Justin Clarke-Doane

Columbia University

[This is a corrected version of an article which appears in *Noûs*, Vol. 47.[1]]

What is Absolute Undecidability?[2]

It is often alleged that, unlike typical axioms of mathematics, the Continuum Hypothesis (CH) is indeterminate. This position is normally defended on the ground that the CH is undecidable in a way that typical axioms are not. Call this kind of undecidability "absolute undecidability". In this paper, I seek to understand what absolute undecidability could be such that one might hope to establish that (a) CH is absolutely undecidable, (b) typical axioms are not absolutely

---

[1] The published version of this paper contained the following errors:

p. 479: In footnote 24, "2100" should be "2^100" and "Yessenin-Volpin" should be "Esenin-Volpin".

p. 480: Footnote 30 should refer to p. 4, footnote 18, of Dehornoy [2003], not to Woodin [2001a] and [2001b].

pp. 474 and 476: I claim that Edward Nelson rejects the Successor Axiom, in addition to Mathematical Induction. This was unclear. Nelson's base theory is Q, and this implies that for every x, there is a y, and y is the successor of x. What Nelson appears to deny ([1986], 176) is that for every *actual* (or "genetic") x, there is an actual y, and y is the successor of x (he instead advances an inference rule to which the Deduction Theorem is supposed not to apply). See Nelson's discussion at: http://mathoverflow.net/questions/142669/illustrating-edward-nelsonsworldview-with-nonstandard-models-of-arithmetic For a more straightforward rejection of the Successor Axiom, see Doron Zielberger, ""Real" Analysis is a Degenerate Case of Discrete Analysis", in Aulbach, Bernd, Saber N. Elaydi, and G. Ladas (eds.), *Proceedings of the Sixth International Conference on Difference Equations*, Augsburg, Germany: CRC Press, p. 33. (Thanks to Walter Dean for discussion.)

p. 480 (Bibliography): Arrigoni, Tatiana. [2011] should appear in the *Bulletin of Symbolic Logic*, not in the *Review of Symbolic Logic*.

p. 481 (Bibliography): The following item is missing.

Dehornoy, Patrick. [2003] "Recent Progress on the Continuum Hypothesis (After Woodin)." Manuscript. Available online at: http://www.math.unicaen. fr/˜dehornoy/Surveys/DgtUS.pdf

undecidable, and (c) if a mathematical hypothesis is absolutely undecidable, then it is indeterminate. I shall argue that on no understanding of absolute undecidability could one hope to establish all of (a) – (c). However, I will identify one understanding of absolute undecidability on which one might hope to establish both (a) and (c) to the exclusion of (b). This suggests that a new style of mathematical antirealism deserves attention -- one that does not depend on familiar epistemological or ontological concerns. The key idea behind this new view is that typical mathematical hypotheses are indeterminate because they are relevantly similar to CH.

The Argument for the Indeterminacy of CH

George Cantor's Continuum Hypothesis (CH) is the conjecture that there is a bijection between every uncountable subset of the real numbers and the whole set of them, or, equivalently, given the Axiom of Choice, the conjecture that the cardinal number of the real numbers is the next greatest after the cardinal number of the natural numbers. It is often alleged that, unlike typical mathematical hypotheses, CH may be indeterminate. For example, Solomon Feferman writes,

> [D]espite all [the recent progress in set-theory]…the Continuum Hypothesis is *still* completely undecided, in the sense that it is independent of all remotely plausible axioms…including all "large" large cardinal axioms which have been considered so far….That may lead one to raise doubts not only about [the program of trying to discover the truth-value of CH] but its very presumptions. Is the Continuum Hypothesis a definite problem as Gödel and many current set-theorists believe? [Feferman 2000, 404 – 405]

Similarly, Donald Martin remarks,

Those who argue that the concept of set is not sufficiently clear to fix the truth-value of CH have a position which is at present difficult to assail. As long as no new axiom is found which decides CH, their case will continue to grow stronger, and our assertion that the meaning of CH is clear will sound more and more empty [Martin 1976, 90 – 91].

Feferman's and Martin's concern is that CH may be broadly analogous to the hypothesis that a man with a borderline-number of hairs on his head is bald. It is widely believed that the question of whether the latter hypothesis is true is, in some sense, not a fully factual one. Of course, the latter hypothesis is *vague*, and CH may not be vague *per se*. The properties mentioned in CH may not admit of borderline cases. But it may still be that the question of whether CH is true fails to be fully factual is something like the way that such vague hypotheses so fail.[3]

Feferman and Martin do not doubt that *typical* mathematical hypotheses are determinate. For example, they do not doubt that the Axiom of Pairs in set theory or the Successor Axiom in arithmetic is determinate. Nor do they doubt that hypotheses that (first-order) follow from such axioms – i.e. typical theorems -- are determinate. They hold that there is something *special* about CH such that, unlike typical mathematical hypotheses, it may be indeterminate.

---

[3]Of course, on *some* views of indeterminacy -- namely Williamson [2004] -- the question of whether the hypothesis that a man with a borderline-numbers of hairs on his head is bald is true does *not* fail, in any sense, to be fully factual. I believe that the argument that I will make here would be sound even given a Williamsonian understanding of indeterminacy. But I assume for the purposes of this paper that such an understanding of indeterminacy is incorrect, as I take typical advocates of the indeterminacy of CH to assume.

Of course, in suggesting that CH may be indeterminate, Feferman and Martin are not denying

that either CH or its negation is determinately true *relative to a model*. Given the determinacy of

first-order logic, CH is obviously determinately true relative to any model of ZFC + CH.

Feferman and Martin doubt that CH or its negation is determinately true in a *non-relative* way,

the way in which typical axioms of mathematics are commonly thought to be true. For example,

it is commonly thought that the Successor Axiom (SA) of arithmetic expresses an objective

mathematical truth -- the objective truth that every natural number has a successor. While it is,

of course, true that *relative to models of PA* every natural number has a successor, it is often

thought that there is an *intended* class of models and that SA is determinately true in *that* class.[4]

One way of characterizing the question that concerns Feferman and Martin is as the question of

whether the intended class of models suffices to make CH or its negation determinately true.

