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Book Review

Kurt Gödel. *Collected Works*, Volume IV, Correspondence A–G, edited by Solomon Feferman et al., Clarendon Press, Oxford, 2003, ISBN 0 19 850073 4, pp. xxii+662.

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1 Introduction

When I arrived at Stanford as a graduate student in 1984 I immediately heard about the projected publication of Gödel's *Collected Works* (henceforth CW, [10] and [11]). My interest was piqued not only by hearing Solomon Feferman talk about the project but also through my friendship with one of the early collaborators, a young man from Japan, Tadashi Nagayama, who was an expert on Gabelsberger, the special stenography used by Gödel and many other German and Austrian academicians at the time to take notes or draft lectures and letters. I often wondered: How does someone from Japan end up being an expert on Gabelsberger? Perhaps at the time I was naïve enough to think that the major obstacle to such an edition was the transcription of such documents. Twenty years afterward and with five volumes of Gödel's *Collected Works* on my desk it has become apparent to me what a gigantic effort the publication of these five volumes has been. The Gödel Nachlaß at Princeton had to be catalogued, a selection of what would go into the volumes had to be determined, experts on and off the editorial board had to be commissioned to write introductions and so on to other, and not less daunting, problems of choices of typography, layout, textual annotations, and so forth.

Following the original plan, all the published writings were reprinted in Volume I [7] and Volume II [8] accompanied by facing translations into English for German originals. Beyond these, the remaining volumes were to contain "Gödel's unpublished manuscripts, lectures, lecture notes, and correspondence, as well as extracts from his scientific notebooks" (CW, Vol. I, preface). Indeed, Volume III [9] contained a comprehensive selection of unpublished essays and lectures, while the present Volumes IV and V are devoted to an equally comprehensive selection from

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the correspondence. However, the plan to publish extracts from the scientific notebooks had to be abandoned, even though extensive transcriptions had been made from them, because the task of publishing them in a coherent form became forbidding. This is material for future scholars to pursue.

The two new volumes give us a broad and representative selection of the most important correspondence in which Gödel engaged throughout his life. In total, fifty individual correspondents are represented. The criteria followed for the selections are described by the editors as follows: "In all cases our criterion for inclusion was that letters should either possess intrinsic scientific, philosophical or historical interest or should illuminate Gödel's thoughts or his personal relationship with others" (CW, Vol. IV, v). The edition itself contains, in addition to the letters, also the corresponding calendars. Furthermore, Volume V contains a full inventory of Gödel's Nachlaß, an extremely useful tool for any future research on Gödel. The list of correspondents is impressive. Represented are, among others, Bernays, Boone, Carnap, Church, P. Cohen, Herbrand, Menger, A. Robinson, Tarski, von Neumann, Wang, and Zermelo. But the editors were wary of just going after the big names. Indeed, they included correspondence with lesser known figures and sometimes even completely unknown persons (at least in academic circles). Interesting in this connection are also items of more personal correspondence, such as the letters to his mother, in which Gödel discusses his opinions on, among other things, a number of religious topics.

2 The Correspondents

The editors have wisely decided to publish the correspondence not in chronological order but rather alphabetically according to the correspondent's name. When the letters are not in English we are presented with the German text and a facing translation into English. Following the format of Volumes I – III, each exchange with a correspondent is prefaced by an introduction written by one of the editors or by a specialist commissioned especially for the occasion. The list of collaborators for the project is impressive and it goes without saying that these, often lengthy, introductions are outstanding pieces of scholarship and a great help to the reader who is thereby informed as to the context and importance of the exchange. The alphabetical presentation and the relevant introduction allow for easier reading as many topics are pursued from letter to letter with the same correspondent.

We find broadly four types of correspondents:

1. Academics (this represents the majority of the selections)
2. Relatives (Gödel's mother, Marianne)
3. Editors (A. Angoff, T. Honderich, P. Schilpp, and others)
4. Occasional correspondents who solicited Gödel's help on various issues related to his work or career (C. Reid, Grandjean, and others).

Before discussing in more detail some of the correspondence, I would like to point out that it not only enlightens us about Gödel's life and work but also gives us precious information about the correspondents themselves in some of whose cases, for example, Herbrand, we have very little else left.

We do, of course, know quite a bit about Gödel's life and work, in particular through the books by Wang [21] and [23] and Dawson [4] among others.[1] Wang had consulted the Gödel Nachlaß early on and having recently reread his book *Reflections* *on Kurt Gödel* I was actually surprised at how much he was able to take in of this vast collection. However, Wang's treatment of Gödel's opinions has to be approached cautiously and thus the correspondence is a more trustworthy source of information (on Wang on Gödel see Parsons [19]). And in any case, no summary can do full justice to the original texts now in front of us.

