath's boat." In this process, a certain kind of *conjecture* occupies a central role. On this sense of conjecture, Mazur writes:

> These conjectures are expected to turn out to be true, as, of course, are all conjectures; their formulation is often a way of "formally" packaging, or at least acknowledging, an otherwise shapeless body of mathematical experience that points to their truth. From these conjectures, implications may be perfectly rigorously made. Best, if the conjectures are, loosely speaking, "testable", or "falsifiable" in the sense that they imply a stream of particular, numerical perhaps, predictions many of which may be directly checked. But these conjectures are architectural in that they play the role of "joists" and "supporting beams" for some larger mathematical structure yet to be made. These conjectures sometimes round out a field by being clear, general (but not yet proved) statements enabling one to understand where a certain amount of on-going, perhaps fragmentary, specialized work is headed; they provide a focus. Their formulation sometimes serve to "allow the field to proceed": a research program may continue, conditional on the truth of these statements, in order to see what lies further down the road. One effect of the formalization of Conjecture is to give concrete language—"a local habitation and a name"—to expectations, analogies, hoped-for constructions, etc., long before the methods needed for their elucidation are available, giving us a rich source of palpable "historical artifacts" about ideas at an early stage in their development. [27, p. 199]

The canonical models program is a theory building endeavor progress in which hinges on programmatic, often dichotomous conjectures.[22] They are what the canonical model researchers "test against." Architectural conjectures provide the link between the mathematics and the philosophy of the canonical models program, which interact in the following feedback loop:

1. theorems that tell us about the universe of sets induce

[22] For example, much of inner model theory is conditioned on some form of the *iterability conjecture* (see e.g. [38, Conjecture 6.5]). The conjecture that the hereditarily ordinal definable sets of models of the Axiom of Determinacy satisfy the Generalized Continuum Hypothesis [42, Conjecture 8.2] is an example of a programmatic conjecture which seems out of reach but guides contemporary research. The *Mouse Set Conjecture* (see §5.2.4) is a "supporting beam." The canonical models solution is contingent upon (some version of) Woodin's Ultimate L Conjecture.

2. a philosophical[23] idea of the nature of the universe of sets, which is distilled to
3. mathematically precise conjectures and test questions which are proved or refuted in the form of more
4. theorems that tell us about the universe of sets, which induce stronger...

That the philosophy is so closely intertwined with the mathematics is evidenced by the technical nature of the axioms arrived at via this process.

The qualification that the theorems be about the universe of sets is due to the fact that many theorems in set theory establish the relative consistency of various mathematical states of affairs; they rarely establish that those states of affairs must unequivocally hold in the universe of sets. The feedback loop generates stronger and stronger theorems about V, leading to more and more theory.

3.1.1 Feedback in the canonical models research program

In this section I will illustrate the feedback loop at different levels of resolution by giving a quasi-chronological account of the development of the canonical models solution to CH. In doing so, I will meet the following criticism due to Lingamneni:

> In the case for large cardinals, one is (meant to be) persuaded by the goal of maximizing interpretive power, which leads to belief in the large cardinal axioms and the discovery of truths such as [Projective Determinacy]. In another perspective on the case [Peter Koellner's], one comes to believe that PD is true (e.g., because PD's regularization of descriptive set theory makes it the correct venue for analysts), and then one comes to accept large cardinals because they are natural hypotheses from which determinacy can be derived. Nor is it necessary to choose one "direction" for the argument to the exclusion of the other; the "web of implications"... between the two classes of hypotheses leads naturally to a picture where the beliefs in them are mutually supporting. The point is that the web is not "free-floating", but is anchored somewhere to the ground: it rests on some external reason or reasons to believe that in adopting large cardinals and determinacy, rather than V = L and definable failures of determinacy, we have arrived at the right answer...
> But in the case for "V = Ultimate-L", neither the proposed axiom nor its conjectured consequences (including $2^{\aleph_0} = \aleph_1$) have a suitable external

[23] For some value of "philosophical." 'Informal' would work here in a lot of cases, but sometimes the informal ideas grow into philosophical views like "V is like the HOD of a determinacy model."

> ground. The justification for CH is then essentially circular: $2^{\aleph_0}$ equals $\aleph_1$ because we want to fix a value for it, and any value will do. This is not a realist attitude to the continuum problem. [26, p. 615]

The feedback loop will provide the mutually supporting web of implications Lingamneni thinks is absent in the canonical models solution.

We begin with two theorems about V as input to the feedback loop.[24] Gödel's universe L is the minimal universe of set theory.[25] A universe M of set theory is Γ-*correct* if whenever a sentence φ is of complexity Γ, then $M \models \varphi$ implies that φ is true in V. Any universe of set theory is Σ^1_1-correct.

Theorem 3.1 (Shoenfield)**.** *L is Σ^1_2-correct.*

Shoenfield's theorem tells us about how L relates to V and allows us to leverage that relationship in arguments: For logically simple enough problems φ, it suffices to solve φ in L, where one can appeal to its fine structure. If one can show that $L \models \varphi$, then Shoenfield's theorem implies that φ is simply true.

The second theorem is again about how L and V relate. We say that a model M of set theory *computes successors of singular cardinals correctly* if $(\kappa^+)^M = \kappa^+$ when κ is a singular cardinal. $0^\#$ is a real number that cannot exist in L (it is the least witness to the fact that L is a narrow universe of sets).[26]

Theorem 3.2 (Jensen's Covering Lemma)**.** *If $0^\#$ does not exist, then L computes successors of singular cardinals correctly.*[27]

The covering lemma also gives rise to a form of argument used to gauge the logical strength of certain mathematical statements. One shows that if the statement holds, then L does not compute successors of singular cardinals correctly, and hence $0^\#$ exists and the statement has fairly substantial large cardinal strength. The point of describing the patterns of argument which Jensen's and Shoenfield's theorems give rise to is to emphasize that they are useful, and their utility suggests that more general versions of them might hold and would be desirable to have.

[24] This thread of the rational reconstruction (through Woodin's generalizations below) follows Steel's 2001 talk "Inner model theory."

[25] If M is a universe of set theory, then $L \subseteq M$.

