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Gaisi Takeuti,
"Incompleteness Theorem and Its Frontier", in Matthias Bazz, Sy-David Friedman, & Jan Krajícek (editors), *Logic Colloquium 2001. Proceedings of the Annual European Summer Meeting of the Association for Symbolic Logic, held at Vienna, Austria, August 1-6, 2001* Wellesley, MA: A K Peters/Association for Symbolic Logic, 2001, pp. 434–439.

REVIEW

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In this article, Takeuti, himself a major figure in the history of proof theory, a friend of Gödel's, and already a contributor to our understanding of Gödel's work (see [Takeuti 1982; 2003][1]), begins by declaring that Gödel's "seemingly simple theorem" of [Gödel 1931] "changed our view of mathematics completely" (p. 434).

Now if we should read John Dawson's [1985; 1991] article on the impact which Gödel's incompleteness results had on his fellow mathematicians at the time, we should find ourselves initially perplexed by Takeuti's assertion (p. 434) that "[t]his revolutionary theorem changed the way mathematicians think of mathematics drastically." Dawson [1991, 84], to the contrary, reminds us that each Jean van Heijenoort [1967, 594], Georg Kreisel [1979, 13], and Stephen Kleene [1976, 767], among others close to Gödel, argued that Gödel's incompleteness theorems neither surprised mathematicians nor changed the way they worked. Indeed, so convincing was the proof of the second theorem in [Gödel 1931] that Gödel deemed unnecessary the anticipated second part of the aricle in which a more detailed proof would be set forth. Moreover, Gödel had already announced his results at the Second Conference on Epistemology of the Exact Sciences held in Königsberg in early September 1930. Certainly Gödel's Viennese colleagues, among them Carnap, had known of the results prior to the conference ([Dawson 1992, 85-86]; see also [Dawson 1984]). He even

[1]See also, *e.g.* [Takeuti 2000] for a technical discussion of the role and relevance of Gödel sentences in proof theory, in particular as applied to bounded arithmetic.

pointed out that Paul Finsler had gone so far as to claim that he had already anticipated and arrived at the same result [Dawson 1991, 89-91]. Finsler had argued that his [1926] conception of undecidability and Gödel's were markedly different. He rejected both formalistic, logicist-axiomatic, and intuitionistic efforts to deal with the paradoxes in favor of an unmitigated Platonism relying not on the internal consistency of the theory set forth, but on relating the elements of the theory to mathematical objects that are well-defined, rather than primitive. His goal was to avoid the paradoxes, rather than to, so-to-speak, sweep them under the rug, as he claimed, did the others. He distinguished formal from conceptual domains, the former comprised of all expressions of a language whose grammar and vocabulary have been rid of any ambiguities, the latter comprised of all extra-linguistic definitions, propositions and proofs, whether expressible or merely left unexpressed linguistically. Gödel replied to Finsler, in a letter of 25 March 1933 that Finsler's system of 1926 was "*überhaupt nicht definiert*" (see Dawson 1991, 91]. Others also misunderstood, Dawson [1991, f. 91] noted, what were the meaning and implications of his 1931 results.[2]

Among those who accepted the gist of Gödel incompleteness was Jean van Heijenoort, who often pointed in his lectures when the topic arose, to Fermat's Last Theorem, the Riemann Hypothesis, and the

[2]As late as 1963, for example, Bertrand Russell wondered whether Gödel incompleteness meant that 'school-boy arithmetic' is inconsistent rather than incomplete, and permitted $2 + 2 = 4.001$ (see [Dawson 1992, 95-96] and [Anellis 1995, 11]). Dawson [1991, 96], upon examining this episode, wondered whether Russell's response reflected Russell's momentary bewilderment upon learning of Gödel's theorems, or a continuing "puzzlement". Dawson [1991, 96] asked whether Russell was saying that "intuitively, he had recognized the futility of Hilbert's scheme of proving the consistency of arithmetic but had failed to consider the possibility of rigorously *proving* that futility", or if he actually was "revealing a belief that Gödel in fact had shown arithmetic to be *inconsistent*," and he notes that Henkin, for one, assumed that Russell supposed Gödel to have proven the inconsistency of arithmetic. He adds [Dawson 1991, 96-97] that, in either case, Gödel eventually received a copy of Russell's letter and consequently remarked to Abraham Robinson on 2 July 1973 that "Russell evidently misinterprets my result; however he does so in a very interesting manner. . . ." Another interpretation of Russell's reaction to Henkin could arise from consideration of the fact that of course Gödel's results do not assert merely the *impracticality* of obtaining a proof of the decidability of a theorem, but that it is theoretically impossible to find such a proof, so that the effect of Gödel's work was to deflate the sails of Russell's claims for logicism. Ivor Grattan-Guinness [2000, 593] renders an even more austere judgment, declaring not only that Russell "misunderstood Gödel's [incompleteness] theorem," but that "Russell was still struggling with the theorem at the end of his life when he wrote an addendum to his replies for a new edition" of [Schilpp 1994; 1971, xviii-xix]; see [Russell 1971].

