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Who Proved the Independence of the Continuum Hypothesis?

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Abstract: Paul Cohen is routinely represented as having proved the independence of the Continuum Hypothesis from the axioms of Zermelo-Fraenkel set theory — despite the equally uncontroversial, and apparently contradictory, concession that Cohen proved only one of the two conditions on independence, Kurt Gödel having proved the other. In this essay we explore, and argue for a position on, this strange and unsatisfactory situation, and suggest that our position generalizes in ways that would upset the current conventions governing the assignment of credit for intellectual discoveries.

§ 1. — Introduction[1].

Disputes about who discovered a given result — in mathematics, in physics, in philosophy, and in most other fields — are common; and often, further investigation, far from resolving the dispute, seems to make it more intractable. In order to resolve the dispute, we must sometimes think afresh about the concepts which frame the very notion of credit. An admirable example of the latter strategy is Michael Harris' recent paper about the Taniyama-Shimura-Weil Conjecture, and the priority dispute attaching to that Conjecture.[2] Harris reviews the facts, and then suggests — convincingly, in our view — that a different virtue was expressed by the accomplishments, vis à vis that Conjecture, of the three mathematicians who have been credited with its development. By Harris' lights, the priority dispute dissolves into the question of (as he puts it) "the relative importance one is willing to assign to each of these virtues".

How about the independence of the Continuum Hypothesis? Who proved that? By contrast with the Taniyama-Shimura-Weil Conjecture, the answer, surely, is obvious. No controversy here. Everyone knows it was Paul Cohen who proved independence. But did he really?

Strangely enough, this is, in a sense, neither a historical nor a mathematical question. The historical and mathematical facts are — at one level of description, anyway — a matter of universal consensus. Here are those facts:

1. In 1939 Kurt Gödel proved that the Continuum Hypothesis (CH) is consistent with the Zermelo-Fraenkel axioms for set theory with the axiom of choice (ZFC) — in other words, that CH cannot be disproved from ZFC.[3]

(1) For the record, the origin of the present paper is a dispute that took place years ago between one of the authors, Palle Yourgrau, and his colleague, Robert Tragesser. In spite of his best efforts, Yourgrau was unable to convince his colleague of his thesis that in spite of common practice, it is a mistake to attribute to Paul Cohen proof of the independence of Cantor's Continuum Hypothesis. It is our hope that the discussion that follows will prove more successful.

(2) Harris 2023.

(3) The Continuum Hypothesis is the thesis that the cardinality of the continuum is the smallest uncountable cardinal number — equivalently, that there is no set whose cardinality is strictly between that of the integers and the real numbers. (Gödel's and Cohen's proofs assume, of course, that ZF is consistent.)

2. In 1963 Paul Cohen proved that the negation of CH is consistent with ZFC — in other words, that CH cannot be proved from ZFC.
3. Gödel's result does not, by itself, entail the logical independence of CH from ZFC.
4. Cohen's result does not, by itself, entail the logical independence of CH from ZFC.
5. The conjunction of Gödel's and Cohen's results does entail the independence of CH from ZFC.

In a Wittgensteinian mood we might be tempted to say: if no one disputes the facts, then what question remains to be answered? ('Since everything lies open to view there is nothing to explain.') But we shouldn't let Wittgenstein steal our problems from us. And there is a problem here, one which opens onto a large and puzzling field of questions. In this essay we will address the case of Cohen and Gödel, but also use this case as an opportunity to explore that larger field of questions.

§ 2. — A Peculiar Situation, and a Dismissive Solution.

The problem begins with the fact mentioned at the outset: everyone knows, or at least believes, that Cohen proved the independence of CH from ZFC. This claim appears in virtually every discussion of the continuum hypothesis and its history.(4) Cohen himself makes it:

(4) A few representative examples: '...[Cohen's] proof of the independence of the continuum hypothesis from the other axioms of set theory...', Encyclopedia Britannica 2021; 'This independence was proved in 1963 by Paul Cohen...' ('Continuum Hypothesis', Wikipedia); 'Paul Cohen is of course best known in mathematics for his Fields Medal-winning proof of the undecidability of the continuum hypothesis within the standard Zermelo-Fraenkel-Choice axioms of set theory...', Fields Medal winner Terence Tao, 2007; 'Cohen's task, then, was to show that the continuum hypothesis was independent of ZFC (or not)', Mastin 2020; 'He is best known for his proofs that the continuum hypothesis and the axiom of choice are independent from Zermelo-Fraenkel set theory' ('Paul Cohen', Wikipedia); 'Cohen proved that the continuum hypothesis was independent of standard set theory...the work of Paul J. Cohen in the 1960s demonstrated that the question was not settled by the standard axioms' ('Paul Joseph Cohen', 'The Independence of the Continuum Hypothesis', Encyclopedia.com); '[Cohen] astounded the mathematical world by proving the independence of the Continuum Hypothesis and the Axiom of Choice from the system of Zermelo-Fraenkel', Hersh 2011; 'By 1963 [Cohen] had produced his proof of the independence of the continuum hypothesis', Sarnak 2007; '[Forcing] was invented in 1963 by Paul Cohen, who used it to prove the independence of the Continuum Hypothesis' Jech 2008.