[26] $0^\#$ codes the theory of a unique club class of indiscernibles for L, and it exists if and only if there is a nontrivial elementary embedding from L to itself. (Such an embedding cannot exist inside of L.) The definition generalizes to other inner models, and in fact sharps can, with hindsight, be viewed as mice, see [33].

[27] This is really *weak covering* [28]. The standard covering lemma says that either $0^\#$ exists, or for any set of ordinals x, there is a set $y \in L$ such that $x \subseteq y$ and $|y| = |x| + \aleph_1$. Weak covering can hold of larger inner models than L.

In modern terminology, Jensen's theorem says that if $0^\#$ does not exist, then L is the *core model*. Generally, the *core model*, denoted K, is the maximal canonical inner model under an anti-large cardinal hypothesis. As Mitchell writes,

> "the core model" is always singular: there is at most one core model in any given model of set theory, and in particular there is at most one true core model in the true universe of sets [29, p. 1490].

Significantly for my purposes, K is *unambiguous*: it is rigid,[28] absolute,[29] and definable.[30]

Shoenfield's theorem inspires the philosophical idea that truths in V reflect into canonical universes, which in turn gives rise to the conjecture that larger canonical models extend Shoenfield's theorem to more complicated quantificational structure. Woodin confirmed the conjecture for projective truths.

Theorem 3.3 (Woodin)**.** *If $M_n(x)$ exists, then $M_n(x)$ is Σ^1_{n+1}-correct.*[31]

Jensen's Covering Lemma inspires the philosophical idea that under more generous smallness assumptions, the universe is approximated by a canonical structure, which in turn gives rise to the conjecture that V is saturated with sharps for models with Woodin cardinals. Otherwise V would be K under some smallness condition which, as a consequence of commitment to large cardinals, the universe of sets does not satisfy. Woodin confirmed the conjecture.

Theorem 3.4 (Woodin)**.** *Assume $M_n^\#(x)$ exists for all $x \in \mathbb{R}$. If there is an $x \in \mathbb{R}$ such that $M_{n+1}^\#(x)$ does* not *exist, then there is a fine structural inner model K that computes successors of singular cardinals correctly.*

What about canonical models containing more than Woodin cardinals? The loops up to the M_n's inspire more general philosophical ideas:

1. Larger canonical models are correct for larger rank initial segments of the universe of sets.

2. The core model under *no* anti-large cardinal hypothesis is the universe of sets.

[28] There is no nontrivial elementary embedding from K to itself.

[29] "[T]he core model is absolute for a class of sentences which falls just short of including the sentence asserting that there is a set not in that core model" [29, p. 1491].

[30] "The core models are uniquely defined by a formula which is absolute under set generic extensions" [29, p. 1491]

[31] $M_n(x)$ denotes the minimal inner model over the real x containing n Woodin cardinals.

Here the loop almost halted. The M_n models are *pure extender models*,[32] and pure extender models containing Woodin cardinals are non-trivial generic extensions of inner models. Moreover, they are built using *iteration strategies*, definable objects which they provably cannot contain. V should satisfy the Ground Axiom[33] (otherwise why isn't the inner model V?), and it's nonsense to say that the universe of sets is constructed using a set which the universe of sets cannot contain. V cannot be a pure extender model.

In the early 1990s, Steel proved that the class of hereditarily ordinal definable sets (HOD) of $L(\mathbb{R})$, assuming the Axiom of Determinacy (AD), is a pure extender model up to rank V_θ. Woodin showed that the full $\mathsf{HOD}^{L(\mathbb{R})}$ is a *hod mouse*, a pure extender model together with enough fragments of its iteration strategy that the model can see how it iterates.[34] Hod mice satisfy the Ground Axiom and thus address the deficiencies in pure extender models, and their theory has been developed sufficiently so that HOD of much stronger models of determinacy than $L(\mathbb{R})$ have been analyzed.

But then there was an argument, based on a theorem of Woodin,[35] that hod mice must be small, that they can only witness middling large cardinal axioms.[36] So they would be of limited utility in concretely realizing the first philosophical idea. And there was still no suggestion that there could be a core model under *no* smallness assumption. Every canonical model, whether a pure extender model or a hod mouse, had an associated smallness theorem to the effect that some large cardinal could not exist inside it.

In the mid-1990s, Woodin showed that if the nonstationary ideal is ω_2-saturated[37] and there is a measurable cardinal, then the second uniform indiscernible[38] is ω_2. Hence under these hypotheses there is a definable witness to the failure of CH. This

[32]A pure extender model, or pure extender mouse, is a model of set theory of the form $L[\vec{E}]$, where $\vec{E}$ is a coherent sequence of extenders, which is *iterable*. We refer the reader to [41, §1.3,1.5] to unpack the definition.

[33]The statement that V is *not* a generic extension of an inner model.

[34]The Steel-Woodin analysis is the subject of [42]. A hod mouse or *strategy mouse* is a model of set theory of the form $L[\vec{E}, \Sigma]$, where again $\vec{E}$ is a coherent sequence of extenders and Σ is an iteration strategy for $L[\vec{E}, \Sigma]$.

[35]Namely that no cardinal can be strong past a successor Solovay point $\theta_{\alpha+1}$ in HOD of a determinacy model.

[36]It was thought they could not have extenders overlapping Woodin cardinals on their extender sequences.

[37]The nonstationary ideal on ω_1 is the set of all nonstationary (intuitively, 'negligible') subsets of ω_1. Its being ω_2-saturated means that there is no family of ω_2-many stationary subsets of ω_1 all of whose pairwise intersections are nonstationary.

[38]Suppose $x^\#$ exists, for all reals x. ($x^\#$ codes the theory of indiscernibles for $L[x]$.) Then a *uniform indiscernible* is an indiscernible for $L[x]$, for all reals x.

was achieved by forcing over the canonical model $L(\mathbb{R})$, assuming $\mathsf{AD}^{L(\mathbb{R})}$, in such a way that the resulting generic extension remains canonical. The metamathematical properties of the model established in [48] made it arguably the most compelling alleged solution to CH to date. But a connection between the generic extension of $L(\mathbb{R})$, which is a narrow inner model, and V was lacking, and the theory of the generic extension had counterintuitive consequences: The theory of third order arithmetic in the model is Turing reducible to the theory of second order arithmetic. The definable counterexample to CH is obtained, roughly, by greatly simplifying third order arithmetic in the model. The idea that third order arithmetic is no more complex than second order arithmetic seems to conflict with intuitions about how the power set generates new complexity. It is notable that despite its counterintuitiveness, this reducibility did not lead to the abandonment of the canonical $\neg$CH model.