Poincaré Conjecture, as likely examples of undecidable propositions. Given the history of these problems, it is hardly difficult to comprehend why van Heijenoort might so judge these propositions. By the time, after all, that Andrew Wiles finally perfected his proof of FLT [Wiles 1995][3], van Heijenoort was long since deceased, and some three and a half centuries had elapsed since Fermat claimed to have solved the problem, while in the interim the best mathematical minds undertook efforts to prove the theorem, a few in the twentieth-century coming close—close, but no cigar—or Fields Medal. And the dust having settled on Perlman's proof of the Poincaé Conjecture [Perelman 2005], van Heijenoort was even longer passed from the scene. Whether the Goldbach Conjecture and the Riemann Hypothesis are amenable to decidability remains an open question. And the time may come when our tools are sufficiently developed that many more open questions can be determined to be decidable or undecidable.[4] But that does not necessarily obviate the value of the line of research in recursion theory, and especially in computability, that Gödel's work inaugurated. As Dawson [1991, 92] noted, the early work of Church, Turing, and Kleene and the developments that followed thereon and continue to be pursued, have led to a refinement and clearer and sharper understanding of Gödel's initial work and in particular to its applications to logic and computer science.[5] In this sense, Gödel's incompleteness theorems have been a boon to mathematical logic, contributing to the development of recursion theory as well as to considerable work in proof theory.

[3]Wiles delivered a series of lectures on modular elliptic curves at Cambridge University in the Summer of 1993. When he completed his talk, it was clear that he had proven a conjecture that was already known to imply Fermat?s Last Theorem [FLT]. The first draft of the written proof amounted to some three hundered pages. It was subsequently discovered that the proof given at Cambridge had a gap in it. The complete and simplified, much shortened, proof, of some hundred pages, was published as *Modular Elliptic Curves and Fermat's Last Theorem* [Wiles 1995]. A summary and exposition of this work was given at the American Mathematical Society meeting in Orlando, Florida in January 1996. For an account of the history of FLT and of Wiles's proof that are accessible to the non-specialist, see [Singh 1997].

[4]Henry Pogorzelski [1977] claimed to have solved the Goldbach Conjecture; but his proof is not generally accepted; see, *e.g.* [Shanks 1985, 30-31, 222]. To date, only certain examples are accepted as settled; see, *e.g.* [Richstein 2001].

In the summer of 2004, Louis De Branges claimed to have solved the Riemann Hypothesis. He posted his 126-page proof on his web page, but has since posted an "Apology for the Proof of the Riemann Hypothesis"; see http://www.math.purdue.edu/ branges/site//Papers for both posts.

[5]For a survey of the origin and early history of recursion theory from the point of view of one of its principal founders, see, *e.g.* [Kleene 1981].

From the perspective of history, the import of Gödel incompleteness for Takeuti (p. 345) is that it "dealt a blow on Hilbert's program." In particular,

> It shows that Hilbert's program is "almost" impossible since the finite standpoint allows us to use only the elementary, clear, intuitive arguments on finite concrete figures but that consistency of set theory cannot be proved even in set theory, which is far beyond the elementary finite standpoint.

Before continuing with Takeuti's explanation, we should stop here and note that Solomon Feferman in "Systems of Predicative Analysis" [Feferman 1964], made use of his technique, developed in his paper "Transfinite Recursive Progressions of Axiomatic Theories" [Feferman 1964-8], of building up a set-theoretic hierarchy $\mathbf{Z}^*$ in stages, where at each, stage one could assume the consistency of earlier stages. For example, he could define a sequence of theories:

$$\begin{aligned}
\mathbf{Z}^0 &= \mathbf{ZF} \\
\mathbf{Z}^{\alpha+1} &= \mathbf{Z}^\alpha \cup \{\mathrm{Con}(\mathbf{Z}^\alpha)\} \\
\mathbf{Z}^\beta &= \cup_{\alpha<\beta}\mathbf{Z}^\alpha \\
\mathbf{Z}^* &= \cup_\beta \mathbf{Z}^\beta
\end{aligned}$$

and that of [Anellis 1979a; 1979b; 1980a; 1980b; 1983] was a similar effort. If Takeuti is correct, then both Feferman's hierarchy and mine must fail.