Feferman and Martin are not explicit about the special feature they take CH to have and typical

mathematical axioms to lack such that CH, unlike typical axioms of mathematics, may be

indeterminate. But the intuitive idea seems to be that CH is undecidable in a way that typical

axioms are not. For example, Feferman writes, "…the Continuum Hypothesis is *still* completely

undecided, in the sense that it is independent of all remotely plausible axioms…", and Martin

qualifies his statement, "As long as no new axiom is found which decides CH…" For

convenience, I shall call the relevant kind of undecidability "absolute undecidability".[5] In what

follows, I take up the question of what absolutely undecidability could be such that one might

hope to establish that (a) CH is absolutely undecidable, (b) typical axioms are not absolutely

---

[44] There are familiar reasons to take this point of view seriously. For example, we know from Godel that PA + Con(PA) and PA + ~Con(PA) are each true relative to some model (if PA is). But, *prima facie*, PA + Con(PA) must be determinately true, while PA + ~Con(PA) must be determinately false (if PA is true, and, hence, consistent).

[5] I borrow the term "absolutely undecidable" (or "unsolvable") from Godel [1951]. See also Koellner [2009].

undecidable, and (c) if a mathematical hypothesis is absolute undecidable, then it is indeterminate.

Absolute Undecidability as Formal Undecidability with respect to Plausible Axioms

The *prima facie* obvious answer to the question at issue is simply that absolute undecidability is (first-order) *formal undecidability*. Neither CH nor its negation is (first-order) provable from the axioms. The problem with the obvious answer is that of specifying the set of axioms with respect to which CH is supposed to be formally undecidable. For a start, of course, one might claim, with Feferman, that CH is formally undecidable with respect to all *plausible* axioms. But this just raises the question of what counts as a plausible axiom.[6] Typical advocates of the determinacy of CH *deem* sets of axioms as plausible with respect to which CH is *not* formally undecidable. For example, Devlin deems $V = L$ as plausible.[7] But $V = L$ proves both CH and the Generalized Continuum Hypothesis. Similarly, Hugh Woodin claims that we should want an Omega-complete theory of $H(w2)$, the level at which CH lives.[8] But, given the Strong Omega-conjecture (the conjunction of a completeness conjecture for Omega-logic and the conjecture that the AD+-conjecture is Omega-valid), Woodin proved that any Omega-complete theory of $H(w2)$ Omega-implies not-CH.[9] Finally, Foreman maintains that "Generic Large Cardinal Axioms"

---

[6] I treat plausibility as a stance-independent feature of axioms, as Koellner [2009] or Maddy [2011] treat cognate notions. I will say that A *deems* axiom p plausible where one might have said p is (stance-dependent) plausible *relative to* A. See the next section for discussion of an analysis of absolute undecidability in terms of what mathematicians *deem* plausible.

[7] See Devlin [1973], [1977], or [1984]. See also Jensen [1995] and Arrigoni [2011].

[8] See Woodin [2001a] and [2001b]. (Woodin's view regarding CH may be changing. This will not affect the basic line of argument in what follows. For a popular discussion of Woodin's current view, and one alternative to it, see Wolchover [2013].)

[9] See Koellner's [2009].

(GLC) inherit the plausibility of traditional large cardinal axioms.[10] However, like V = L, GLC

prove both CH and the Generalized Continuum Hypothesis.

Of course, there might be non-question-begging arguments against some of these positions. For

example, suppose that the Strong Omega-Conjecture is false. Then, since Woodin's position

depends on it, a disproof of the Strong Omega-conjecture would undermine Woodin's

position. Similarly, it is sometimes doubted whether Forman's GLC are consistent. Suppose

that they are not. Then, since Foreman's position depends on the consistency of GLC, a proof of

their inconsistency would undermine Foreman's position. But Devlin's view that V = L is

plausible depends on no outstanding conjectures (save that V = L is consistent, which is hardly

doubted). Moreover, the Strong Omega-Conjecture may well be true and Foreman's GLC may

well be consistent. In the absence of *novel challenges* to the plausibility of such principles, the

suggestion that CH is formally undecidable with respect to plausible axioms just blatantly

assumes what typical advocates of the determinacy of CH explicitly deny. It seems, then, that at

least if one is going to argue for (a) – (c) in a non-question-begging way, absolute undecidability

cannot be understood simply as formal undecidability with respect to plausible axioms.

As Formal Undecidability with respect to Axioms that are Deemed Plausible

Rather than trying to understand absolute undecidability in terms of the contentious notion of a

plausible set of axioms, perhaps it should be understood in terms of the more tractable notion of

a set of axioms that is *deemed* plausible. The problem with CH, it might be thought, is precisely

that the plausibility of axioms that formally decide it *is contentious*. At a first approximation, we

---

[10] See Foreman Ibid.

might say that, unlike typical mathematical hypotheses, CH is absolutely undecidable in the sense that it is formally undecidable with respect to axioms that are *deemed* plausible by most.

But what is the group with respect to which this hypothesis is supposed to be evaluated? It cannot be *all* mathematicians. It is doubtful that typical mathematicians have a view as to which axioms are plausible (it is doubtful that they could even name the axioms of a single system of set theory).[11] But if there is no non-empty set of axioms that most mathematicians deem plausible, then *every* axiom is absolutely undecidable in the present sense (because every axiom is formally undecidable with respect to the empty set of axioms).[12] But nor can the relevant group simply be mathematicians with views as to which axioms are plausible. Many such mathematicians may not be in a position to assess axioms that formally decide CH. Vanishingly few mathematicians actually work on CH, and arguments and evidence relevant to it are highly inaccessible to the non-specialist. The fact, if it is one, that CH is formally undecidable with respect to axioms that are deemed plausible by most mathematicians with views as to which axioms are plausible only because those among them who are not familiar with relevant arguments and evidence continue to disagree does nothing to suggest that CH is indeterminate.[13]

---

[11] For an assessment of the typical mathematician's knowledge of foundational matters (which still seems accurate), see Kriesel [1967].