Then in the 2000s, Woodin proved a theorem [49, Theorem 3.26] which opens the door to the possibility of a core model with no anti-large cardinal hypothesis. It says that if there is a canonical model that is "large enough," then that model is "as large as" V. Given the purported limitations on hod mice, however, the theorem could've been vacuous.

A few years later, Sargsyan identified a gap in the argument that hod mice must be small. The canonical $\neg$CH model was abandoned, hod mice pursued, and a general correctness principle—the axiom $V =$ UItimate L—formulated. The foreword to the second edition of [48] is explicit:

> recent results concerning the inner model program undermine the philosophical framework for this entire work. The fundamental result of this book is the identification of a canonical axiom for $\neg$CH which is characterized in terms of a logical completion of the theory of $H(\omega_2)$... *But the validation of this axiom requires a synthesis with axioms for V itself for otherwise it simply stands as an isolated axiom*... I remain convinced that if CH is false then the axiom $(*)$ holds... But nevertheless for all the reasons discussed at length in [47] I think the evidence now favors CH...
>
> The picture that is emerging now... is as follows. *The solution to the inner model problem for one supercompact cardinal yields the ultimate enlargement of L. This enlargement of L is compatible with all stronger large cardinal axioms and strong forms of covering hold relative to this inner model.* At present there seem to be two possibilities for this enlargement, as an extender model or as a [hod mouse]. There is a key distinction however between these two versions. An extender model in which there is a Woodin cardinal is a (nontrivial) generic extension of an inner model

which is also an extender model whereas a [hod mouse] in which there is a proper class of Woodin cardinals is not a generic extension of any inner model. The most optimistic generalizations of the structure theory of $L(\mathbb{R})$ in the context of AD to a structure theory of $L(V_{\lambda+1})$ in the context of an elementary embedding,

$$j : L(V_{\lambda+1}) \to L(V_{\lambda+1})$$

with critical point below λ require that V not be a generic extension of any inner model which is not countably closed within V. Therefore these generalizations cannot hold in the extender models and this leaves the [hod mice] as essentially the only option. *Thus there could be a compelling argument that V is a [hod mouse] based on natural structural principles.* [48, p. 19, emphasis mine]

My emphases bear out the rational reconstruction regarding the pivot from axiom $(*)$ and the primacy of hod mice. Without knowing that axiom $(*)$ could be forced over V ("synthesized with axioms for V"), the case for the alleged negative solution to CH was incomplete. But the theorem that the inner model for a supercompact cardinal, if it exists, is the core model under no smallness assumption rendered this issue moot. Woodin raises the ideas that V is a hod mouse and that this may follow from a natural conception of the universe of sets which is grounded in the mathematics.

The change of mind recorded in this passage follows mathematical developments. No pre-theoretic or external notions are appealed to; everything is internal to the theory that's being developed, and it's concerned with building an intended interpretation of set theory.

3.1.2 The first word

Why should the feedback loop be initiated on *these* as opposed to other first principles, theorems, and concepts? Transposing Austin [1], our common stock of first principles—the ZFC and large cardinals axioms—embodies resources and modes of reasoning mathematicians have found worth using and the theorems they have thought worth proving in the lifetime of many academic generations. "They are likely to be more numerous, more sound, since they have stood up to the test of the survival of the fittest"—producing and systematizing good mathematics—"and more subtle, at least in all ordinary and practical mathematical matters"—since they arose in response to mathematical need—"than you or I are likely to think up in our armchairs of an afternoon—the most favoured alternative method" [1, p. 130]. They are the first word. (We have to start with something, after all.) They are not the last word: The feedback loop generates new concepts and conjectures and theorems governing them. These new concepts are tethered; they do not come out of nowhere. No mathematics comes out of nowhere.

What if set theory had evolved differently? Pudlák has us wonder

> what would have happened if Cantor had not become interested in infinite cardinals, continued his research in analysis and discovered the determinacy principle. Imagine that the Axiom of Determinacy had been introduced first, and before the Axiom of Choice was stated the nice consequences of determinacy, such as measurability of all sets, had been proved. Imagine that then someone would come up with the Axiom of Choice and the paradoxical consequences were proved. Wouldn't the situation now be reversed in the sense that the Axiom of Determinacy would be 'the true axiom', while the Axiom of Choice would be just a bizarre alternative? [32, p. 221]

Doesn't Pudlák's thought experiment raise—without semantic ascent—a skeptical worry about taking our concepts as given? Couldn't we have different concepts and intuitions?

I will address this with a point particular to AD, and then I will argue that the point is, on our current state of knowledge, more general than it first seems. It will be clear that I am skeptical of the import of such counterfactuals. (A "What if...?" question about ways mathematics could have developed or how our mathematical beliefs could purportedly have been different should be accompanied by a "Why didn't...?" or "Why don't we...?" question.)

AD never inspired foundational conflict because it was from its inception conjectured to hold in the inner model $L(\mathbb{R})$. If AD had been discovered before Choice, I'd expect a development symmetric to the actual development of set theory to take place. We would work in a universe satisfying AD. In the course of developing determinacy theory, it would be noticed that Choice holds in HOD.[39] It would eventually be proved that ω_1^V is measurable in HOD. Further large cardinal properties of cardinals in HOD would be researched. The discrepancies between measure large cardinals under AD and embedding large cardinals under Choice would be scrutinized. If it was observed that the embedding large cardinals "outstrip" AD in terms of consistency strength,[40] and the theorems saying that models of AD are "small" were proved, the

[39] HOD being the natural apotheosis of our concept of definability.