'Now in 1962 I began to think about proving independence.'[5] But relative to facts 1-5 — facts which are, to repeat, universally recognized and uncontested — this claim sounds absurd. To prove a claim is, presumably, to offer a sound argument for it; and everyone — not least, Cohen himself — knows that Cohen didn't do this. He didn't offer an argument, sound or unsound, for independence. Independence, on any account, consists in the truth of a *conjunction* of claims — viz., the claims mentioned in 1 & 2, above — and Cohen did not prove the truth of that conjunction. Rather, he proved *one* of the conjuncts. Gödel supplied the proof of the other. So why is it so often said that Cohen proved independence? What tempts people to say this? What exactly is going on here? The point can be put this way. Suppose I tell my students the following: 'Gödel proved you can't disprove CH from ZFC. Cohen proved you can't prove CH from ZFC.' Is that not literally true? Have I left out anything in my account of the situation? Should I say: 'Oh, I forgot to add a third thing — Cohen proved independence?'

The most obvious and natural answer to what's going on is: nothing. Perhaps one should dismiss talk of Cohen as having proved independence as nothing more than a case of sloppy, inexact talk. On this interpretation of the situation, nobody would deny that, strictly speaking, Cohen didn't prove independence, and so the apparent contradiction is only that — an appearance. But there are compelling reasons to reject this answer. As we have already noted, the claim that Cohen proved independence isn't a stray remark, something one finds only occasionally, in hurried or informal presentations of the history. On the contrary, it is ubiquitous, and appears in the most rigorous statements of his work (including Cohen's own, 'for the record' statement[6]). Here for example is how the Fields Medal Committee, in its official citation — presumably, a context in which scrupulous accuracy was the order of the day — described what Cohen did: '[Cohen] used a technique called 'forcing' to prove the independence in set theory of the axiom of choice and of the generalized continuum hypothesis.'[7] Do we want to say that *the Fields Committee* was being sloppy

(5) Cohen 2002, p. 1088. Cf. these remarks by Cohen: 'It was my great fortune and privilege to be the person who fulfilled the expectations of Gödel in showing that the Continuum Hypothesis (CH), as well as other questions in Set Theory, are independent of the usual set-theory axioms' (Cohen 2008, x); 'The main objective was to give the proof of the independence of the continuum hypothesis.' (Cohen ibid, xxii)

(6) Cohen 2002.

(7) International Mathematical Union 1966.

and inexact? More generally, it is hard to see how 'Cohen proved independence' is a sloppy way of saying that Cohen did *not* prove independence. No talk is *that* loose.

Unsurprisingly, a legendarily unsloppy logician, Alonzo Church, in his formal introduction to the presentation of the Fields Medal to Cohen, stated that 'the first half of the solution to the continuum problem, on which subsequent work heavily depends, was taken by Kurt Gödel in 1938–40.' And yet, even as precise a thinker as Church went on to say not that the second half of the solution is of course proving the *consistency of the negation* of CH, but rather, '... the second half of the negative solution is of course *independence*.'(8) And, once again, this was not simply a rare oversight by Church, who went on to say that '... the independence of the continuum hypothesis ... remained for Paul Cohen in 1963-64.'(9) Similarly, Martin Davis says that 'Cohen used forcing to show that neither Gödel's A nor CH could be proved from the Zermelo-Fraenkel axioms. Combined with what Gödel had shown using his constructible sets, this proved that both of these propositions are *independent* of the Zermelo axioms.'(10) Yet Davis immediately goes on to say the following: 'The independence of CH leads to a difficult philosophical quandary. Is there still a fact of the matter to be resolved regarding CH, or has *Cohen* done all that can be done with the question?'(11)

Indeed, occasionally mathematicians, beyond crediting Cohen with having proved independence, *explicitly* credit Cohen with having proved *both* of the conditions that jointly constitute independence. Thus mathematician Shai Ben-David states in a lecture (on Gödel, no less) that 'Paul Cohen proved in 1960 that we can neither prove [the CH] nor disprove it.'(12) That statement, so to speak, wears its falsity on its face. It *means* the same thing as simply saying that Paul Cohen proved the independence of CH, but it renders obvious what the other formulation disguises: that one shouldn't be saying that it was Cohen (as opposed to Gödel) who proved that you can't disprove the CH from ZFC.