[12] That is, every axiom which is consistent and not a (first-order) logical truth is so undecidable.

[13] Indeed, even if CH does not afford a real life example of an axiom that primarily generates disagreement among non-experts, Projective Determinacy (PD) may well. It may be that PD fails to enjoy convergence among mathematicians with views as to which axioms are plausible. But the case for PD, which has only been developed over the last several decades, is highly subtle, turning on a wide body of results concerning the properties of "simple" sets of reals and connections between determinacy and large cardinal principles. Moreover, *among those who seem privy to such arguments and evidence* there may well be majority agreement as to the truth of PD. For more on the virtues of PD, see Woodin [2001a].

A more promising suggestion is that the relevant group is mathematicians with views as to which axioms are plausible *that are familiar with arguments and evidence relevant to CH*. It is plausible that CH is formally undecidable with respect to axioms that most such mathematicians deem plausible. Mathematicians with views as to which axioms are plausible that are familiar to arguments relevant CH are primarily limited to those that actually work on CH and intimately related questions – i.e. to the likes of Devlin, Woodin, and Foreman. It does not seem that there is majority convergence among such mathematicians with respect to a set of axioms that formally decides CH. Nevertheless, it is doubtful that (b) typical axioms of mathematics are not absolutely undecidable in the present sense. It seems that among those mathematicians with views as to which axioms are plausible *that are familiar with arguments relevant to an axiom, A* -- typically, those who actually work on axiom, A, and intimately related matters -- there is normally a great deal of disagreement as to the truth-value of A. As Thomas Forster writes,

> [F]or people who want to think of foundational issues as resolved…[the standard axioms provide] an excuse for them not to think about foundational issues any longer. It's a bit like the role of the Church in Medieval Europe: it keeps a lid on things that really need lids. Let the masses believe in [standard] set theory. To misquote Chesterton "If people stop believing in [standard] set theory, they won't believe nothing, they'll believe anything [Forster Forthcoming, 15]![14]

---

[14] See also Bell and Hellman [2006], p. 64. They write, "Contrary to the popular (mis)conception of mathematics as a cut-and-dried body of universally agreed upon truths…as soon as one examines the foundations of mathematics [the question of what axioms are true] one encounters divergences of viewpoint…that can easily remind one of religious, schismatic disagreement."

Of course, it might be thought that there is not more agreement among those working on typical axioms such as the Least Upper Bound axiom (LUB) because most mathematicians believe that they, unlike CH or its negation, are determinately true, and see no more reason to become acquainted with serious research into their truth-values than to become acquainted with serious research into the truth-value of F = ma. But, first, the fact remains that such mathematicians are unlikely to be familiar with arguments and evidence relevant to the likes of LUB -- just as those who do not directly work on CH are unlikely to be familiar with arguments relevant to CH. It is hard to see, again, how agreement over the likes of LUB among the relevantly ignorant could help to establish that LUB is determinate. Second, it remains unobvious at best that most mathematicians have a serious view as to the question of whether such axioms as LUB are determinately true or false. Mathematicians are overwhelmingly concerned with questions of *logic* -- questions of *what follows from what*. It is an open question at best whether those among them who do not directly acquaint themselves with work relevant to the truth-value of the likes of LUB nonetheless harbor serious views on the matter.

However, even if it *were* the case that (b) typical axioms are not absolutely undecidable in the present sense, it would still be doubtful that (c) if a mathematical hypothesis is absolutely undecidable, then it is indeterminate. There is a problem with *any* suggestion that takes numbers of mathematicians who *actually* happen to disagree over a hypothesis, H, to settle the determinacy of H – even if those mathematicians are relevantly knowledgeable. How opinions are distributed on an issue tends to depend on highly contingent factors that seem irrelevant to the determinacy of H. For example, it might be that Foreman's point of view on CH has not generated majority convergence among those familiar with relevant arguments and evidence

simply because he has had very few graduate students, or because the continuum problem is not

a significant area of research at his university, or because his approach is especially difficult to

understand. If the world were slightly different from this one, Foreman's GLC would have

commanded majority convergence among relevantly knowledgeable mathematicians, and CH

would not have been absolutely undecidable in the present sense after all. It is hard to believe

that, given that CH would have been determinate in such a world, it is indeterminate in ours.[15]

As Non-Unique Decidability with respect to Axioms Endorsed by Cognitively Flawless Agents

This suggests that the way in which actual disagreement over a hypothesis, H, among relevantly

knowledgeable mathematicians might bear on the absolute undecidability of H is *indirectly*.

Disagreement over H among the relevantly knowledgeable may afford *defeasible reason* to think

that there *could* be disagreement as to that truth-value of H among *cognitively flawless agents* --

and H is *absolutely undecidable* just in case there could be such disagreement.[16]

By *cognitively flawless agents*, I mean agents whose cognitive virtues can be specified in a way

that is independent of the status of the hypothesis at issue. Roughly, an agent is *cognitively

flawless* with respect to a hypothesis, H, if it is logically omniscient, competent with concepts

relevant to H, perfectly imaginative, sincere, attentive, and so on. (Such agents must not be

omniscient *in general*, since then they would trivially agree as to the status of H.) The intuitive

idea of a cognitively flawless agent with respect to a hypothesis, H, is that of an agent that does

---

[15] This follows from the assumption that a given mathematical hypothesis has whatever truth-value status it has of necessity. But it should seem plausible even to one, such as Field [1989] or Balaguer [2001], who denies this.
[16] If we assume that for any mathematical hypothesis, H, any cognitively flawless agent either endorses H or endorses not-H, then the current proposal says that a hypothesis is absolutely undecidable just in case it is formally undecidable with respect to the set of axioms that would be endorsed by any cognitively flawless agent.

the best it theoretically could with respect to H, given its evidence (e.g. observations or intuitions).