[40] The strongest determinacy theories known today are weaker than a Woodin limit of Woodin cardinals, which is much weaker than a supercompact cardinal. It was conjectured that the theory $\mathsf{ZF} + \mathsf{AD}_{\mathbb{R}} +$ "ω_1 is supercompact" is equiconsistent with a Woodin cardinal which is a limit of

picture would likely invert: Choice holds in the ambient universe, which extends beyond the universe of AD we initially worked in. The reasons why Choice was actually accepted—why shouldn't we still want to prove Tychonoff's theorem, in Pudlák's world?—would likely come into play.[41]

Even if the picture did not invert to what we have now, *we already have a sense of what it would be like if* ZF + AD *were our foundational theory, for the Cabal set theorists produced an enormous body of celebrated* AD *research* [22, 23, 24, 25].[42] That research continues today. The canonical models program is an extension of it [41, 49]. There is thus reason to believe that in Pudlák's world, research effort would eventually be directed at defining canonical models for large cardinals.

I do not see that we would have fundamentally different set theoretic concepts in Pudlák's world. (About *intuitions*, see the next remark.) Now for the general point. There are few strong foundational theories which rest on fundamental conceptual advances in set theory: large cardinals, inner models, determinacy, and forcing axioms. They and their relations with each other have been extensively studied by set theorists. They are not disjoint; they are interwoven. While there is historical contingency in the actual development of set theory, as in everything, that contingency does not entail that the concepts themselves are contingent in the way the skeptical worry suggests they might be. The worry would be more compelling if there were more new fundamental ideas 1) embodied in strong foundational set theories but 2) not interwoven with the others. Speculating about whether there are such ideas and what that would entail for set theory is interesting, but one cannot do mathematics with such speculations. They are not the first or the last or any word.

Remark 3.5. [6] identifies two ways in which Gödel may have led us down a garden path: by adopting Russell's regressive method and advocating for intuitions as justifications for mathematical beliefs. In Russell's regressive method, possible first principles are adjusted in response to how well their implications fit with intuitions in a supposed analogy with how physical laws are adjusted in response to how well they fit observations. The authors of [6] claim "There is agreement on the data to be systematized in the scientific case that has no analog in the mathematical one"

Woodin cardinals, or a proper class of such cardinals. This was recently refuted [identifying citation removed]. Yet for all we know right now, determinacy theories may be cofinal in the consistency strength hierarchy. If so, with sufficient resources devoted to AD research, perhaps they would be discovered in Pudlák's world, in which case we may skip to the next paragraph.

[41] It is significant that even the most vocal opponents of Choice like Baire, Borel, and Lebesgue used Choice unwittingly in their mathematical work [30, §1.7], whereas I am not aware of anyone unwittingly using AD in an argument.

[42] This is not to say that we know what e.g. topology looks like under AD to anywhere near the extent that we should in Pudlák's world.

[6, p.1]. [6, §9] concludes that rather than being akin to empirical science, mathematics and *philosophy* are on a par as "armchair" endeavors because of what the authors take to be essential reliance of justification in mathematics and philosophy on conflicting intuitions.

But there *is* data about which there is widespread agreement and which needs to be systematized in the mathematical case—namely, theorems! In my rational reconstruction, it is crucial that the feedback loop acts on theorems about V, that it stakes its development on conjectures and theorems, that it operates internal to theory. This eliminates the appeals to intuition that [6] argues do not form a common ground among mathematicians in the way empirical observation does for researchers in physical sciences. (Recall that the highly counterintuitive reducibility in the canonical $\neg$CH model did not lead to its being abandoned.) The only intuitions that play a role in the reconstruction are the intuitions that lead an advisor to think it is reasonable to assign a problem to a PhD student, or which suggest a conjecture, a strategy for tackling a problem, an estimation of how far current methods are from yielding a proof of a conjecture, etc. Justifications of our mathematical beliefs do not depend on systematizing *these* intuitions.

The feedback loop leading to the canonical models solution to CH is not an instance of the regressive method, and the notion of solution below treats CH as a paradigmatic mathematical problem. An implication of this paper, then, is that philosophy and mathematics are not on a par, or at least not in virtue of *mathematical* justification intrinsically involving conflicting intuitions or bottoming out in disputes over them. (It would be more correct to say that many mathematicians, *when philosophizing*, do what many philosophers do—appeal to their intuitions.) $\dashv$

3.2 Responses to Lingamneni's objections

The Arbitrary Decision objection assumes a pre-theoretic notion of mathematical truth as in a correspondence theory of reference.[43] Hence the insistence on an "external criterion of set theoretic truth" which a solution to CH should be answerable to. On such a view, the canonical models solution seems to be a reframing, rather than what I take it to be, namely a clarification.

The thrust of Tait's observation is that there is no such external criterion of mathematical truth. The criterion for truth is proof in the theory of the intended

[43]Feferman's argument that CH is inherently vague [10] appeals explicitly to a pre-theoretic idea of arbitrary set of natural numbers. According to Feferman, his idea cannot be faithfully internalized to a theory; any internalization changes the subject. His argument depends on an external question being meaningful.

interpretation of the language. Once we eschew the idea of an external criterion, we can better see what the canonical models solution does: It gives the criterion of truth—proof in the theory of the intended interpretation, i.e. from the axiom V = Ultimate L together with large cardinals—and the solution to CH in tandem. It builds the apparatus necessary for something to even count as a solution to CH in the course of deciding CH.

To ask "But is CH *really* true?" is to insist on the meaningfulness of an external question. There is no possibility of an answer which is not a "reframing." *Every* mathematical problem is internalized to, or mediated by, a theory. Theories—whether pebble arithmetic or the strongest set theories—provide the resources to even articulate problems.

An external criterion of truth is unnecessary for problems to have the character of admitting discoverable solutions. What gives a mathematical problem the character of discoverability is its being internalized to a theory in which the intended interpretation of the relevant language is specifiable. The structural analysis Lingamneni thinks our conception of the universe of sets does not support is our only known means of achieving the determinateness that is required of an intended interpretation of the language of set theory. To treat CH as an ordinary problem with a discoverable solution, it is sufficient to admit a fine structural analysis. It is likely fine structure is necessary for this purpose (see §5.2).

What of the fact that from Ultimate L one can in the generic multiverse access universes of sets in which CH is false? The Reframing objection is that this leaves CH open, for it seems to acknowledge that the problem has merely been reframed in a designed situation. On the contrary, it shows that Ultimate L provides foundations for the practice, which often involves studying aspects of problems by working under various hypotheses. We will return to this in §4.1.