Few mathematicians are as blatant as Ben-David, but, as we've seen, even Church is guilty of misstatement. By contrast, the

(8) Church 1968.
(9) Church ibid.
(10) Davis 2008, viii, emphasis in the original.
(11) Davis ibid, emphasis added.
(12) Ben-David 2015.

logician Raymond Smullyan stated things more precisely and accurately than Church (how often can such a claim be made about Alonzo Church?) and Davis. According to Smullyan: 'In 1938 Kurt Gödel proved the famous result that the generalized hypothesis is formally consistent with the axioms of ZFS [Zermelo-Fraenkel-Skolem] ... And in 1963, Paul Cohen settled the matter in the other direction; he showed that the negation of the generalized continuum hypothesis ... is consistent with ZFS. Thus, the continuum hypothesis is independent of the axioms of ZFC.'(13)

(13)Smullyan 1967, p. 208. We have just seen that someone might try to put this whole bizarre situation down to mere sloppy talk. In this connection it has been suggested to us that the word 'independence' is used in two ways in mathematical logic: in the *broader* sense, a proposition p is independent of a theory T if p can be neither proved nor disproved from T, while in in the *narrower* sense, a proposition p is independent of a theory T simply if p cannot be proved from T. But even if this claimed ambiguity exists (and other discussants have denied that it does), the problem is that in order for the ambiguity to be relevant, it would have to be true that when people credit Cohen with the discovery of independence, they are using the word 'independence' in the *narrower* sense. It's abundantly clear, however, from the many quotations offered earlier, that this is not the case.

For example, Davis (as we saw) says that the independence result in question was achieved only when "*combined* with what Gödel had shown using his constructible sets." Clearly, in the *narrower* sense of 'independence' this would be false: Gödel's work was completely unnecessary for proving independence in that narrower sense. (The problem, to repeat, is that Davis immediately goes on to wonder whether *Cohen* — not Gödel and Cohen together, but just Cohen — "has done all that can be done with the question".) Similarly, Terence Tao uses the older term 'undecidable' to characterize Cohen's achievement — which, unlike 'independence', is *not* ambiguous, but always means independence in the broader sense: "Paul Cohen is of course best known in mathematics for his Fields Medal-winning proof of the *undecidability* of the continuum hypothesis within the standard Zermelo-Fraenkel-Choice axioms of set theory..."

What recognizing the two uses of 'independence' might help to explain — assuming that these two uses do indeed exist — is why the question of what exactly Cohen's achievement was has been, for want of a better term, so "muddied." Indeed, Cohen himself has arguably contributed to muddying these waters. In *Set Theory and the Continuum Hypothesis*, for example, he writes: "Our main object in this chapter [Chapter II, "the *Independence* of the Continuum Hypothesis and the Axiom of Choice"] is to prove that CH cannot be proved from ZF (with AC included), and that AC cannot be proved from ZF. *Together* with the results of Chapter III ["The Consistency of the Continuum Hypothesis and the Axiom of Choice"] this will give a complete proof of the *independence* of CH and AC. ... In attacking the *independence* of CH, the first question is to decide whether to search for standard or non-standard models ..." (pp. 107 - 108: emphasis and brackets added). Here, in a single passage, Cohen himself can be plausibly read as going back and forth between the narrow and the broad sense of 'independence'. No wonder the waters are muddy.

§ 3. — Mathematical Priority and Temporal Priority.

In the previous section we considered, and rejected, the idea that the common practice of crediting Cohen with proving independence can be understood as mere loose talk. In this section we address two other theories. Here is the first theory: while both proofs — Gödel's and Cohen's — were, admittedly, logically necessary conditions on establishing independence, nevertheless Cohen's work was, from a substantive point of view, much more important than Gödel's. Here we could cash out importance in various ways: the difficulty of the mathematics, the significance on their own terms (and outside the context of CH's relation to ZFC) of the methods or results, etc.

Before we get to the truth of this idea, we can ask whether it would validate the claim at issue even if it were true. It is widely believed that Bertrand Russell did the lion's share of the work on *Principia Mathematica*. Suppose that this is true. Does it follow that Russell could rightly claim to have written that book? Not at all. It was co-authored with Whitehead, even if Russell wrote 90% of it, and if Russell, coming up for tenure, put it on his publications list without mentioning Whitehead, this would be correctly regarded as actionable dishonesty. (Russell, for his part, in fact made various representations of Whitehead's contributions — from 'Dr. Whitehead had an equal share in the work'(14) to 'he [Russell] did all the dirty work, since Whitehead was a hard-working lecturer'(15), and everything in between.(16))

Needless to say, the very issue of how to assess the significance of one person's contribution to a larger project is an exceedingly thorny one. But at a minimum, we can say that it cannot be reduced to quantitative facts about the length of time each person devoted to the project, how much weight they lifted, etc. Wittgenstein, when he worked on the design of his sister's mansion in Vienna, 'only'

(14) Russell 1922, p. 124.