Now we examine Takeuti's explanation of what he means by asserting that Hilbert's program is "almost impossible"; he wrote (p 435)

> ... I said "almost" impossible in place of "absolutely" impossible only because there is no precise definition of the finite standpoint.

(That being the case, the Feferman and Anellis hierarchies being extended to $\kappa \geq \omega_1$, one could well argue that it is conceivable that there may be some stage in the hierarchy at which a Hilbert-type system is ω_1-complete and ω_1-conistent for κ; and moreover that the extended system is κ-complete and κ-consistent for some $\kappa, \kappa < \omega_1 < \omega_2$[6].)

[6] Takeuti [1994] also refers elsewhere in this context to the Gregorzyk hierarchy; see, *e.g.* [Gregorczyk *et al.* 1958].

I mentioned Hilbert-type systems because Takeuti tells us (p. 435) that Gödel's theorem was directed not so much at *Principia* specifically, as the title of his article indicates, but more specifically and directly at any effort to construct a system within or by the Hilbert program. Takeuti goes so far as to consider (p. 435) that: "It is rather curious that Gödel's arithmetization somehow resembles Hilbert?s formalization," although he fails to explain how, or to what degree, Gödel's arithmetization "somehow resembles" Hilbert's formalization. Nor does he say how, or why he believes (p. 435) "Gödel's idea of arithmetization was inspired by Hilbert's method." Moreover,

> It is an irony that as a result of Gödel carrying out Hilbert?s idea mathematically, he proved exactly the opposite of what Hilbert was trying to achieve. (p. 435)

Then follows the illuminating historical note (p. 435) that Paul Bernays, Hilbert's "primary advocate," once told Takeuti that "Gödel accomplished through his diligence what we, Hilbert's disciples, were too lazy to pursue."

Before proceeding with some personal reminiscences of Gödel and Kurt Schütte with which his article concludes, Takeuti takes what at first seems a detour, but which in fact illumines his previous remark concerning establishing that Gödel incompleteness shows the Hilbert program to be "almost", rather than "absolutely", impossible. The example given (pp. 435-437) is that of Samuel R. Buss in which the bounded arithmetic S^i_2, $(i = 0, 1, 2, \ldots)$ corresponding to Δ^p_i, S^i_2 is a very weak system of arithmetic.[7] It is noted that the separation of S^1_2 and S^2_2 is "closely related" to the separation of P and NP.[8] Takeuti remarks that Buss proved that Gödel incompleteness holds for S^i_2, *i.e.* that S^i_2 cannot prove its own completeness. Other related examples are also given, notably that Alex J. Wilke and Jeffrey B. Paris proved that $S_2 + Exp$ fails to prove the consistency of even the weakest of all systems of bounded arithmetic, namely Abraham Robinson's Q.[9]

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[7]See, *e.g.* [Buss 1986; 1994; 1995; 1997; 1999; 2005]; [Buss & Beckman 2004], [Buss *et al.* 1993], and [Buss *et al.* 1992].

[8]Takeuti spoke on "Incompleteness Theorems, Forcing and P $\neq$ NP" at the DIMACS Workshop on Feasible Arithmetics and Length of Proofs, held at Rutgers University, April 21-23, 1996.

[9]See, *e.g.* [Wilke & Paris 1985; 1987]. See also [Wilke 1980; 1989].

Elsewhere, Henkin had studied and generalized the conceptions of ω-completeness and ω-consistency (Henkin 1954; 1957]).

For Takeuti's contributions to the problem of the incompleteness of bounded arithmetics, see *e.g.* [Takeuti 1982; 1991; 1995; 2000; 2003], [Clote & Takeuti 1986], and [Krajícek *et al.* 1991].

Working with S_2^1's axiom PIND (polynomial-time induction), Takeuti shows (p. 437) that

$$S_2^1 \vdash \varphi_i \leftrightarrow \forall x \neg Prf(|x|_i \ulcorner \varphi \urcorner)\,.$$

And thus that there exist infinitely many Gödel sentences ϕ for the proof predicate Prf.

The weakest and most disappointing, even frustrating, aspects of Takeuti's article are two. (1) That he fails, as we noted, to make clear what he means in some of what he says: in the case of failing to explain how, or to what degree, Gödel's arithmetization "somehow resembles" Hilbert's formalization; and of how, or why he believes, "Gödel's idea of arithmetization was inspired by Hilbert's method" And (2) that there are no references provided for the results to which Takeuti refers, in particular to references for Buss's work that he sketches in some detail; or for the Wilke-Paris theorem.

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