4 What it is to be a solution to the Continuum Hypothesis

The solution to CH consists of

1. a fully determinate interpretation of the language of set theory,

2. argument for why it is the *intended* interpretation,

3. all of the mathematics needed to specify that interpretation,

4. a proof/refutation of CH in the theory of that interpretation, and

5. a way of accessing models of incompatible/false theories.

A few remarks are in order.

Remark 4.1. Showing that the theory is implied by some of its signature consequences—that it is *necessary for those consequences*—will justify the use of the definite article. ⊣

Remark 4.2. All that's missing in the solution to RH is item 4. This is why number theorists seem like fish in water, compared to set theorists—all the conditions that need to be in place for us to have a notion of what a definite solution to a number theoretic problem is are there. Number theorists do not have to think about them. The situation with respect to RH would be exactly analogous to CH if a community of mathematicians in the year 800 C.E.—largely prior to the actual historical development of the informal theory that we have formalized as Peano Arithmetic—developed all of the mathematics needed to obtain a solution to RH and solved it. They would have to do what the canonical models set theorists are doing now—develop mathematics sufficient to solve RH from within. ⊣

Remark 4.3. This notion of "solution" is expansive. It is not just a proof in a formal theory. It includes the justifications given for the theory used and puts strong requirements on that theory. ⊣

4.1 Ghosts in the house of mathematics

Conjectures can function as promissory notes, to be cashed later [27]. Until the Riemann Hypothesis is proved, researchers may entertain both possible outcomes. Littlewood assumed the Riemann Hypothesis and its negation in a proof by cases to show that the differences between (a) the asymptotic estimates on the distribution of prime numbers given by the prime number theorem and (b) the actual numbers change sign infinitely often. Today, number theorists study strong counterexamples to the Riemann Hypothesis called *Siegel zeros* or *exceptional characters*, despite the widespread belief that they will, in the end, not exist. An interesting fruit of this labor is Heath-Brown's theorem [19] that either the Twin Prime conjecture is true, or there are no Siegel zeroes.

But once the Riemman Hypothesis is resolved, on the standard picture the experience researchers have accrued investigating the outcome that does not obtain will be revealed to have been illusory, like assumptions in reductios. Friedlander and Iwaniec write

> Sometimes it almost seems as though there is a ghost in the House of Prime Numbers. Perhaps that will be ruled out some day. There are

> suggestions of a youngster who might do this, one who will come from the Automorphic Room of the house. In the meanwhile, happy-go-lucky prime counters remain temporarily free... to base some fantastic theorems on either of the two assumptions (that exceptional characters exist or don't exist), whichever one their superstitions dictate. [13]

Once one of the assumptions is ruled out, we will see that, e.g., the distribution of primes "necessarily" *couldn't* be as chaotic as that entailed by the nonexistence of Siegel zeros. Siegel zeros will be "wiped out," revealed to be ghosts, and the theorems proved assuming they exist will be vacuous. There is no divergence in number theory.

CH does not seem to conform to this picture. While one of RH or ¬RH is presumably inconsistent with the axioms of mathematics, $\mathsf{ZFC} + \mathrm{CH}$ and $\mathsf{ZFC} + \neg\mathrm{CH}$ are each consistent if ZFC is. It is nearly trivial to transform a model of one to a model of the other. There are thousands of theorems proved assuming hypotheses that entail CH or ¬CH. If CH is true, are all of the ¬CH theorems really vacuous? The feeling that they are not motivated Hamkins to issue the following challenge.

> My challenge to anyone who proposes to give a particular, definite answer to CH is that they must not only argue for their preferred answer, mustering whatever philosophical or intuitive support for their answer as they can, but also they must explain away the illusion of our experience with the contrary hypothesis. Only by doing so will they overcome the response I have described, rejection of the argument from extensive experience of the contrary. Before we will be able to accept CH as true, we must come to know that our experience of the opposing ¬CH worlds was somehow flawed; we must come to see our experience in those lands as illusory. [17, p. 144]

The challenge as I understand it is to explain how the self-consistent scenarios in which CH is false are in fact not possible. This accountability is very unlike what will be required of a solution to RH. We seem to have identified the fundamental distinction between RH and CH: The solution to CH must not "wipe out" the work done under its contrary—there are no ghosts in set theory—whereas on the standard view, the solution to RH *will* wipe out the work done under its contrary.

The standard view, however, is wrong. Suppose RH is true and is proved in ZFC. RH is equivalent to a Π^0_1-sentence [7], and hence it is provably equivalent in Peano Arithmetic to $\mathrm{Con}(T)$ for some formal theory T.[44] Suppose for sake of argument that

[44] From the independence of a Π^0_1-sentence we can essentially infer its truth: If RH is independent of ZFC, then counterexamples cannot be found in ZFC. An independent Π^0_2-sentence of intrinsic

RH has enough logical strength that it is independent of the subsystem of second order arithmetic ACA_0. Then there is a model of $\mathsf{ACA}_0 + \neg$RH, and in that model we can study the computable properties of the prime counting function, the rate of growth of arithmetical functions, Siegel zeros, etc. All that distinguishes RH from CH is the historical accident that RH is researched in an ambient theory in which

1. it promises to be determinate because the categorical intended interpretation of the complex number structure is articulable, and

2. models of $\mathsf{ACA}_0 + \neg$RH are given.

That is, RH is investigated internal to the theory we intend, the one that accounts for all the mathematics of the problem, whereas we're still trying to figure out which theory we mean when it comes to third order arithmetic (where CH "lives") and above. But there are weaker theories in which $\neg$RH can be meaningfully studied. Once there is a ZFC-proof of RH, we will retroactively understand $\neg$RH research as having taken place in models of such theories.[45]

Nothing I have said is particular to RH or CH, either. Call a mathematical problem *almost analytic* if its negation is refuted in the first theory it is expressible in. In other words, a problem is almost analytic if we cannot meaningfully entertain its negation.

Remark 4.4. Here are two examples of almost analytic problems.