(15) Littlewood 186, p. 128.

(16) For a detailed exploration of this issue, see Landini 2016. Here is another description by Russell: 'As for the mathematical problems, Whitehead invented most of the notation, except in so far as it was taken over from Peano; I did most of the work concerned with series and Whitehead did most of the rest. But this only applies to first drafts. Every part was done three times over. When one of us had produced a first draft, he would send it to the other, who would usually modify it considerably. After which, the one who had made the first draft would put it into final form. There is hardly a line in all the three volumes which is not a joint product' (Russell 1959, p. 74).

contributed the radiators, doors, and windows; but 'this is not so marginal as it may at first appear, for it is precisely these details that lend what is otherwise a rather plain, even ugly house its distinctive beauty...the details are thus everything...'[17]

But the fact is that we cannot ground the routine description of Cohen as the prover of independence in the significance of his contribution relative to Gödel's. Even by the lights of those who say that Cohen proved independence, Gödel's proof that CH is consistent with ZFC is a very important and difficult piece of mathematical work, one comparable, arguably, in mathematical stature with Cohen's proof — as Cohen himself said: "... I felt that [Gödel and I] shared a great bond in that we had successfully discovered, each in his own way, new fundamental methods in Set Theory."[18] No one thinks that Gödel's contribution is *merely* logically necessary for independence — like a single trivial lemma worked out by a graduate student in a three-hundred-page manuscript. Rather, each mathematician's contribution was a major achievement, and recognized as such by both of them. As we just indicated, Cohen fully recognized the significance of (as he put it) Gödel's "momentous discovery of the *Constructible Universe*", and talked about this achievement and its important influence on him at length.[19]

Before we render our own verdict on the Gödel-Cohen problem, let us consider one more candidate explanation of the credit commonly accorded Cohen. Perhaps it's said that Cohen proved independence because Cohen *completed* the structure which, taken as a whole, constitutes a proof of independence. This claim, of course, is certainly true. The question is whether it is relevant to the attribution of credit for proving independence. While it is certainly true that independence wasn't proved *until* Cohen developed his proof, this is not because *what Cohen proved* was independence.[20]

[17] Monk 1990, p. 236.

[18] Cohen 2008, xii.

[19] Cohen ibid, x.

[20] In this connection it is worth noting Gödel's description of the situation in his postscript to Gödel 1947: "... the question of whether Cantor's continuum hypothesis is decidable from the von Neumann-Bernays axioms of set theory (the axiom of choice included) was settled in the negative by Paul J. Cohen." Note that Gödel does not say "Cohen proved independence". What he says is that the question of independence was "settled" by Cohen. What exactly does that mean? "Settle" is not, in this context, a word with a precise meaning, but the stress is generally on the conclusion of a process: "when did they settle their disagreement?" means "when did their disagreement come to an end?" Presumably, then, Gödel means

Putting the final piece of a puzzle into place does not magically turn you into the sole person who solved the puzzle, if others have contributed to its solution. It doesn't even make you the most important contributor. This is true even if the others tried and failed to complete the puzzle.[(21)] Once again, the temporal order of the events may *explain* why the credit is so often given to Cohen, but it is hard to see how it *justifies* this practice. For one thing, this proposal would force us to say, implausibly, that had Gödel decided in 1939 to put his proof in a drawer and then published it in 1965, he would have proved independence!

To emphasize the point further, consider the following hypothetical scenario. Gödel and Cohen decide to team up to prove the independence of the continuum hypothesis. It is agreed that Gödel will attempt to prove the consistency of CH with ZFC, while Cohen will attempt to prove the consistency of the negation of CH with ZFC. In the event, Gödel completes his proof first. Six months later, Cohen completes his proof. They're delighted, and celebrate with a glass of wine at a local restaurant. "We did it!" they will say. What Cohen will obviously *not* say is: "I did it! I succeeded in proving the independence of the continuum hypothesis." Nor would anyone else say this. But should this hypothetical scenario be dismissed as irrelevant fiction, given what actually happened in the case of Gödel's and Cohen's proofs? We deny its irrelevance. We believe it draws attention to the question at issue, namely, what the relevance is of the temporal order of proofs of P and of Q, when the final goal is a proof of the conjunction, P & Q.[(22)] What is irrelevant to the *logic* of this situation, we believe, is the question of *time*.

that once Cohen developed his proof, the question of independence was settled in the sense: over and done with. And that claim — unlike the ubiquitous claim that Cohen proved independence — is true.

[(21)] 'In a letter to Menger, December 15, 1937, we learn that Gödel was working on the independence of CH, but, as he wrote, "... [I] don't know yet whether I will succeed with it." It seems that from 1941 to 1946 he devoted himself to attempts to prove the independence.' Cohen 2002, p. 1087.