This understanding of absolute undecidability finally makes it intelligible why someone might hold that (c) if a mathematical hypothesis is absolutely undecidable, then it is indeterminate. There is something *prima facie* bizarre in the view that there are mathematical truths that radically outstrip out ability to detect them, in the sense that even a *cognitively flawless* agent could fail to grasp them. In this respect, mathematics may differ from empirical science. Arguably, the view that there could be physical truths that are undetectable by a cognitively flawless agent is all but constitutive of physical realism. But the corresponding view in mathematics is harder to take seriously. Indeed, I am not even sure that *Godel*, commonly regarded as the most ardent platonist, holds that there are mathematical truths that even *cognitively flawless* agents could fail to detect.[17] I suspect that this is why he, like many mathematicians and philosophers of mathematics, takes undecidability results in mathematics to have potentially great significance for the (objectivity and) determinacy of the subject, while he does not evidently take undecidability results in empirical science to have such significance.

What accounts for this difference between mathematics and empirical science? One natural thought runs as follows.[18] The possibility of cognitively flawless disagreement over a hypothesis, H, suggests that our corresponding concepts are not fine-grained enough to fix the truth-value of H by themselves. And our reference in intuitively *a priori* areas, such as

---

[17] For example, in Godel [1964], Godel seems to suggest -- with Martin [1976] -- that whether a mathematical proposition is determinate depends on whether its truth-value is "fixed" by our concepts. On a natural interpretation of "the truth-value of p is fixed by our concepts", this is something that any cognitively flawless agent would know.
[18] See Field [1998] for an argument in the spirit of this one.

mathematics, is determined by our concepts if it is determined at all (after all, there do not seem to be any causal relations between the likes of numbers and ourselves that could help to fix reference). Thus, if H is a mathematical hypothesis, then the fact that our relevant concepts fail to determine their reference sufficiently to fix the truth-value of H suggests that the truth-value of H is not determinately fixed. By contrast, it is generally believed that we are still able to refer determinately-enough with intuitively empirical concepts like "water" or "electron" to fix the truth-value of a given hypothesis, H', involving those concepts, even though cognitively flawless disagreement over H' may be possible, because causal chains help to tie down the extensions of those concepts. As a result, the possibility of cognitively flawless disagreement over a given empirical hypothesis does not, by itself, suggest that that hypothesis is indeterminate.[19]

In fact, the present understanding of "absolutely undecidable" makes the argument from the absolute undecidability of CH to its indeterminacy a special case of a style of argument that is commonly advanced with respect to intuitively *a priori* domains. It is commonly argued that because cognitively flawless disagreement over hypotheses from an intuitively *a priori* area, such as metaphysics or morality, is possible, such hypotheses may fail to be determinate.[20] The argument begins that hypotheses of the relevant kind, F, would be *a priori* knowable. But, if they would be *a priori*, and if there were any interesting determinately true F-hypotheses, then, platitudinously, any two agents in the same cognitive situation with respect to those hypotheses would agree as to their truth-values. However, the argument continues, two agents in the same –

---

[19] Note that this account of the significance of cognitively flawless disagreement over mathematical hypotheses is quite different from the one that a Dummettian intuitionist might give. Dummett does not acknowledge an in principle difference between the mathematical case and the empirical one. See, for instance, Dummett [1991].
[20] See Wright [2004]. For a discussion of the argument as applied to morality, see Schiffer [2002] or Schroeter and Schroeter [2012].

ideal -- cognitive situation with respect to any interesting F-hypotheses could *disagree* as to their truth-values. It follows that there are no interesting determinately true F- hypotheses.

Finally, the present understanding of absolute undecidability makes it understandable why someone might hold that (a) CH is absolutely undecidable. It is plausible that there could be cognitively flawless agents who disagree over CH. The reason is that there *actually has been* disagreement over CH that does not seem to result from lack of relevant logical knowledge, conceptual incompetence, insincerity, inattentiveness, and so on. For example, it does not seem that the dispute between advocates of V=L with Woodin is ultimately explicable in terms of lack of logical knowledge, conceptual incompetence, insincerity, inattentiveness, and so on. *Prima facie*, this dispute would endure even if parties to it became logically omniscient, perfectly sincere, perfectly attentive, and so on.[21] Note that the plausibility of (a) is not hostage to the *current* existence of parties to the dispute. Even if everyone who disagrees over CH were to die tomorrow, it might still be plausible that CH is absolutely undecidable in the present sense. As long as there *was* an advocate of CH, and an advocate of not-CH, that appeared to be conceptually competent, logical, sincere, attentive, and so on, there might still be (non-question-begging) reason to think that there could be cognitively flawless disagreement over CH.

I submit that the suggestion that a mathematical hypothesis is absolutely undecidable just in case there could be cognitively flawless disagreement with respect to it is the best understanding of absolute undecidability available to the advocate of the argument from the absolute undecidability of CH to its indeterminacy. Unlike the suggestion that absolute undecidability is

---

[21] Rather than reflecting a cognitive deficit of some sort, Devlin and Woodin's dispute seems to reflect what Jensen calls in his [1995], "deeply rooted differences in mathematical taste" (p. 401).

(first-order) formal undecidability with respect to any plausible set of axioms, the present

suggestion does not result in an argument from the absolute undecidability of CH to its

indeterminacy that simply begs the question against typical advocates of the determinacy of CH.

And unlike the suggestion that absolute undecidability is formal undecidability with respect to

axioms that are deemed plausible by certain actual mathematicians, it does not modally tie

absolute undecidability to intuitively irrelevant aspects of the contingent distribution of belief.

Finally, the present understanding of absolute undecidability makes the argument from the

absolute undecidability of CH to it indeterminacy a special case of a style of argument that is

commonly endorsed outside of the philosophy of mathematics. The problem with the present

understanding of absolute undecidability is that it is false that (b) typical axioms are not

absolutely undecidable under the present understanding. Indeed, (b) fails in a radical way.