Gödel was obviously a very reserved person on personal matters. The correspondence in Volumes IV and V does not tell us much about, say, his relationship with his wife, or with the academic community in Princeton, or about his health problems, or his views on contemporary world affairs. Such topics might surface from time to time (for instance, Gödel discusses various types of drugs with Bernays, CW, Vol. IV, pp. 298–301) but overall we do not get much. Incidentally, even the letters to his mother published here do not so much discuss his feelings or personal matters but rather discuss more intellectual matters. Of course, since I have not seen the complete bulk of the correspondence (totaling 245 letters from Gödel to his mother, five of them published (excerpts) in Volume IV) it is quite possible that the selection here was determined by the intrinsic intellectual interest of the topics discussed.[2] In these letters Gödel shows a keen interest in religion. As John Dawson informs us in the lucid introduction to the exchange, Gödel, who as a youth attended Protestant schools in Brno, was very critical of the religious instruction he had received. He seemed to have despised organized religion while holding on to the importance of religion per se. He characterized his position as theistic but not pantheistic (see the unsent reply to Grandjean, CW, Vol. IV, p. 448). While the correspondence *says* little about the more personal aspects of Gödel's life, it certainly *shows* much about him as a person and a scholar. Gödel was meticulous and cautious. While these are virtues, especially in a logician, in the case of Gödel he was so almost to a fault. This led to feelings of exasperation and in some cases rage on the part of the affected correspondents.[3] The correspondence is a real window into this aspect of Gödel's personality. Examples of such are the finicky discussion with Behmann about a minor correction of a statement by Dubislav to be published in *Erkenntnis*, the stalling tactics with Heyting about a projected joint book on foundations or with Schilpp for the essay on Carnap, and his correspondence with Nagel, Angoff, and Follett concerning the publication of Nagel and Newman's "Gödel's Proof" [18]. Additional examples could easily be provided. In some cases, it was obvious intellectual dissatisfaction that stopped Gödel (as in the case of the failure to deliver to Schilpp the essay for the volume on Carnap, "Is mathematics syntax of language"; or the English translation of a revised version of "On a hitherto unutilized extension of the finitary standpoint," long promised to Bernays and published only posthumously); in other cases, such as the ones mentioned above with Nagel, Angoff, and Follett, personal idiosyncrasies played a role too.

Gödel's caution was especially evident in the case of philosophical topics, as in the two cases just mentioned. While he had very strong opinions (for instance about the negative effect of "anti-metaphysical" or "anti-platonist" attitudes for logic, science, and culture in general) he rarely expressed them publicly. However, the correspondence is more explicit about these issues. Famously, he claimed that what stopped Skolem or anyone else from discovering the completeness theorem was the finitist bias shared by all researchers in logic in the 1920s. On December 7, 1967, discussing issues related to Skolem's work, he wrote to Wang:

> This blindness (or prejudice, or whatever you might 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 non-finitary reasoning . . . I might add that my objectivistic conception of mathematics and metamathematics in general, and of transfinite reasoning in particular, was fundamental also to my other work in logic. (CW, Vol. V, pp. 397–98; already in Wang [21], p. 8)

It is nothing short of paradoxical that the person with whom Gödel shared this criticism of the "finitistisches Vorurteil" (CW, Vol. V, p. 422), that is, Ernst Zermelo, was to show very little understanding of Gödel's achievements.[4] This particular commitment to finitism seems to have been only one facet of what Gödel characterized as a prejudice of the time, which could perhaps be negatively characterized as the rejection of "Platonism" in philosophy, religion, and the sciences. Instances of such prejudice were mechanism in biology, nominalism in the philosophy of mathematics, and the claim that there is no mind separate from matter (see Wang [21], p. 326). The correspondence gives us many examples of Gödel's position and how it influenced even the history of his publications or lack thereof. For instance, concerning his essay on Carnap, Gödel wrote to Schilpp:

> However, I feel I owe you an explanation why I did not send my paper earlier. The fact is that I have completed several different versions, but none of them satisfies me. It is easy to allege very weighty and striking arguments in favor of my views, but a complete elucidation of the situation turned out to be more difficult than I had anticipated, doubtless in consequence of the fact that the subject matter is closely related to, and partly identical with, one of the basic problems of philosophy, namely the question of the objective reality of concepts and their relations. On the other hand, ~~in view~~ because of widely held prejudices, it may do more harm than good to publish half done work. (Gödel to Schilpp, Feb. 3, 1959, Vol. V, p. 244)

As for the consequences in religion of these biases, in a letter to his mother he remarks that "90% of contemporary philosophers see their principal task to be that of beating religion out of men's heads, and in that way they have the same effect as the bad churches" (CW, Vol. IV, pp. 436–37).

3 Some Novel Aspects about Gödel's Thought Which Emerge from the Correspondence

One obvious question to ask is whether the correspondence gives us novel information about Gödel's thought and work. Thus, the question I would like to address is: *What more do we know that we could not have known without the correspondence?*

It would obviously be useless within the scope of a review to try to answer this question by giving a survey of the range of topics treated with so many different correspondents. It suffices to say that Feferman's clear and informative introduction to the long term correspondence with Bernays runs to thirty-nine pages and the aim there is only to highlight the major topics of discussion between the two scholars. In the light of this I propose to mention in this section two aspects, of the many which could be selected, of Gödel's thought about which the correspondence provides novel information. Then the rest of the review will focus on one specific issue only, that is, what the correspondence contributes to our knowledge of the context of Gödel's most important and best known result, that is, the 1931 incompleteness theorem (this is shorthand for first and second incompleteness theorems).