1. The Strong Downward Directed Grounds Hypothesis is first statable and provable in ZFC.

2. ATR_0 and Σ^1_1-Separation bear this relationship because they're actually equivalent [36, Theorem V.5.1], and ATR_0 is essentially the first place where Borel sets can be reasoned with. ⊣

An almost analytic problems is *nontrivial* if it is not equivalent to a "comprehension jump."

Conjecture 4.5. *The nontrivial almost analytic problems are sparse in the space of mathematical problems.*

interest would arguably undermine the picture of unambiguous arithmetical truth, for this inference doesn't go through. Are there any?

[45] I say "will" because the Riemann Hypothesis is expressible but unlikely to be provable in $I\Delta_0$.

That is, almost all problems are like CH or RH in that the theory in which they first become provable is much stronger than the theory in which they are first expressible, and the experiences we have with the negations of their truth values as in our default theory are captured in models of subtheories they are independent over and in which they take the opposite truth value. We can meaningfully entertain their contraries, even if we need bounded arithmetics to do so.[46] There is nothing to explain away.

4.2 Comparison with Hamkins' notion of solution

The other notion of solution to CH in the literature is due to Hamkins.

> On the multiverse view the continuum hypothesis is a settled question, for the answer consists of the expansive, detailed knowledge set-theorists have gained about the extent to which the CH holds and fails in the multiverse, about how to achieve it or its negation in combination with other diverse set-theoretic properties. . . . the point is that **the most important and essential facts about CH are deeply understood, and it is these facts that constitute the answer to the CH question**. [17, my emphasis]

As the challenge quoted above attests, Hamkins thinks any stronger or more traditional notion of solution will "wipe out" our experience in contrary universes of sets. I've just argued that this aspect of the conception of ordinary mathematical inquiry is mistaken. If I'm right, we can replace 'CH' with 'RH' in this passage. If having models of a sentence and its negation means that there is no fact of the matter about that sentence, then there is no fact of the matter about RH. The "solution" to RH would then be all the ways of internalizing the question. But this is not *at all* what mathematicians mean when they talk about solving RH. Hence it shouldn't be what we mean by "solving CH." On my view, Hamkins' notion confuses aspects of the problem revealed in different ambient theories with the problem's solution.

[46] [11] is an example of a the kind of research I am describing: Proofs of simple enough facts from the (false) hypothesis that the unit ball in a Hilbert space is strongly compact can be correct, for any proof that the unit ball is not strongly compact must be complicated. Working below that level of complexity, one can derive true facts from the false hypothesis.

As another example, Nelson's [31] develops mathematics in theories in which exponentiation is not total. There are prospects for solving P vs NP—not merely studying aspects of the problem but solving it—by studying the growth of functions in models of such theories.

5 Conclusion

I've argued the Continuum Hypothesis is an ordinary mathematical problem in the same sense that the Riemann Hypothesis is. In particular, it is not to be arbitrarily decided by the mathematical community, and it does not require that the meaning of "solution" be revised. And I've argued that the canonical models solution does not arbitrarily decide CH. It provides the conditions that have to be in place for there to be a solution, together with the solution.

The aim of this final section is to argue that even if Ultimate L does not exist, the eventual solution to CH, if there is one, will be more like it than not: It must specify the intended interpretation of the language of set theory. This is a controversial claim; aside from the canonical models solution, no alleged solution to CH is global in this sense. I will put forward some mathematical questions having to do with the vague question whether the only way to specify the intended interpretation is to provide a fine structure. §5.2 is much more technically demanding than the preceding.

5.1 Intended interpretations of nth order arithmetic

During the canonical ¬CH model phase, Woodin wrote

> ... the incremental approach [towards solving CH] comes with a price. What about the general continuum problem; i.e. what about $H(\omega_3), H(\omega_4), H(\omega_{(\omega_{1+2010})})$, etc.? The view that progress towards resolving the Continuum Hypothesis must come with progress on resolving all instances of the Generalized Continuum Hypothesis seems too strong. The understanding of $H(\omega)$ did not come in concert with an understanding of $H(\omega_1)$, and the understanding of $H(\omega_1)$ failed to resolve even the basic mysteries of $H(\omega_2)$. [46, p. 690]

The view I'm advocating *is* a strong view. Allow me to motivate it.

First, the notion of solution I've put forward is more expansive than "proof of the relevant implication." Our understanding of $H(\omega_1)$ Woodin alludes to is given by Projective Determinacy and involves, on my view, the Martin-Steel proof of PD from large cardinals, the core model induction results that collectively imply that essentially any theory as consistency-wise strong as PD implies that PD is true, and so on. Thus the intended interpretation of second order arithmetic requires for its specification large cardinals in V. The solution yields a local principle but is not itself local to $H(\omega_1)$.

Not only is PD not as local as it superficially seems, but its being implied by large cardinals is atypical. Large cardinals do not settle the theory of $H(\omega_2)$. Arguments for local principles deciding the theory of $H(\omega_2)$ will be weaker, and it's not guaranteed that a given theory of $H(\omega_2)$ will be compatible with the "best justified" theory of $H(\omega_n)$ in the way that the theory of $H(\omega_1)$ is. They stand incomplete, and we don't know how or if they fit into the intended interpretation.

The looser relationship with large cardinals is part of what lies behind the second reason for the global requirement: Various alleged local solutions to CH are mutually incompatible. This state of affairs arises because the solutions do not put enough in place. A pessimistic metainduction here tells us that we have no reason to believe *any* of the local alleged solutions are on the mark. And a big reason for that lies behind the third reason: Unlike first and second order arithmetic (analysis), third order arithmetic isn't entrenched in our way of (mathematical) life sufficiently that we have intuitions to guide us in its development in isolation from the rest of set theory. This is to say nothing of e.g. 12th order arithmetic.

Finally, given how long CH has been open, to work incrementally on CH, then $(2^\omega)^+$, then $(2^\omega)^{++}$, and so on, is to punt on values of the continuum function beyond some small finite number of successors of (2^ω). It is to tacitly accept that higher set theory is meaningless. That cannot be correct insofar as the local solutions require resources from higher set theory for their justifications.

5.2 Fine structure

I will counter an immediate objection to the account in §5.2.1. The remainder of the paper is a discussion of the following questions:

1. Is fine structure necessary for solving some particular problems?
2. Can something akin to fine structure arise from non-inner-model-theoretic or non-invariant processes?
3. Must any invariant way of producing models be fine structural?