The Generality of Absolute Undecidability

Recall that the (non-question-begging) reason for thinking that cognitively flawless agents could

disagree over CH is that there actually *have been some* apparently logical, sincere, attentive,

informed, etc. mathematicians who are competent with the relevant concepts and who disagree

over CH. It is irrelevant how many such mathematicians there happen to have been. It is also

irrelevant whether such mathematicians are currently alive. But there have been *some* such

mathematicians that have disagreed over mathematical hypotheses besides CH. For example,

Michael Potter has challenged the axioms of Choice and Replacement[22] and Peter Azcel has

challenged the Axiom of Foundation.[23] Potter and Azcel appear to be intellectually virtuous in

pertinent respects and competent in relevant respects. It is doubtful that *all* such mathematicians

---

[22] See Potter [2004]. See also Boolos [1999].
[23] See Aczel [1988]. See also Quine [1937].

would converge on a view *vis a vis* the likes of Choice, Replacement, and Foundation if they were to learn more logic, were to become more sincere, attentive, imaginative, and so on.

Indeed, there *have been some* apparently conceptually competent, logical, sincere, attentive, etc. mathematicians who have challenged practically every "typical" axiom of set theory. For example, there have been some that have challenged Infinity and Powerset.[24] There have also been some such mathematicians that have challenged the characteristic axiom of the calculus, the Least Upper Bound axiom (LUB). When Weyl proclaimed "in any wording, [LUB] is false" he did not seem to be guilty of conceptual incompetence or a pertinent intellectual failing.[25] There have even been *some* mathematicians who have rejected axioms of *arithmetic*, especially (instances of) Induction.[26] For example, the Princeton mathematician, Edward Nelson, rejects this axiom.[27] Perhaps he fails to believe truths in so doing. But it does not seem that he is incompetent with the concepts of arithmetic. Nor does it seem that he would change his view if he were to learn more logic, were to become more sincere, attentive, imaginative, and so on. All the evidence suggests that Nelson's view -- that, on the one hand, all mathematical axioms should be predicatively acceptable, and, on the other, Induction is not -- is *plausible by his lights*.[28] Given that the (non-question-begging) reason to think that cognitively flawless agents

---

[24] Finitists challenge Infinity. For critical discussions of Powerset, see Boolos [1999], Forster [Forthcoming], Fraenkel, Bar-Hillel, and Levy [1973], and Quine [1969]. (In the published version of this article, I also mention challenges to the Axiom of Extensionality. But I now think that this case is complicated in distracting ways.)

[25] Quoted in Kilmister [1980], p. 157.

[26] See Nelson [1986]. See also Friedman [2002] and Esenin-Volpin [1970]. Friedman writes, "I have seen some…go so far as to challenge the existence of 2^{100} as a natural number…[2002, p. 4]."

[27] See Nelson [1986] and Buss [1996].

[28] Of course, from *our* perspective there is a great deal of implausibility to Nelson's point of view. For example, it is a theorem, due to Visser [1990], that the totality of exponentiation, which Nelson rejects, is interpretable in Q + Con(Q), and many might take this to show that Nelson's position is untenable. But, of course, Nelson is aware of this theorem, and he responds (in personal correspondence) that the consistency of Q is one thing, and the consistency of the arithmeticized theory of Q, which "Con(Q)" expresses, is another.

could disagree over a mathematical hypothesis, H, is that there actually has been intellectually virtuous and conceptually competent disagreement over H, it seems that flawless agents could disagree over a great deal of mathematics indeed.

The Continuum Hypothesis and Typical Axioms

Nevertheless, one is apt to balk at the move from the conclusion that CH may be absolutely undecidable in the present sense to the conclusion that typical axioms may be. I can think of three ways that one might challenge this move. Let me consider them in turn.

First, one might contend that those who reject typical axioms in mathematics, such as Potter, Azcel, Weyl, or Nelson, are conceptually incompetent, irrational, insincere, inattentive, uninformed, or unimaginative in a way that those who reject CH or its negation, such as Devlin, Woodin, or Foreman, are not. Of course, if one assumes that, contra Potter, Azcel, Weyl, or Nelson, the evidence and arguments for standard axioms, as opposed to the evidence and arguments for CH or its negation, are compelling then it trivially follows Potter, Azcel, Weyl, and Nelson each exhibit a cognitive failing that Devlin, Woodin, and Foreman do not. For example, given that the standard evidence and arguments for Choice – that it is fundamental to proofs of "standard" theorems in many branches of mathematics, that it imposes order on the theory of the transfinite, that the arguments against it, such as the argument from the Banach-Tarski "paradox", can be explained away, and so on – are compelling, then one can charge Potter, who is doubtful of Choice, with a failure to recognize the objective superiority of Choice's merits over the merits of alternatives to Choice. But a cognitive failing that can only be specified given the objective superiority of the hypothesis at issue is not the sort that is relevant

to the question of whether cognitively flawless agents could disagree over that hypothesis.

Again, agents are cognitively flawless with respect to a hypothesis, H, just in case they are

cognitively virtuous in a way that can be specified *independent* of the status of H – i.e. just in

case they are logically omniscient, perfectly sincere, attentive, conceptually competent, etc.

Perhaps the relative *unpopularity* of Potter's, Azcel's, Weyl's, or Nelson's views constitutes

reason to think that they exhibit a cognitive failing that the likes of Devlin, Woodin, or Foreman

do not? It would obviously be blatantly question-begging to assume so. It is not as if Nelson,

say, is unaware that he is in the minority in rejecting Induction. But *he* does not take that to

constitute compelling reason to change his views. Indeed, it is doubtful that he should. Nelson

is among the most gifted mathematicians alive. It is hard to see why he should be any more

rationally compelled to change his mathematical views on the mere grounds that they are

unpopular than Einstein was to change his physical views on the mere grounds that they were.

Indeed, there is an irony about the present understanding of absolute undecidability. It is that, if

anything, it is *easier* to argue that disagreement over CH results from an independently-

specifiable cognitive shortcoming than it is to argue that disagreement over "typical axioms",

like Choice, Foundation, Least Upper Bound, or Induction does. The reason is that it is at least

conceivable that all disagreement over CH could one day be shown to result from *logical*

mistakes. As was mentioned earlier, various views with respect to CH depend on conjectures

that are outstanding. For example, Woodin's views depend on the Strong Omega-Conjecture.