Let me thus briefly indicate the importance of the correspondence in enlightening Gödel's thought on philosophy and finitism.

3.1 Philosophy It is well known that, after 1943, philosophical interests came to dominate Gödel's thinking. Writing in 1966 to Bernays he expresses thanks "for the wishes concerning my philosophical investigations. For they have been my principal interest for a long time" (Gödel to Bernays, May 22, 1966, Vol. IV, p. 253). Concerning Gödel on philosophy the correspondence is surprising in two ways. First of all, for what it contains. The correspondence with Günther, wonderfully introduced by Parsons, shows that Gödel had quite a substantial interest in post-Kantian idealistic philosophy (Fichte, Hegel, Schelling). In the correspondence with Bernays we find extensive discussions of, among others, Fries, Nelson, Wittgenstein, and Hegel. In some cases the comments are quite biting: "As for Wittgenstein's book on the foundations of mathematics, I also read parts of it. It seemed to me at the time that the benefit created by it may be mainly that it shows the falsity of the assertions set forth in it. The footnote adds, "and in the *Tractatus*. (the book itself really contains very few assertions)" (Gödel to Bernays, Oct. 30, 1958, CW, Vol. IV, p.161).

However, the two volumes of correspondence contain very little on the two philosophers Gödel spent most time reading and thinking about, Husserl and Leibniz. This is where we wish we had more by way of transcriptions of the notebooks and probing deeper in this connection is a task left for future researchers.

3.2 Finitism Gödel's position on finitism underwent several changes. As Feferman points out in his introduction to the Gödel-Bernays exchange, the correspondence allows us to gauge the extent to which Gödel's views concerning the upper bound of finitary reasoning remained "unsettled." While in the 1931 paper (in a remark to be discussed below), Gödel seems to entertain the possibility that finitistic reasoning might outstrip the resources of Peano Arithmetic (**PA**), later comments indicate that Gödel would consider finitary reasoning as contained in **PA**. From previously available material, it would have seemed safe to conclude that for Gödel finitistic reasoning could in fact be captured in systems much weaker than Peano Arithmetic, quite possibly in Primitive Recursive Arithmetic. However, the correspondence with Bernays shows that Gödel was much taken by Kreisel's 1960 characterization of finitistic proof by means of autonomous progressions of formal systems of ordinal logics (in the sense of Turing). In 1961 Gödel writes to Bernays: "I had interesting discussions with Kreisel. He now really seems to have shown in a mathematically satisfying way that the first ϵ-number is the precise limit of what is finitary. I find this result very beautiful, even if it will perhaps require a phenomenological substructure in order to be completely satisfying" (CW, Vol. IV, p. 193). And again to Bernays in 1967: "I am now convinced that ϵ_0 is a bound on finitism, not merely in practice but also in principle, and that it will also be possible to prove that convincingly" (CW, Vol. IV, p. 255). The issue of what exactly is the extent of finitism re-emerges also in later correspondence concerning Bernays' proof of transfinite induction up to ϵ_0 for the second edition of *Grundlagen der Mathematik* (1968–1970). This led to a discussion of whether free choice sequences are to be included in finitary mathematics. Other essential information on the topic of Gödel's views on finitism is also to be gained from other correspondence, such as the exchange with Herbrand, von Neumann (see below), and others.

Let us now move to the question of what the correspondence contributes to our knowledge of the context of Gödel's incompleteness theorem. I will survey in succession:

1. what Gödel says about the heuristics of the theorem;
2. a number of comments he makes related to the proof;
3. the immediate impact of the theorem (wonderfully documented by the correspondence with, among others, Bernays, Herbrand, and von Neumann);
4. finally, a few things about Gödel's own interpretation of the lasting philosophical significance of the result.

I will assume that the reader has encountered the incompleteness theorem before. I should also state here that throughout the review I refer to parts of the correspondence that might have been familiar to, and used in publications by, those researchers who had access to parts of the Gödel correspondence prior to their publication in the *Collected Works*. But this does not detract from the fact that the information I will refer to became known through the correspondence, which is now made available to the general public. For this reason I will only refer briefly in notes to articles in the literature where the correspondence was already exploited.

4 The Heuristic Path to the Theorem: Truth and Provability

As we have seen, it was his unorthodox epistemological attitude that Gödel identified as the condition of possibility for his groundbreaking results on completeness. In the case of the incompleteness theorems the key to the result was again to focus on a notion of which philosophers and logicians were skeptical, that is, the notion of truth. He writes this explicitly to Wang (CW, Vol. V, p. 398). To the same attitude he credits his work on the consistency of the axiom of choice with the remaining axioms of set theory in contrast to similar developments in Hilbert (see letter to van Heijenoort, July 8, 1965, Vol. IV, p. 324).