[(22)] Indeed, as we point out later, Richard Taylor, who assisted in the final proof of Fermat's last theorem, raised just this question by invoking a hypothetical ("fictional") scenario concerning what one would say if the temporal order of discovery of crucial results relevant to Andrew Wiles' proof had been different. Clearly, Taylor did not think he was engaging in an amusing but logically irrelevant exercise of fiction.

§ 4. — Another Case — But the Same Peculiar Situation.

For all the reasons offered here, we believe that the bald assertion that it was Paul Cohen who proved the independence of CH is simply false. Factually mistaken and morally dubious. Histories of this intellectual episode, we believe, should be revised. This is not, of course, to take anything away from Cohen's great achievement. Cohen fully deserves his Fields Medal, and the great acclaim, honor and admiration attendant upon his achievement. There is no need, however, to gild the lily. Cohen's work – in particular, his creation, in his proof, of the epoch-making method of forcing – rightly secures him a place at the very summit of 20th century logic and mathematics.

But this revisionist conclusion contains the seeds of an outright revolution. To bring this out, let's turn briefly to another case. As we have seen, in the Gödel-Cohen case, we find a strange vacillation between two inconsistent claims, the claim that Cohen proved independence, and the claim that independence was the joint outcome of proofs by Gödel and Cohen. And as we noted at the beginning of this essay, the priority dispute in the case of the Taniyama-Shimura-Weil Conjecture is well-known and often acrimonious.(23) By contrast, no one denies that Andrew Wiles deserves credit for proving Fermat's Theorem, by proving that Conjecture. Even here, however, there are well-known complications. For example, Wiles's initial proof was faulty. After the flaw was discovered, André Weil — a seminal figure on the road to proving Fermat's last theorem--emphasized sardonically (as was his wont) the difference between completing a proof and *nearly* completing it. 'I am willing to believe,' said Weil, '[Wiles] has had some good ideas in trying to construct the proof, but the proof is not there. ... [T]o some extent, proving Fermat's theorem is like climbing Everest. If a man wants to climb Everest and falls short of it by 100 yards, he has not climbed Everest.'(24)

In the end, Wiles collaborated with Richard Taylor to successfully repair the proof. Taylor's description of the collaboration is, however, not quite straightforward. It is, to put it delicately, a somewhat ambiguous excursion into the mysterious realm of assigning credit. According to Taylor: 'What Andrew Wiles did and Andrew

(23) See Harris 2023 for the details of this acrimony and its grounds.
(24) Weil 1994.

and I completed was prove...'[(25)] So, Wiles *did* it but Wiles and Taylor *completed* it? Isn't completing part of doing? Be that as it may, the typical assignment of credit in this case is that Wiles proved Fermat's Theorem. Indeed, we might well be tempted to take this case as paradigmatic and say: if Wiles didn't prove Fermat's Theorem, it follows that rarely, if ever, has anyone proved anything!

The case of Wiles appears, in fact, to be eerily similar to the Gödel-Cohen case. After all, what Wiles proved was the Taniyama-Shimura-Weil conjecture. And the Taniyama-Shimura-Weil Conjecture is not the same thing as Fermat's Theorem, nor did Wiles show that Fermat's Theorem *follows* from the Taniyama-Shimura-Weil Conjecture. Someone else proved that — or, more precisely, several others proved that. (Indeed, the story of *that* proof recapitulates the very pattern we're calling attention to here.) The proof of Fermat's theorem required *two* results: 1.) If the Taniyama-Shimura-Weil Conjecture is true, then Fermat's Theorem is true; 2.) The Taniyama-Shimura-Weil Conjecture is true. Wiles proved *one* of these two propositions (the second one). So why do we say that it was Wiles who proved Fermat's Theorem? As in the Gödel-Cohen case, the proof of the *other* proposition was not only logically necessary, but highly non-trivial; indeed, it involved mathematics of the very first order — just as Gödel's contribution to independence did.

And, in fact, the same strange vacillation mentioned earlier *does* show up here too: 'Wiles's proof of Fermat's Last Theorem is a proof by British mathematician Andrew Wiles of a special case of the [Taniyama-Shimura-Weil Conjecture]. Together with Ribet's Theorem, it provides a proof for Fermat's Last Theorem.'[(26)] As with Gödel and Cohen, we are faced — here, in *adjoining sentences* — with the assertion of a plain logical contradiction. Obviously, Wiles did not prove *Fermat's Last Theorem* if what he proved is *not* Fermat's Last Theorem! It would be just as correct, and just as incorrect, to say that *Ribet* proved Fermat's Last Theorem, since together with Wiles's proof Ribet's work entails the truth of Fermat's Theorem. On this front — and echoing, as we noted earlier, our previous discussion of the danger of invoking time in deciding the logical allocation of credit — listen to a striking remark by Taylor: 'The thing that amuses me is that it seems that history could easily have been reversed. All these things could have been proved about the

[(25)] Taylor (undated).