Similarly, Foreman's views depend on the conjecture that Generic Large Cardinals (GLC) are

consistent. Devlin's views, according to which V = L is plausible, do not seem to depend on any

outstanding conjectures. But it is at least *conceivable* that, say, the Strong Omega-Conjecture will be *disproved*, Foreman's GLC will be proved *inconsistent*, and all apparently reasonable, sincere, attentive, etc. mathematicians will converge on V = L (that would be quite a development!). By contrast, theorems relevant to Choice, Foundation, Least Upper Bound, and Induction, seem largely to be in. Short of a proof that such axioms are inconsistent (which seems very unlikely), it is hard to imagine how more *logic* might suffice to show that all disagreement over these axioms is the product of independently-specifiable cognitive shortcomings.

I suspect that the case for the absolute undecidability of CH appears to be much stronger than the case for the absolute undecidability of, say, LUB because apparently reasonable, sincere, attentive, etc. disagreement over CH is much more *salient* than such disagreement over, say, LUB. *For practical purposes*, the mathematical community has decided to *use* LUB, whether or not the case for its determinate truth is compelling. By contrast, the mathematical community has made no such decision regarding CH or its negation. Disagreement that is salient is *disagreement is practice*, and there is negligible disagreement in practice over the likes of LUB.

The second way that I can imagine challenging the move from the conclusion that CH is absolutely undecidable to the conclusion that typical axioms are is by claiming that disagreement over typical axioms is *merely verbal* in a way that disagreement over CH is not. Consider Nelson's dispute with advocates of Induction, for example. One might argue that Nelson is just working with a different *concept* of natural number in rejecting this axiom. He is arguing, in effect, that *relative to a radical predicativist conception of natural number*, this axiom fails to hold. But this is, of course, consistent with the view that *relative to a classical conception of*

natural number this axiom does hold. This, it might be thought, is all that the advocate of Induction needs to maintain. So, there is no real dispute between Nelson and such an advocate. But there is no corresponding sense in which Woodin's and Forman's dispute, say, can be dismissed as merely verbal. They are arguing about whether *the* concept of set vindicates the Continuum Hypothesis.

There are two problems with this line of thought. The first is that it offers no principled reason to regard apparent disagreement over CH differently from apparent disagreement over the likes of Induction. Why *not* say that Woodin and Forman are really just working with different concepts of set – concepts which "coincide" up through large cardinals but "divorce" at the level of CH? Perhaps we can allow that Woodin and Foreman have a genuine *normative* disagreement – a disagreement about which concept is best to invoke for various purposes. However, given that we refuse to recognize apparent disagreement over the likes of Induction as genuine, on what grounds can we regard apparent disagreement over CH this way?

The more important problem with the present proposal, however, is that it relies on the false assumption that all that is being disputed in any given case is *what "follows" from a given concept*. That is evidently *not* all that is being disputed in the relevant cases. Edward Nelson does not think that the axiom of Induction is true of *any* mathematical objects. He does not believe, that is, that in addition to the *radically predicative natural numbers* there are *also* the *classical natural numbers*. He believes that there are just the radically predicate natural numbers. (Or, to put the point in terms of truth, he does not believe that both his axioms and his opposition's axioms are true – even true of distinct domains of mathematical entities.) Similarly,

advocates of the Axiom of Choice do not believe that in addition to the well-orderable sets, there

is *also* a class of things that are just likes sets except that some of them are well-orderable and

others are not. *They believe that there are only sets, all of which are well-orderable*. If it were

allowed that corresponding to any "consistent" mathematical concept there was a universe of

mathematical entities that satisfied the theory "following" from it, then there would *obviously* be

no substantive issue as to the truth-value of CH. CH would be true in that universe of

mathematical entities satisfying Foreman's GLC, and false in that universe of mathematical

entities satisfying Woodin's constraints. The question of whether CH is "really" true would be

like the question of whether a bank is "really" a monetary institution. In order for the debate

over the determinateness of CH to have interest it must be granted -- as those with positive views

on the issue, from Godel through Woodin, have always maintained -- that there is a "single"

universe of mathematical entities which either does or does not satisfy CH.[29]

The final way that I can imagine challenging the move from the conclusion that CH is absolutely

undecidable to the conclusion that typical axioms of mathematics are is by contending that

disagreement over typical axioms, as opposed to disagreement over CH, is *philosophical*. It

might be thought that there could be cognitively flawless *philosophical* disagreement over *any*

hypothesis whatever – even over such banalities as the hypothesis that if there are dogs, then

there are dogs. After all, there can arguably be cognitively flawless disagreement over *nihilism*

(error-theory) with respect to any area, from ontology, to ethics, to logic, and such disagreement

---

[29] Of course, one could still argue about what was "packed into" a given concept of set. But under the assumption
that corresponding to any "consistent" mathematical concept is a mathematical universe satisfying the theory
"following" from it, such an argument would really just be about us and would appear to lack much mathematical
interest. It would really just be about whether our concept characterized this part or that of the mathematical
universe, as opposed to being about what the mathematical universe contains. For one development of such a
radically pluralist view -- which openly embraces these deflationary consequences – see Balaguer[2001].

straightforwardly translates into disagreement over "first-order" claims from the relevant area. One might think that, if anything is relevant to the determinacy of a mathematical axiom, H, it is whether there can be cognitively flawless *non-philosophical* disagreement over H.

I cannot here refute the suggestion that there could be cognitively flawless philosophical disagreement over any hypothesis -- though there are a wealth of differences between axioms of mathematics and, say, trivialities of ontology or logic that should make one suspicious of this suggestion. However, the thought that there is a relevant *difference* between disagreement over Induction and disagreement over CH is cannot, I believe, be sustained.

Compare, for example, Edward Nelson's argument against Induction to Hugh Woodin's argument against CH. Nelson, it is true, argues from intuitively philosophical conclusions regarding the nature of certain mathematical objects – natural numbers – to the negation of Induction. In particular, he argues that natural numbers must be thought to, in a certain sense, depend on our axioms that characterize them, and that impredicative axioms, such as Induction, are dubiously coherent in light of this.[30] However, Hugh Woodin's argument against CH is no different in this respect. That argument proceeds from a conclusion regarding the nature of sets – namely, that sets are restricted to those that are not "blurred" via forcing.[31] No straightforwardly mathematical theorem could establish that. Woodin, like Nelson is taking an intuitively philosophical stand on the nature of the mathematical universe, and that stand leads him to substantive "first-order" mathematical conclusions.