Let us begin with a description of the heuristics which led to the incompleteness theorem given in an unsent letter to Yossef Balas, a master degree student at the University of Northern Iowa.[5] The letter dates from around 1970:

> I have explained the heuristic principle for the construction of propositions undecidable in a given formal system in the lectures I gave in Princeton in 1934 . . . The occasion for comparing truth and demonstrability was an attempt to give a relative model-theoretic consistency proof of analysis in arithmetic. This leads almost by necessity to such a comparison. (Gödel to Balas, undated, Vol. IV, p. 10)

A crossed out paragraph connects nicely with remarks I made in the previous section on the prejudices of the time:

> However in consequence of the philosophical prejudices of our times 1. nobody was looking for a relative consistency proof because i[t] was considered axiomatic that a consistency proof must be finitary in order to make sense 2. a concept of objective mathematical truth as opposed to demonstrability was viewed with greatest suspicion and widely rejected as meaningless. (Gödel to Balas, undated, Vol. IV, p. 10)

An interesting question here is: How did Gödel manage not to fall prey to what he called the prejudices of the time? This is something that the correspondence does not clear up for us. He writes to Grandjean (CW, Vol. IV, p. 448) that one important philosophical influence was Heinrich Gomperz. Wang ([23], p. 22) seems to locate the source of Gödel's Platonism in his discovery of Plato through Gomperz's lectures. But the correspondence is silent on this. I think this is a point that would be interesting to investigate more thoroughly.[6]

Let us conclude with a the description of the heuristics for the discovery of the incompleteness results given by Gödel to Balas:

> ⟨For an arithmetical model of analysis is nothing else but an arithme⟩tical $\in$-relation satisfying the comprehension axiom $(\exists n)(x)[x \in n \equiv \phi(x)]$. ~~by an arithmetical~~ Now, if in the latter "$\phi(x)$"⟨is replaced⟩ by "$\phi(x)$ is provable", such an ⟨$\in$-⟩ relation can easily be defined. Hence, if truth were equivalent to provability, we would have reached our goal. However, (and this is the decisive point) it follows from the correct solution of the semantic paradoxes ~~i.e., the fact~~ that ~~the concept of~~ "truth" of the propositions of a language *cannot be expressed* in the same language, while provability (being an arithmetical relation) *can*. Hence true $\neq$ provable.⟩ (Gödel to Balas, undated, Vol. IV, p. 10)

This suggests that the undefinability of truth (a theorem usually attributed to Tarski) was the key fact in the heuristics leading to the incompleteness theorem.[7] Of course, since these lines were written in 1970 we want to make sure that they are not just a "rational reconstruction" of what happened forty years earlier. The best evidence we have to support Gödel's account in this connection comes from a letter to Bernays dated April 2, 1931 where Gödel discusses at length the definition of truth for a first-order system Z in a second-order system S. Gödel writes that

> Simultaneously and independently of me (as I gathered from a conversation), Mr. Tarski developed the idea of defining the concept "true proposition" in this way (for other purposes, to be sure). (Gödel to Bernays, Vol. IV, p. 97)

Concerning this passage, Feferman remarks that

> the specific definition of W [the set of true sentences of arithmetic, PM] that [Gödel] describes depends on the fact that every element of the standard model of Z is denoted by a numeral; the more general definition of truth for languages of other structures given by Tarski in terms of satisfaction is not noted by Gödel. (CW, Vol. IV, p. 45)

Gödel appeals to the undefinability of truth within a formal system of arithmetic also in his explanations to Zermelo on October 12, 1931:

> In connection with what has been said, one can moreover also carry out my proof as follows: the class W of correct formulas *is never* coextensive with a class sign of that same system (for the assumption that that is the case leads to a contradiction). The class B of provable formulas *is* coextensive with a class sign of that same system (as one can show in detail); consequently B and W cannot be coextensive with each other. But because B $\underline{C}$ W, B C W holds, i.e. there is a correct formula A that is not provable. Because A is correct, not-A is also not provable, i.e., A is undecidable. This proof has, however, the disadvantage that it furnishes no construction of the undecidable statement and is not intuitionistically unobjectionable. (Gödel to Zermelo, October 12, 1931, Vol. V, pp. 427 and 429, his emphasis)

We thus see why the reasoning that led to the discovery of the theorem was later removed from the final presentation of the proof in 1931, where Gödel does not prove the undefinability of arithmetical truth. While he did not share in the supposed prejudice of the time he realized that his argument would be open to objection had he made use of a proof that was not "intuitionistically unobjectionable." The reader should keep in mind that in 1931 "intuitionistically unobjectionable" was taken to mean “finitistically unobjectionable” (see Mancosu [16], pp. 167–68). In a postscript to a letter to van Heijenoort written on February 22, 1964, Gödel remarks again on the motivations that led him to the incompleteness theorem:

> Perhaps you were puzzled by the fact that I once said an attempted relative consistency proof for analysis led to the proof of the existence of undecidable propositions and another time that the heuristic principle and the first version of the proof were those given in Sect. 7 of my 1934 Princeton lectures. But it was precisely the relative consistency proof which made it necessary to formalize either “truth” or “provability” and thereby forced a comparison of the two in this respect. (Gödel to van Heijenoort, February 22, 1964, Vol. V, p. 313)

It was thus the comparison between truth and provability that was the heuristic key to the theorem. And while some of this could have been already gathered from the published version of the 1934 lectures (in Davis [1], pp. 63–65; and now in CW, Vol. I), the correspondence adds considerable information on the issue.