[(26)] 'Wiles's Proof of Fermat's Last Theorem'.

relationship between modular forms and Galois groups, and then *Frey* could have come along and given nearly a two-line proof of Fermat's last theorem.'[(27)]

Before Wiles proved the Taniyama–Shimura–Weil conjecture, most mathematicians took it to be (in Ribet's words) 'completely inaccessible'[(28)] — that is, impossible to prove (at least relative to the mathematical knowledge at the time). It might be thought that this fact shows that Wiles did prove Fermat's Theorem after all. Wiles' meta-insight that the Taniyama-Shimura-Weil conjecture was accessible to proof was, undeniably, itself a great epistemic achievement (quite apart from the proof itself): more generally, seeing that something *can* be done is very often a crucial part of the story of doing it. But to repeat, what Wiles alone believed was that the *Taniyama-Shimura-Weil conjecture* was provable; this is what gave him the nerve to try to prove it, and this in turn, led him to prove it — to prove *Taniyama-Shimura-Weil*, not Fermat.

§ 5. — Credit Nihilism.

Now, the cases of Gödel-Cohen and (Frey-Serres)-Ribet-Wiles-(Taylor) clearly generalize. This discussion inclines one, thus, to a thoroughgoing skepticism about our current system of conventions regarding credit attribution concerning proofs — specifically, the part of that system that structurally supports our evident and deep-seated desire for heroes. This is not to say that there *are* no

(27) Taylor (undated), italics added. A referee on an earlier draft of this essay has emphasized to us how common it is to name a theorem after two or three mathematicians, and how diverse the kinds of contribution reflected in these shared accreditations are. As the referee notes, sometimes one person generalizes another's theorem; in other cases, one mathematician offers an outline of a proof, which is then fleshed out by another. And so on. To quote the referee, we are confronted here with "choices which, more than questions of truth, express the way in which novelty, beauty, originality, generality and depth are valued in mathematical practice by mathematicians themselves." We agree that this is how mathematical credit is often assigned, and (more to our purposes) that this policy is ethically wholesome. Indeed, we ourselves began this essay by noting Harris' recent 'different virtues' analysis of the Taniyama-Shimura-Weil conjecture along just these lines. And in sections 5 and 6 of this essay we are advocating for a potentially radical generalization of this policy. On the other hand, this policy is essentially *additive* in character — and our concern in this essay is about credit in the Cohen-Gödel case (as well as in other cases, including that of Wiles) being *subtracted*, rather than added.

(28) Singh 1997, p. 203.

heroes — sometimes an individual may really do something great in isolation, and when this happens, we can admire them for this achievement. But, if our reasoning has been correct, our current conventions falsify history and systematically misdescribe cases of joint or collaborative achievement as the work of a single (and singularly 'heroic') individual.

Note, please, that we are not pretending that there is some simple formula for determining when to assign credit for an achievement to one person, and when to two (or three, or twenty). There isn't, and this is due to indeterminacy in the boundary conditions on thoughts, the qualitative nature of the value or significance of a result, and many other factors. For example, when Ribet was working on his theorem, he came to a gap he couldn't figure out how to fill. Barry Mazur showed him how to fill it. Should we therefore deny that Ribet proved Ribet's Theorem? In this case, the answer is (arguably[29]) no, because Mazur's contribution was something that Mazur saw that Ribet himself was capable of seeing (and was, as it were, only temporarily overlooking): in Mazur's own words, 'But don't you see? You've already done it!'[30] No one would make this claim in the case of Gödel and Cohen — nor of Wiles and Ribet. Judgment and wisdom are necessary here, as they are everywhere else in life. But the existence of vagueness, qualitative judgments, etc. should not lead us to timidity. Fire engines are really, definitely red, even if blood oranges aren't; similarly, some questions in the arena of credit have clear answers, even if not all do. The claim that Cohen proved independence is, for the reasons we have provided, a clear case of error — one of many.

Someone might go further than our proposed revolution (to a Reign of Terror?), and endorse what we might call *credit nihilism*. On this view, the entire enterprise of assigning credit for achievements is intellectually, and even morally, bankrupt, and should be abolished altogether. For those sympathetic with this view, the explorations we have undertaken here will seem frivolous, or even malign.

(29) Ribet remarked on this incident: 'I said [to Mazur] you're absolutely right — of course — how did I not see it? I was completely astonished because it had never occurred to me...it was the crucial ingredient that I had been missing and it had been staring me in the face' (Singh 1997, p. 201). See our discussion of Skolem and Gödel, below, for the caution to be taken in attributing a discovery of a fact to someone because it is 'staring them in the face'.

(30) Singh ibid.