---

[30] See Nelson [1986] or Buss [1996].
[31] See Dehornoy [2003], p. 4, fn. 18.

Of course, Nelson's argument is in a certain sense more *radical* than Woodin's. Many fewer with views on relevant issues would be willing to take seriously the idea we should reject impredicative axioms, even in arithmetic, than would be willing to take seriously the idea that we should want an Omega-complete theory of H(w2). But that does not show that Nelson's argument is different in kind from Woodin's. Both arguments rest ultimately on intuitively philosophical premises that cannot be established via standard mathematical methodology. In this respect Nelson's and Woodin's arguments are both part of the long tradition in mathematics – of arguments for the introduction of negative and imaginary numbers, arguments for the generalization of the notion of function to arbitrary ordered pairs, and of arguments for the view that the set-theoretic universe is (or is not) a cumulative hierarchy. When basic principles are at issue in mathematics, it invariably turns philosophical, at least in the minimal sense in which Nelson's argument against Induction is philosophical. This is true of CH in particular.

Conclusions

I have surveyed a wide range on possible understandings of absolute undecidability. I have argued that under no such understanding could one hope to establish (a) CH is absolutely undecidable, (b) typical axioms are not absolutely undecidable, and (c) if a mathematical hypothesis is absolutely undecidable, then it is indeterminate. However, I have identified one understanding of absolute undecidability on which it could arguably be established both that (a) CH is absolutely undecidable and that (c) if a mathematical hypothesis is absolutely undecidable, then it is indeterminate. This leaves the advocate of the common argument from the absolute undecidability of CH to its indeterminacy with two very different options.

The first is to embrace the most thorough-going form of what Shapiro calls "truth-value realism", according to which every (well-formed) sentence at all in the language of mathematics is determinately true or false.[32] It follows from the above that this view is committed to the surprising claim that flawless agents could be almost completely wrong with respect to the mathematical truths.

The other option is to embrace a new kind of truth-value antirealism, according to which vanishingly few sentences of interest in the language of mathematics are determinate. This new kind of antirealism would not depend on the view that mathematics is about a peculiar realm of objects. Hence, it would not be motivated with reference to familiar ontological objections to mathematical realism. Nor would it, at least obviously, depend on the view that we would have no "epistemic access" to mathematical truths, if there were any. Hence, it would not be motivated with reference to familiar epistemological objections to mathematical realism either. It would merely depend on the view that the mathematical truth would be significantly objective – i.e. that, say, arithmetic with the Successor Axiom is better with respect to truth than its alternatives. And it would be motivated with reference to objections that are familiar from other areas of philosophy, such as metaethics. According to such objections, the fact that there could be cognitively flawless disagreement over hypotheses from an intuitively *a priori* area calls into question the view that such hypotheses are determinate. I suspect that most advocates of the argument from the absolute undecidability of CH to its indeterminacy will pursue the first option. However, I suggest that the second is also worthy of serious consideration.

---

[32] See Shapiro [2000].

BIBLIOGRAPHY

Arrigoni, Tatiana. [2011] "V=L and Intuitive Plausibility in Set Theory. A Case Study." *Bulletin of Symbolic Logic*. Vol. 17. 337 – 359.

Azcel, Peter. [1988] *Non-Well-Founded Sets*. SLI Lecture Notes. Vol. 14. Stanford, CA: Stanford University, Center for the Study of Language and Information.

Balaguer, Mark. [2001] *Platonism and Anti-Platonism in Mathematics*. New York: Oxford University Press.

Bell, John and Geoffrey Hellman. [2006] "Pluralism and the Foundations of Mathematics." in Kellert and Waters.

Benacerraf, Paul and Hilary Putnam. [1983] *Philosophy of Mathematics: Selected Readings* (Second Edition). Cambridge: Cambridge University Press.

Boolos, George. [1999] "Must We Believe in Set Theory?" in Sher and Tieszen.

Browder, Felix (ed.) [1976] *Mathematical Developments Arising from Hilbert Problems* (Proceedings of Symposia in Pure Mathematics. Vol. 28). Providence, Rhode Island: American Mathematical Society.

Buss, Samuel. [1996] "Nelson's Work on Logic and Foundations and Other Reflections on Foundations of Mathematics." in Farris.

Dehornoy, Patrick [2003] "Recent Progress on the Continuum Hypothesis (After Woodin)." Available online at: http://www.math.unicaen.fr/~dehornoy/Surveys/DgtUS.pdf

Devlin, Keith. [1973] *Aspects of Constructibility*. (Lecture Notes in Mathematics, Vol. 354). Berlin: Springer-Verlag,

-----. [1977] *The Axiom of Constructibility: A Guide for the Mathematician* (Lecture Notes in Mathematics, Vol. 617). Berlin: Springer-Verlag,

-----. [1984] *Constructibility*. Berlin: Springer-Verlag.

Dummett, Michael. [1991] *The Logical Basis of Metaphysics*. Cambridge: Harvard University Press.

Farris, William (ed.) [2006] *Diffusion, Quantum Theory, and Radically Elementary Mathematics*. Princeton: Princeton University Press.

Feferman, Solomon. [2000] "Does Mathematics Need New Axioms?" *Bulletin of Symbolic Logic*. Vol. 6. 401 - 446.

Field, Hartry. [1980] *Science Without Numbers*. Princeton: Princeton University Press.

-----. [1989] *Realism, Mathematics, and Modality*. Oxford: Blackwell.

-----. [1998] "Which Undecidable Mathematical Sentences have Determinate Truth-Values?" in Laurence and Macdonald.

Foreman, Mathew. [1998] "Generic Large Cardinals: New Axioms for Mathematics?" *Proceedings of the International Congress of Mathematicians, Vol. II*. Berlin.

Forster, Thomas. [2008] "The Iterative Conception of Set." *Review of Symbolic Logic.* Vol. 1. 97 – 110.