5 The 1931 Presentation

Concerning the specific details of the incompleteness paper we learn something new from the correspondence with van Heijenoort. The 1931 article shows the existence of independent arithmetical statements from a theory P which consists of a system of axioms for arithmetic with a simple theory of types as background logic. The simple theory of types also has two essentially mathematical axioms needed to include Peano Arithmetic, namely, axiom I.3, induction in second-order form, and axiom IV, full comprehension. The individual variables of the system range over the class of individuals, which is here identified with the class of natural numbers. However, Gödel could have proceeded by simply developing arithmetic within the simple theory of types (with the axiom of infinity). Why did he choose the former approach? One reason is given in a letter dated August 14, 1964. Gödel says:

> I identified the individuals of PM with the integers in order to obtain a system every proposition of which has a well-defined meaning in classical mathematics and, therefore, viewed from the standpoint of classical mathematics, must be either true or false. The question of completeness is of philosophical interest only for systems which satisfy some requirement of this kind. (Gödel to van Heijenoort, Vol. V, p. 316)

And in the following letter (August 15, 1964) a second motivation is adduced:

> On rereading my letter of Aug. 14 I find that in suggestion 2. ad M(4) I have given the wrong impression that what I say there was my only reason for adjoining Peano’s axioms. Another reason, of course, was the simplification of the proofs which ⟨results⟩ from it. In fact, I believe that either one of these two considerations would have been sufficient by itself. However, if the second one had been my only reason, I could have omitted the axiom of complete induction, thereby admitting other individuals besides the integers. (p. 317)

As Goldfarb remarks in his useful introduction to the Gödel-van Heijenoort correspondence, Gödel’s “remarks here are not echoed in any other known writing of his” (CW, Vol. V, p. 304).

6 The Impact of Gödel's Incompleteness Theorem

After the announcement of his first incompleteness theorem at Königsberg in September 1930, the news of Gödel's epoch-making result traveled fast. Nöbeling informs Menger (CW, Vol. IV, p. 85), Courant and Schur inform Bernays (CW, Vol. IV, p. 81), von Neumann tells Herbrand (CW, Vol. V, p. 15; see also Hempel to Kaufmann, December 13, 1930 in Mancosu [17]) who in turn tells Behmann (CW, Vol. IV, p. 39). In Mancosu [17] I documented the rapid spread of the news among several logicians and philosophers of mathematics. However, knowing of the forthcoming edition of the correspondence, I did not discuss the correspondence between Gödel and Bernays, Herbrand[8] and von Neumann[9] on the relevance of Gödel's incompleteness theorem for Hilbert's program, an issue which had already been briefly summarized in Wang [23], pp. 43 and 84–91 and Dawson [4], pp. 68–75. Gödel in his 1931 paper states that his results

> do not contradict Hilbert's formalistic viewpoint. For this viewpoint presupposes only the existence of a consistency proof in which nothing but finitary means of proof is used, and it is conceivable that there exist finitary proofs that cannot be expressed in the formalism of P. (CW, Vol. I, p. 195)

Gödel will eventually abandon this viewpoint.[10] For instance in March 1966 he writes to Constance Reid that

> Hilbert's scheme for the foundations of mathematics remains highly interesting and important in spite of my negative results. What has been proved is only that the *specific epistemological* objective which Hilbert had in mind cannot be obtained. This objective was to prove the consistency of the axioms of classical mathematics on the basis of evidence just as concrete and immediately convincing as elementary arithmetic. (Gödel to Reid, March 22, 1966, Vol. V, p. 187, his emphasis)

But this is not the way things stood in the immediate aftermath of the discovery and publication of the result. Whereas von Neumann and Herbrand saw in Gödel's results a definitive defeat for Hilbert's program, Bernays and Gödel were more cautious. Let us recall that after the unassuming remark made by Gödel at the Königsberg meeting stating what amounts to the first incompleteness theorem (September 7, 1930), John von Neumann very quickly realized the implications of the result Gödel had announced. Soon afterward von Neumann was able to prove that "the consistency of mathematics is unprovable," that is, what we now call the second incompleteness theorem. He informed Gödel of the discovery:

> I have recently concerned myself again with logic, using the methods you have employed so successfully in order to exhibit undecidable properties. In doing so I achieved a result that seems to me to be remarkable. Namely, I was able to show that the consistency of mathematics is unprovable. (von Neumann to Gödel, November 20, 1930, Vol. V, p. 337).

Here is how von Neumann states the result:

> In a formal system that contains arithmetic one can express, following your considerations, that the formula $1 = 2$ cannot be the end-formula of a proof starting with the axioms of this system—in fact, this formulation is a formula of the formal system under consideration. Let it be called W. In a contradictory system any formula is provable, thus also W. If the consistency [of the system] is established intuitionistically, then it is possible, through a "translation" of the contentual intuitionistic considerations into the formal [system], to prove W also. (On account of your result one might possibly doubt such a "translatability". But I believe that in the present case it must obtain, and

> I would very much like to learn your view on this point). Thus with unprovable W the system is consistent, but the consistency is unprovable. I showed now: W is always unprovable in consistent systems, i.e., a putative effective proof of W can certainly be transformed into a contradiction. (von Neumann to Gödel, November 20, 1930, Vol. V, p.337)

Von Neumann concludes his letter by asking Gödel to express his point of view on the issue of the "translation" of "contentual intuitionistic" considerations into formal proofs. Moreover, he asked when Gödel's article would appear and whether he could get a copy of the proofs so as to present his result in agreement with Gödel's presentation. Finally, he informed Gödel that the mathematician E. Schmidt considered his result "to be the greatest logical discovery in a long time" (CW, Vol. V, p. 337).