5.2.1 The L versus $L[c]$ problem

Underlying Lingamneni's arguments is the question: *Why should a fine structural interpretation of the language of set theory win by (what seems to be) default?* I've intimated the apparent uniqueness of fine structure as a means of eliminating ambiguity from the language of set theory and will expand on it below. It seems that fine structure is constitutive of global alleged solutions, and I've argued that the solution to CH will be global in character, if it exists.

But there seems to be a further worry, namely that a fine structural interpretation of the language of set theory *cannot* win by default: Small generic extensions of fine structural models inherit fine structure from the ground model. Let c be a real which is Cohen generic over L. Then $L[c]$ has a fine structure, and CH may be false in $L[c]$.[47] Similarly, while CH is true in Ultimate L, it is false in the generic extension of Ultimate L by $\aleph_2$-many Cohen reals. Why is Ultimate L the privileged venue for evaluating CH? Why one fine structural interpretation of set theory rather than the other? All that I've pointed to is the determinateness of the intended interpretation given by the canonical models solution, but if that solution pans out, we can generically extend it to a new determinate interpretation. This is the reframing objection restricted to fine structural universes.

As with the original reframing objection, there are multiple reasons this incarnation is not compelling. First, while it may be an interesting question whether there are Ultimate L-generic Cohen reals, it's not a question evidence will ever bear on. We can dismiss it out of hand. Second, fine structural models are built without arbitrarily coding information into their extender sequences. They're 'unbiased'. The only reason to propose $V = \mathsf{Ult} - \mathsf{L}[c]$ is if you're biased towards ¬CH.[48]

Third, there is an asymmetry in that we can distinguish Ultimate L from its generic extensions in a principled way. For example:

Theorem 5.1. *Assume* AD^+*. Suppose*

$$\mathsf{HOD} \models \text{"there exist arbitrarily large Woodin cardinals."}$$

Let c *be a Cohen real generic over* HOD*. Then*

$$\mathsf{HOD}_c \models V = \mathsf{Ult} - \mathsf{L}.$$

And in the $\mathsf{Ult} - \mathsf{L}[x]$ case, where x is more complicated than a Cohen real, $\mathsf{HOD}^{\mathsf{Ult}-\mathsf{L}[x]}$ will be a "corruption" of the usual fine structural HOD:

Theorem 5.2. *Suppose* $V = \mathsf{Ult} - \mathsf{L}[x]$*. Then* HOD *is a hybrid mouse with an extra Woodin cardinal at the bottom, together with a fragment of its iteration strategy at its first Woodin cardinal.*

[47]But the theory of $L[x]$, for $x \in \mathbb{R}$, stabilizes on a Turing cone, and CH is in this theory, so there is a principled reason to dismiss $L[c]$ as a viable candidate for V (apart from its smallness). We will do the same for small generic extensions of Ultimate L below.

[48]I will use the notion $V = \mathsf{Ult} - \mathsf{L}$ for the axiom $V =$ Ultimate L to make statements of theorems cleaner.

Generally, we can distinguish the small forcing extensions of hod mice by the fact that they do not satisfy the Ground Axiom:

Theorem 5.3. *Assume* AD^+. *Then for a Turing cone of* x,

$$\mathsf{HOD}_x \not\models \text{ the Ground Axiom.}$$

Finally, there is a crucial methodological asymmetry: To define the generic extensions of a canonical model, we need to antecedently know the canonical model. $L[c]$ wasn't discovered before L was defined, and that's not an accident of forcing being discovered after Gödel specified L. The natural model conceptually precedes its generic extensions. It is also worth noting that once it is forced that $V \neq L$, it cannot then be forced that $V = L$, and this generalizes to larger canonical models.

5.2.2 Is fine structure necessary for solving some particular problems?

It is mostly futile to predict methodological developments in mathematics, so I will state two basic questions one can ask about a given model of set theory which so far have exclusively been answered using fine structure.

(1) Does $V = \mathsf{HOD}$ hold?

With respect to the natural models in which $V = \mathsf{HOD}$ holds, the proofs utilize fine structure. In many cases, like in AD^+ models, HOD in turns satisfies $V = \mathsf{HOD}$, and the arguments establishing this are again fine structural. Unless $V = \mathsf{HOD}$ is adopted as an axiom, it's likely that any explanation of where the wellordering of the reals comes from will need to appeal to fine structural analysis.

If $V \neq \mathsf{HOD}$ holds, what is HOD? Is it describable as a canonical structure? What is $\mathsf{HOD}^{\mathsf{HOD}}$, and so on? For example, $\mathsf{HOD}^{L[x]} \not\models V = \mathsf{HOD}$, but it's "HODs all the way down" in the sense that in $L[x]$, $\mathsf{HOD} \neq \mathsf{HOD}^{\mathsf{HOD}} \neq \mathsf{HOD}^{\mathsf{HOD}^{\mathsf{HOD}}} \ldots$ The problem in this situation is that the mantle of $L[x]$ is not a ground.[49] Sufficient large cardinals rule this out and ensure that $V = \mathsf{HOD}$ persists to the mantle.

Theorem 5.4. *Assume there is an extendible cardinal. If* $V = \mathsf{HOD}_A$ *for some set* A, *then* $\mathbb{M} \models V = \mathsf{HOD}$.

(2) Is there is a $(\Sigma^2_1)^{\mathrm{Hom}_\infty}$-wellordering of the reals?

[49] A transitive class W is a *ground* of V if there is a forcing notion $\mathbb{P}$ and a filter $g \subseteq \mathbb{P}$ generic over V such that $V = W[g]$. The *mantle* is the intersection of all the grounds of V.