-----. [Forthcoming] The Axioms of Set Theory. Cambridge University Press. Draft available online at: < http://www.dpmms.cam.ac.uk/~tf/axiomsofsettheory.pdf>

Fraenkel, Abraham, Yehoshua Bar-Hillel, and Azriel Levy. [1973] *Foundations of Set Theory* (Studies in Logic and the Foundations of Mathematics, Volume 67). New York: Elsevier Science Publishers.

Friedman, Harvey. [2002] *Philosophical Problems in Logic* (Lecture Notes). Princeton University.

Godel, Kurt. [1951] "Some Basic Theorems on the Foundations of Mathematics and their Philosophical Implications." in Rodriguez-Consuegra.

-----. [1964] "What is Cantor's Continuum Problem?" (Revised Version) in Benacerraf and Putnam. First appeared as [1947] *American Mathematical Monthly*. Vol. 54. 515 -- 525.

Jensen, Ronald. [1995] "Inner Models and Large Cardinals." *Bulletin of Symbolic Logic*. Vol. 1. 393 -- 407.

Kellert, Stephen, Helen Longino, and C. Kenneth Waters (eds.) [2006] *Scientific Pluralism: Minnesota Studies in the Philosophy of Science, XIX*. Minneapolis, MN: University of Minnesota Press.

Kilmister, Clive W. [1980] "Zeno, Aristotle, Weyl and Shuard: Two-and-a-Half Millenia of Worries over Number." *Mathematical Gazette*. Vol. 64. 149 – 158.

Kino, Akino, John Myhill and Richard Vesley (eds.) [1970] *Intuitionism and Proof Theory*. Amsterdam and London: North-Holland Publishing.

Koellner, Peter. [2009] "On the Question of Absolute Undecidability" (Revised Version) in Parsons, Feferman, and Simpson. First appeared as [2006] *Philosophia Mathematica*. Vol. 14. 153 – 188.

Kreisel, George. [1967] "Observations on Popular Discussions of the Foundations of Mathematics." in Scott.

Kracht,Marcus, Maarten de Rijke, HeinrichWansing, and Michael Zakharyaschev (eds.) [1998]. Advances in Modal Logic. Vol. 1. (Berlin, 1996). Volume 87 of CSLI Lecture Notes. 307–359. Stanford, CA: CSLI.

Laurence, Stephen and Cynthia Macdonald. [1998] *Contemporary Readings in the Foundations of Metaphysics*. Oxford: Basil Blackwell.

Maddy, Penelope. [2011] *Defending the Axioms*. Oxford: Oxford University Press.

Martin, Donald. [1976] "Hilbert's First Problem: The Continuum Hypothesis." in Browder.

Nelson, Edward. [1986] *Predicative Arithmetic* (Mathematical Notes. No. 32). Princeton, NJ: Princeton University Press.

Parsons, Charles, Solomon Feferman, and Stephen G. Simpson. [2009] *Kurt Gödel: Essays for his Centennial* (Lecture Notes in Logic. Vol. 33). Association of Symbolic Logic.

Potter, Michael. [2004] *Set Theory and Its Philosophy: A Critical Introduction*. Oxford: Oxford University Press.

Quine, W.V.O.. [1937] "New Foundations for Mathematical Logic." *American Mathematical Monthly*. Vol. 44. 70 -- 80.

-----. [1969] *Set Theory and Its Logic*. (Revised Edition) Cambridge: Harvard University Press.

Rodriguez-Consuegra, Francisco. [1995] *Kurt Gödel: Unpublished Philosophical Essays*. Berlin: Birkhauser Verlag.

Schiffer, Stephen. [2002] "Moral Realism and Indeterminacy." in Sosa and Villanueva.

Schroeter, Laura, and Francois Schroeter. [2012] "Do Moral Realists Need Convergence?" Manuscript.

Scott, Dana. (ed.) [1967] *Axiomatic Set Theory* (Proceedings of Symposia in Pure Mathematics, V. XIII, Part I). Providence, RI: American Mathematical Association.

Shapiro, Stewart. [2000] *Philosophy of Mathematics: Structure and Ontology*. Oxford: Oxford University Press.

Sher, Gila. and Richard. Tieszen. [1999] *Between Logic and Intuition: Essays in Honor of Charles Parsons*. Cambridge: Cambridge University Press.

Sosa, Ernest and Enrique Villanueva (eds.) [2002] *Realism and Relativism* (Philosophical Issues, Vol. 12). Oxford: Blackwell.

Visser, Albert. [1998] "An Overview of Interpretability Logic" in Kracht,Marcus, Maarten de Rijke, Wansing, and Zakharyaschev.

Advances in Modal Logic. Vol. 1. (Berlin, 1996). Volume 87 of CSLI Lecture Notes. 307–359. Stanford, CA: CSLI..

Waters, Kenneth, Helen Longino, and Stephen Kellert (eds.) [2006] *Scientific Pluralism* (Minnesota Studies in Philosophy of Science, Volume 19). Minneapolis: University of Minnesota Press.

Williamson, Timothy. [2004] *Vagueness*. New York: Routledge.

Wolchover, Natalie. [2013] "To Settle Infinity Dispute, a New Law of Logic." *Quanta Magazine*. November 26. Available online at:

<https://www.simonsfoundation.org/quanta/20131126-to-settle-infinity-question-a-new-law-of-logic/>

Woodin, Hugh. [2001a] "The Continuum Hypothesis I." *Notices of the American Mathematical Society*. Vol. 48. 567 – 576.

-----. [2001b] "The Continuum Hypothesis II." *Notices of the American Mathematical Society*. Vol. 48. 681 – 690.

Wright, Crispin. [1994] *Truth and Objectivity*. Cambridge: Harvard University Press.

Yessenim-Volpin, A.S. [1970] "The Ultra-intuitionistic Criticism and the Antitraditional Program for Foundations of Mathematics", in *Intuitionism and Proof Theory* (Proc. Conf., Buffalo, NY, 1968), North-Holland, Amsterdam, 1970, pp. 3 – 45.