However, before von Neumann wrote this to Gödel, the latter had already sent (October 23, 1930) the incompleteness article for publication. In it Gödel had also stated the second incompleteness theorem. Gödel's reply to von Neumann's letter seems to have been lost but on 29 November, 1930 von Neumann thanks Gödel for his letter and says:

> Many thanks for your letter and your reprint. As you have established the theorem on the unprovability of consistency as a natural continuation and deepening of your earlier results, I clearly won't publish on this subject. (von Neumann to Gödel, November 29, 1930, Vol. V, p. 339)

Two points in this letter deserve special emphasis. The first concerns the possibility of translating intuitionistic proofs into formalistic proofs:

> I believe that every intuitionistic consideration can be formally copied, because the "arbitrarily nested" recursions of Bernays-Hilbert are equivalent to ordinary transfinite recursions up to the appropriate ordinals of the second number class. This is a process that can be formally captured, unless there is an intuitionistically definable ordinal of the second number class that could not be defined formally—which is in my view unthinkable. Intuitionism clearly has no finite axiom system, but that does not prevent its being a part of classical mathematics that does have one. (von Neumann to Gödel, November 29, 1930, Vol. V, p.339)

One has to keep in mind that the discussion on intuitionistic demonstrations is here carried out with the assumption that intuitionism and finitism are equivalent (it will only be in 1933 that the two will definitely be shown to be nonequivalent).

Von Neumann drew from Gödel's result the conclusion that the so-called *Grundlagenfrage* could only be answered negatively:

> Thus, I think that your result has solved negatively the foundational question: there is no rigorous justification for classical mathematics. (von Neumann to Gödel, November 29, 1930, Vol. V, 339)[11]

Gödel replied sending two letters and the proofs of the 1931 article but the letters have been lost. The last letter from von Neumann to Gödel dealing with questions of incompleteness was written on January 12, 1931. It contains several interesting points:

1. Von Neumann claims to have a decision procedure for deciding the provability or unprovability of sentences built by means of Boolean operations (conjunction, negation) and the predicate $B(x)$, "provable". As pointed out by Sieg in his introduction (CW, Vol. V, p. 332) this seemingly anticipates a solution to Friedman's 35th problem, given in print by Boolos in 1976.

2. Apparently Gödel had communicated some observations on ω-consistency; in particular, he claimed that ω-consistency at a certain level could be inferred from consistency at the next higher level.
3. Von Neumann expressed disagreement with Gödel's position on the formalization of intuitionistic proofs. Gödel had claimed (in one of the two lost letters) that it is not at all clear that every intuitionistic proof can be captured in a formal system. Von Neumann replied:

    > Clearly I cannot prove that every intuitionistically correct *construction of arithmetic* is formalizable either in A or M or even in Z. [A=first order arithmetic; M=second order arithmetic; Z=von Neumann's axiomatization of set theory] for intuitionism is undefined and undefinable. But is it not a fact, that not a single construction is known that cannot be formalized in A, and that no living logician is in the position of naming such [a construction]? Or am I wrong and you know an effective intuitionistic arithmetic construction whose formalization in A creates difficulties? If that, to my great surprise, should be the case, then the formalization should certainly work in M or Z! (von Neumann to Gödel, January 12, 1931, Vol. V, p. 343)[12]

4. Finally, von Neumann suggested a simplification for the proof of the second incompleteness theorem as presented by Gödel.

This concludes the correspondence between the two scholars centering on the incompleteness theorem.

Another scholar who shared von Neumann's evaluation of the foundational situation in the light of Gödel's results was Jacques Herbrand. I will not, for reasons of space, discuss this part of the correspondence but only refer to Sieg's insightful introduction to the Gödel-Herbrand correspondence in which he spells out the relevance of the exchange both for the issue of the scope of finitism and for the consequences of Gödel's theorems for Hilbert's program. The former topic will lead the reader to explore one facet of the emergence of computability theory through Herbrand's influence on Gödel's notion of general recursive function. The latter topic nicely ties in with the other exchanges Gödel was having with von Neumann and Bernays.

The issue of the effect of Gödel's theorems on Hilbert's program was of course a central one in the discussion with Bernays. The correspondence with Bernays on this topic is much more extended and more technical than the one with von Neumann and thus I will only emphasize a few points.