Even if a theory does not imply there is a $(\Sigma_1^2)^{\mathrm{Hom}_\infty}$-wellordering of the reals, there's still the natural question *How many* $(\Sigma_1^2)^{\mathrm{Hom}_\infty}$*-in-a-countable-ordinal reals are there?*, and it's reasonable to believe that this can only be answered with a fine structural analysis. This is not an idle question; for example, its solution can be used to show that Martin's Maximum does not imply generic absoluteness for the universally Baire sets of reals.[50]

5.2.3 Can fine structure arise from a non-invariant method for producing models?

Generic extensions do not admit a fine structural analysis in general. They do when they arise from forcing over a canonical inner model, and as we said above, the structural analysis of the generic extension traces back to that of the ground model. Fine structure is not known to be forceable in the absence of fine structure in the ground model. Is this necessarily the case? One near counterexample is the Shelah-Stanley forcing principle equivalent to the existence of morasses in L [34, 35]. But when it comes to properties like solidity of the standard parameter, condensation, having a representation as a directed system of suitable iterable mice, or admitting an analysis showing when new reals enter the model and computing the wellordering of the reals, the potential relevance of forcing is unclear.

Is forceability of a property disqualifying of its being *fine structural*? For example, it seems fair to say the combinatorial principle $\square_\kappa$, which is easy to force, is not fine structural but that fine structure has been used to prove that $\square_\kappa$ holds in a large class of canonical models. On the other hand, $\square_\kappa$ fails whenever κ is subcompact, and hence at many cardinals in Ultimate L, which is fine structural (if it exists). I think similar remarks apply to *Local Club Condensation* [14], which is essentially an attempt at answering this section's title.

Here are two conjectures whose purpose is to probe how hard it is to force fine structural features "out of thin air."

Strong Condensation

Woodin [48] defined a generalization of condensation in L. Let M be a rud-closed transitive set and suppose $F : \mathrm{Ord} \cap M \to M$ is a bijection. Then F witnesses *Strong Condensation* for M if for any $X \prec (M, F)$,

$$F_X = F \restriction (\mathrm{Ord} \cap X).$$

[50] Ultimate L has a $(\Sigma_1^2)^{\mathrm{Hom}_\infty}$-wellorder. Force MM^{++} over Ultimate L. Then the $(\Sigma_1^2)^{\mathrm{Hom}_\infty}$-in-a-countable-ordinal reals form an ω_1-sequence of distinct reals, and hence $L(\Gamma^\infty, \mathbb{R}) \not\models \mathsf{AD}^+$.

If there is such a witness for M, then *Strong Condensation holds* for M. Wu and independently Friedman and Holy [14] forced Strong Condensation for $H(\omega_2)$.

Conjecture 5.5. *There is no generic extension of V in which Strong Condensation holds for $H(\omega_3)$.*

The Mantle Conjecture

The Mantle conjecture is intended to recover $V =$ Ultimate L from some of its signature consequences. (Recall the remark about the definite article in §4.) Its relevance here is that it illustrates how hard it is to obtain a pathological model of the *Ultrapower Axiom* [16], a generalization of the comparison process of inner model theory. Comparison generates fine structure.

Recall the *mantle* $\mathbb{M}$ is the intersection of all the grounds of V. Goldberg has conjectured

Conjecture 5.6 (The Mantle Conjecture)**.** *Assume there are arbitrarily large extendible cardinals. Suppose*

$$\mathbb{M} \models \text{"the Ultrapower Axiom."}$$

Then

$$\mathbb{M} \models V = \mathsf{Ult} - \mathsf{L}.$$

Refuting the Mantle Conjecture seems to require forcing to create pathological models of the Ultrapower Axiom.

5.2.4 Must any invariant way of producing models of set theory be fine structural?

One way of making this question precise is as follows. An *inner model operator* [37] is a Turing invariant function $\mathscr{M} : \mathbb{R} \to \wp(\mathbb{R})$ assigning to each real x (1) a countable Turing ideal M_x closed under Turing jump and containing x and (2) a wellorder of M_x. The maps associating a real x to a canonical inner model over x (like $L[x]$, HOD_x) are the paradigmatic inner model operators. The notion is intended to characterize natural models of set theory. Steel showed that assuming AD,[51] for any inner model operator $\mathscr{M}$, either $\mathbb{R} \cap M_x \subseteq L[x]$, or $x^\# \in M_x$. This implies that many generic extensions are not given by inner model operators.

An inner model operator is *fine structural* if and only if for a Turing cone of x there is an $\omega_1 + 1$-iterable countable x-premouse P_x such that $M_x = \mathbb{R} \cap P_x$ [40].

[51] AD is reasonable since we are concerned with invariant—hence definable—ways of producing models of set theory.

Conjecture 5.7 (AD). *Every inner model operator is fine structural.*

Let $\mathscr{M} \leq_m \mathscr{N}$ if for a Turing cone of x, $M_x \subseteq N_x$. Rudominer and Steel have shown that assuming AD, if $\mathscr{N}$ is a fine structural inner model operator and $\mathscr{M} \leq_m \mathscr{N}$, then $\mathscr{M}$ is, too [40]. In the presence of AD, the full Conjecture 5.7 is equivalent to one of the primary movers of the canonical models program.

Theorem 5.8 (B.-Siskind). *Assume* AD*. Then every inner model operator is fine structural if and only if the Mouse Set Conjecture holds.*[52]

The main theorem of [3] is that assuming AD, if $\mathscr{M}$ is an inner model operator, then for a Turing cone of x, $M_x \models$ CH. Since I don't want to put my thumbs on the scales regarding CH, what if AD is dropped? Assuming $\mathsf{ZF} + \mathsf{DC}$ and every real has a sharp, Woodin has shown that there is an inner model operator $\mathscr{Q}$ such that $Q_x \models 2^{\omega_2} > \omega_3$ on a Turing cone of x by producing generics in a Turing invariant way. What other inner model operators exist in this context? More broadly, *is there another way to characterize invariant ways of producing models of set theory?*

Conclusion

Earlier I argued for the general form a solution to CH must take. In this section I have defended the claim that fine structure, in some form, is essential to any claim to have solved CH. For there are questions which seem to require fine structure to answer (§5.2.2); and if the speculations of §5.2.3 are correct, then non-invariant methods for building models of set theory—like generic extension—cannot replicate fine structure, and hence they arguably cannot remove all ambiguity from the language of set theory; and if §5.2.4 and, in particular, Conjecture 5.7 are correct, then every invariant method for building models would seem to be fine structural. I tentatively conclude that even if the canonical models solution doesn't pan out, *if there is an eventual solution*, it will provide a canonical theory of sets via something we would recognize as fine structure.

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