But while we agree with the credit nihilist that our current ways of thinking about credit need, for the reasons sketched, to be shaken up, it is difficult to see what recommends the idea that the whole institution of giving credit for work should be dismantled. Indeed, it isn't clear what that idea really amounts to. Emmy Noether proved Noether's Theorem (hence the name); Albert Einstein didn't. These are just historical facts; and credit, in the sense we have been discussing, seems to consist in acknowledging facts like these. What is the nihilist proposing, exactly? That we should deny, or deliberately forget, that Noether proved that theorem? That we should cease to see the proof as an *achievement* — as something good or commendable? Credit nihilism seems in this way to lead off in the direction of a general nihilism about truth and goodness; and quite apart from the implausibility of these conclusions on their own terms, it should be clear that if credit nihilism has these implications then it is a self-refuting position. (After all, we can ask: is credit nihilism itself true? Is it good to believe it?)

Perhaps the credit nihilist will say that they are not encouraging us to forget or deny (or disparage the significance of) the accomplishments of Emmy Noether, *given* that we have learned that she accomplished what she did. Instead, they are claiming that a society in which we never learned these authorial facts in the first place would be a superior society. (Compare Plato's Republic, in which parents never learn which children are 'theirs', nor do children, raised communally, learn who 'their' parents are.) In this creditless society we would confine ourselves to appreciating the content and intrinsic value of the discoveries themselves, without knowing or giving a thought to who made them; and the people who made the discoveries would, in turn, be liberated from the vanity, ambition, and envy which invariably accompany, and all too often corrupt, creative work when it is pursued in a culture of credit.

It is difficult to assess this view, since it departs so radically from human life as we know it. Is it really better not to admire people who do great things? Would anyone do great things in the absence of any possibility of recognition?

In any event, the people who make the claims about Cohen, Wiles, etc. which we have been disputing here cannot consistently endorse credit nihilism. One cannot have one's cake and eat it too. Those people are making an assertion of credit, and one cannot coherently stand with one foot inside the language game of credit and the other foot outside it and say, 'I think that the whole idea of credit is nonsense, and by the way, Cohen proved independence'. You can endorse the view that we should give up altogether on crediting people, or you can get the accreditation right. There is no defensible middle way.

§ 6. — What is it to make a Discovery in Logic and Mathematics?

We have insisted that Gödel and Cohen together proved independence — though not, obviously, in the sense of a collaboration. Well, perhaps not so obviously. The set theorist Juliette Kennedy has noted that when it came time for Cohen to publish his results,

> what followed over the next six months is a voluminous correspondence between the two [i.e., Cohen and Gödel], centered around the writing of the paper for the *Proceedings of the National Academy of Sciences*. The paper had to be carefully written; but Cohen was clearly impatient to go on to other work. It therefore fell to Gödel to fine tune the argument, as well as simplify it, all the while keeping Cohen in good spirits.'[31]

'Perhaps Cohen sensed,' adds Kennedy, 'while on the brink of his great discovery, the almost physical presence of the one mathematician [Gödel] who had walked the very long way up to that very door, but was unable to open it.' It's not clear to us if the door Kennedy is referring to is the unprovability of CH or, rather, independence. If the latter, we note that while it's true that Gödel hadn't opened it *by himself*, neither had Cohen. In any case, whether or not Cohen's essay was a kind of collaboration in this sense, more fundamentally, it was by virtue of each securing results which were severally necessary and jointly sufficient for the conclusion in question that the independence of the independence of CH was finally established. We emphasize, however, that this latter, strictly logical fact is not by itself the same thing as discovering independence — even jointly. Discovery is an *epistemic* achievement. Had they not *noticed* that their results were necessary and sufficient for independence, those results would not have constituted the *discovery* of independence. In the case of Gödel and Cohen, both of them of

[31] Kennedy 2011.

course *did* see — even in advance of developing their proofs — that if both CH and the negation of CH could be shown to be consistent with ZFC, this would constitute a proof of independence. And each saw the soundness of the other proof. In short, they both saw *that independence had been established by their work*.

By contrast, we can ask, did Thoralf Skolem prove the completeness of predicate logic before Gödel? Certainly, some mathematicians have suggested as much. Here is how Hao Wang (who went on to become Gödel's close associate) put his original understanding of what Skolem had done: 'all the pieces in Gödel's proof of the completeness of predicate logic had been available by 1929 in the work of Skolem...supplemented by a simple observation of Herbrand's...In my draft I explained this fact and said that Gödel had discovered the theorem independently and given it an attractive treatment.'[32] Gödel did not share this view of the situation. Here is how, in a letter, he pushed back on Wang's characterization:

> It seems to me that, in some points, you don't represent matters quite correctly. So I wanted to consider carefully what I have to say. — You say, in effect, that the completeness theorem is attributed to me only because of my attractive treatment. Perhaps it looks this way, if the situation is viewed from the present state of logic by a superficial observer. The completeness theorem, mathematically, is indeed an almost trivial consequence of Skolem 1922. However, the fact is that, at that time, nobody (including Skolem himself) drew this conclusion (neither from Skolem 1922 nor, as I did, from similar considerations of his own). — This blindness (or prejudice, or whatever you may call it) of logicians is indeed surprising. But I think the explanation is not hard to find. It lies in a widespread lack, at that time, of the required epistemological attitude toward metamathematics and toward nonfinitary reasoning.[33]

[32] Wang 1993, p. 122.
[33] Wang ibid.