In a letter dated December 24, 1930 Bernays asks Gödel whether he can send him the proofs of the article on incompleteness, of which he got wind through Courant and Schur. Gödel replies on December 31 sending the proofs of the article. On January 18, 1931, Bernays replies saying that he has received the proofs on January 14. What comes next is very important for the issue of whether the ω-rule in Hilbert 1931 was introduced by Hilbert as a remedy to Gödel's incompleteness. Bernays writes:

> Your results have moreover a special topical interest for me that goes beyond their general significance, in that they cast light on an extension of the usual framework for number theory recently undertaken by Hilbert. (Bernays to Gödel, January 18, 1931, Vol. IV, p. 83)

Bernays specifies that by number theory he means first-order arithmetic. Then he states the extension proposed by Hilbert:

> Hilbert's extension now consists in the following rule: If $A(x_1, \ldots, x_n)$ is a *recursive* formula (according to your designation), which might be shown,

> finitarily, to yield a numerical identity for arbitrarily given numerical values $x_1 = z_1, x_2 = z_2, \ldots, x_n = z_n$, then the formula $(x_1)\ldots(x_n)A(x_1,\ldots,x_n)$ can be used as an initial formula (i.e., as an axiom). (Bernays to Gödel, January 18, 1931, Vol. IV, p. 83)

According to Bernays, Hilbert has shown that if $(x_1)\ldots(x_n)A(x_1,\ldots,x_n)$, with $A(x_1,\ldots,x_n)$ quantifier-free, can be shown to be consistent with number theory by means of finitistic considerations, then $(x_1)\ldots(x_n)A(x_1,\ldots,x_n)$ is provable in number theory augmented by the new rule. Bernays also claimed that the consistency of the new rule followed by the techniques for proving consistency known from the works of Ackermann and von Neumann (as shown by a student, R. Schmidt). Now Bernays draws his conclusion for what Gödel's results mean for Hilbert's program. Gödel has shown that there are (recursive) formulas $A(x_1,\ldots,x_n)$ such that all their numerical instances can be proved in number theory but number theory does not prove $(x_1)\ldots(x_n)A(x_1,\ldots,x_n)$. Thus, Bernays concludes, there are statements that are finitistically justifiable but unprovable in number theory. The consequence to be drawn, according to him, is that if a formal system can be finitistically shown to be consistent, then there is a finitistic sentence that cannot be finitistically justified in the formal system. By contrast, if one assumes with von Neumann that all the finitistic considerations are already included in number theory then one needs to conclude that a finitistic consistency proof of number theory is impossible:

> Thus if, as von Neumann does, one takes it as certain that any and every finitary consideration may be formalized within the framework of the system P [the system of Principia]—like you, I regard that in no way as settled—one comes to the conclusion that a finitary demonstration of the consistency of P is impossible. (Bernays to Gödel, January 18, 1931, Vol. IV, p. 87)

However, Bernays shows that he is unhappy with Hilbert's proposed extended system. He considers it inelegant to have both the axiom of induction and the new ω-rule. This leads him to propose a new rule, R, from which he can derive both the axiom of induction and Hilbert's new rule. R is formulated as follows:

> If $A(x_1\ldots,x_n)$ is a (*not necessarily recursive*) formula in which only $x_1,\ldots,x_n$ occur as free variables and which, through the substitution of any numerical values whatever in place of $x_1,\ldots,x_n$, is transformed into a formula such as is derivable from the formal axioms and the formulas already derived, the formula $(x_1)\ldots(x_n)A(x_1,\ldots,x_n)$ may be adjoined to the domain of the derived formulas. (Bernays to Gödel, January 18, 1931, Vol. IV, p. 89)

For this rule R, Bernays claims to have a sketch of a consistency proof along the lines of those given for number theory.

Notwithstanding Feferman's opinion to the contrary (see Vol. IV, p. 44, note 1), in my view the above exchange (and a detailed study of the chronology surrounding the publication of Hilbert's 1931 article on the ω-rule) is strong evidence that the ω-rule was not introduced by Hilbert as a reaction to Gödel's incompleteness theorems.[13] However, I do agree with Feferman that if one discards his preferred explanation then one needs to give an account of what might have led Hilbert to entertain the extension. At the moment no such account is on offer.

Incidentally, it is only in this correspondence with Gödel that Bernays begins worrying about the formalizability in number theory of an alleged finitistic demonstration of the consistency of number theory (see letters from Bernays to Gödel in April and May 1931; CW, Vol. IV, pp. 91–105). The demonstration in question is referred to by Hilbert in the 1928 paper "The foundation of mathematics" and is attributed to

Ackermann. This is not the 1924 proof contained in Ackermann's dissertation but rather a different proof by means of ϵ-substitution proposed by Ackermann in 1927 (see [25], p. 242). Bernays considers the proof correct and worries about which parts of the proof, in light of Gödel's theorem, cannot be formalized in Z (first-order number theory). He ends up singling out certain forms of nested recursion as the culprit. But this is somewhat puzzling because von Neumann in previous correspondence with Bernays had already pointed out that Ackermann's 1927 proof had a gap (see Zach [25], p. 243). Moreover, from our point of view it is surprising that Bernays thinks that nested recursions are not formalizable in Z, since they are. But evidently that was not clear to Bernays at the time and Bernays' position is understandable given that Gödel had only explicitly shown the primitive recursive functions to be be representable in Z.