The distinguished logician, Gaisi Takeuti, agrees: '... Skolem did not prove the completeness theorem and Gödel did.'[(34)].'

§ 7. — **Conclusion.**

We've reached the end of our discussion. Or so we believe. But no doubt someone, somewhere, isn't going to let us get away so easily. What would Wittgenstein say, they will say. 'Doesn't the very fact that mathematicians routinely describe Paul Cohen as having proved independence show how the word "prove" is actually used by mathematicians? Can't they use that word however they like? Given that this is how the language game is played, is there any room for the sort of challenge you have raised here?'

This description of the language game is certainly correct. Mathematicians do indeed routinely say that Cohen proved independence, as, indeed, we have gone out of our way to emphasize. What we need, however, is some reason to think that we're obliged to *acquiesce* in these ways of speaking. We have offered a series of arguments for the idea that these thoughts and assertions are *false*, and therefore that the present practices of awarding credit need to be revised. Where is the Wittgensteinian refutation of these arguments?

Moreover, assessments of the accuracy of discovery and credit claims are, themselves, an important *part* of the language game of logic and mathematics (and all other sciences). For example, many have protested the fact that Anthony Hewish won the Nobel Prize for the discovery of pulsars, despite the fact that it was his doctoral student, Jocelyn Bell Burnell, who was the first to notice the stellar radio source, and who reported that she 'had to be persistent in reporting the anomaly in the face of skepticism from Hewish, who was initially insistent that it was due to interference and man-made.'[(35)] Whatever else one thinks about this case, it clearly cuts

[(34)] Takeuti is, however, unconvinced by the reasons Gödel gave to Wang as to *why* Skolem didn't prove the theorem. 'To me,' wrote Takeuti, 'this [what Gödel said to Wang] seems to be a one-sided view. For example, Skolem proved various theorems around 1922 by entirely "non-finite" methods ... Using common sense, we would conclude that the major reason Skolem had been unable to do what Gödel was later able to do is that only in 1928 did Hilbert and Ackermann formalize the predicate logic and propose the completeness question as an open problem" (Takeuti 2003, pp. 15 and 24; brackets added).

[(35)] Wikipedia, 'Jocelyn Bell Burnell'.

no ice to say to the protestors — who in making these protests are *participating* in the language game — 'this is how the language game is played'.

In the wake of this Wittgensteinian challenge it is perhaps worth emphasizing that our central contentions here have not been *linguistic* or *sociological* but rather *metaphysical* and *ethical*. Metaphysically, the central question has been: what is it to prove — more generally, to discover — something? Ethically, the central question has been: given what it is to prove/discover something, how should credit for proofs/discoveries be allocated — and, more specifically, how should it be allocated in the case of Cohen and Gödel?

We conclude by recapitulating the main claims of this essay, followed by a final comment. 1. Paul Cohen did not prove independence of CH from ZFC. 2. The omnipresence of the claim that Cohen proved independence cannot be put down to sloppiness, the relative importance of the contributions, the temporal order of the contributions, etc., but must be understood as a genuine *mistake*, one which ought, in the interests of both truth and justice, to be corrected; 3. Analogous corrections, on the basis of the same arguments, are warranted in countless other cases in logic, mathematics, and other sciences — for example, in the case of Andrew Wiles and Fermat's Theorem; 4. Discovery is an *epistemic* achievement — that of *seeing that something is the case* — not a matter of non-epistemic encounters with truth-makers.

Finally, then, our last words on this subject. We've given throughout the text and in a footnote an indication of just how ubiquitous is the attribution to Paul Cohen of having proved the independence of CH. We conclude our discussion by recommending, as a paradigm of how the attribution *should* be recorded, the following, definitive statement by the distinguished set theorist Peter Koellner:

> Despite his efforts Cantor could not resolve CH. The problem persisted and was considered so important by Hilbert that he placed it first on his famous list of open problems to be faced by the 20th century. Hilbert also struggled to resolve CH, again without success. Ultimately, this lack of progress was explained by the combined results of Gödel and Cohen, which together showed that CH cannot be resolved on the basis of the axioms that mathematicians were employing; in modern terms, CH is independent of

Zermelo-Fraenkel set theory extended with the Axiom of Choice (ZFC)."[36]

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