7 The Philosophical Relevance of the Incompleteness Theorem according to Gödel

On November 1942 Gödel wrote to Schilpp that "the meddling of scientist[s] into philosophy has so often proved useful for both" (CW, Vol. V, p. 219). That is certainly the case for Gödel's own pronouncements on various philosophical topics. Concerning Gödel's evaluation of the lasting importance of the incompleteness theorems the correspondence confirms what was already published in Wang [21] and the claims made by Gödel in the Gibbs lecture (1951) now published in Volume III. I will however quote here an extremely nice and succinct formulation of how Gödel saw the philosophical consequences of his result for general philosophy and Hilbert's program in particular. In a letter to Leon Rappaport written on August 2, 1962 Gödel wrote:

> Nothing has been changed lately in my results or their philosophical consequences, but perhaps some misconceptions have been dispelled or weakened. My theorems only show that the *mechanization* of mathematics, i.e. the elimination of the *mind* and of *abstract* entities, is impossible, if one wants to have a satisfactory foundation and system of mathematics.
>
> I have not proved that there are mathematical questions undecidable for the human mind, but only that there is no *machine* (or *blind formalism*) that can decide all number theoretical questions (even of a very certain special kind).
>
> Likewise it does not follow from my theorems that there are no convincing *consistency* proofs for the usual mathematical formalisms, notwithstanding that such proofs must use modes of reasoning not contained in those formalism. What is practically certain[1] is that there are for the classical formalisms, no conclusive combinatorial consistency proofs (such as Hilbert expected to give), i.e. no consistency proofs that use only concepts referring to finite combinations of symbols and not referring to any infinite totality of such combinations. (Gödel to Rappaport, August 2, 1962, Vol.V, p. 176)

In note 1 Gödel specified: "No formal proof has yet been given because the concept of a combinatorial proof, although intuitively clear, has not yet been precisely defined."

This last quote could be the starting point for tracing in the correspondence Gödel's position(s) on the extent of finitism, the philosophical import of the consistency proof given by the Dialectica interpretation, his notion of abstract entity and so on. The reader will have to discover these and many other wonderful topics by going through the correspondence. The above was only meant to give a sample of the rewards to be expected by delving into the volumes.

8 Conclusion

The completion of the publication of Gödel's *Collected Works* marks an epochal moment in our appreciation of the career of one of the most brilliant minds of the twentieth century. The two volumes of the *Collected Works* containing the correspondence provide us with important and essential information about Gödel's life and intellectual achievements. But they also shed light on several important aspects of the history of logic and philosophy in the twentieth century. The correspondence refines and extends our knowledge of the technical and philosophical issues related to constructivism, proof theory, model theory, recursion theory, and set theory. Moreover, it provides information on other areas of Gödel's thought such as physics, general philosophy, and theology. Through it one can retrace the important debates and the profound thoughts that led to, and originated from, some of the most important logical results of the twentieth century. Thanks to the introductions by the editors (Dawson, Feferman, Goldfarb, Parsons, and Sieg) and those of the collaborators (Beeson, Fenstad, Kanamori, Linnebo, Machover, and Malament) the reader can immediately access the correspondence with the required historical, philosophical, and technical background. While I referred to some of the introductions in my review it is impossible to convey the full richness of content contained in them and the fact that these introductions are uniformly of superb quality and of the highest level of scholarship. The translations are accurate and readable at the same time. The editorial apparatus is precise without being daunting. The volumes are beautifully produced in TEX© and contain a rich collection of photographs. The two volumes are expensive but one hopes that OUP will soon bring out a paperback edition, as it has done for Volumes I – III.

I can only conclude by saying that the completion of the publication of Gödel's *Collected Works* is an extraordinary achievement of the highest intellectual importance.

Notes

1. See also Wang [24], Kreisel [15], Köhler et al. [14], and Feferman's "Gödel's life and work" in *Gödel*, [7], pp. 1–36.

2. Extracts from quite a few of Gödel's letters to his mother are published in Köhler et al. [14], Vol. 1, pp. 185–207 in a section entitled "Gödels Briefe an seine Mutter".

3. Perhaps the most extreme case here is Wilson Follett's outburst concerning the publication of "Gödel's Proof" by Nagel and Newman [18]. See Vol. IV, p. 419.

4. More precisely, Zermelo shared with Gödel the criticism of the "finitistisches Vorurteil" mathematically but he did not appreciate the metamathematical point of view. The Gödel-Zermelo correspondence had already been studied by Dawson [2] and Grattan-Guinness [12].

5. This letter to Balas was already discussed in Feferman [5], Wang [22], p. 654, and Wang [23], pp. 84–85.

6. It could be argued that Gödel did at times fall prey to what he called the prejudices of the time. See in particular what Gödel says about Platonism in Gödel *1933o and Feferman's introduction to that lecture in CW, Vol. III.

7. See also Gödel 1934, in CW, Vol. I, pp. 262–63, where he already states the undefinability of truth in the language and explains the relation of his proof of incompleteness to the Liar paradox. The relations to Tarski's work were footnoted later, for its reproduction in Davis [1].

8. The correspondence between Gödel and Herbrand had already been discussed in Dawson [3] and Sieg [20].

9. Interesting passages from the Gödel-von Neumann correspondence were already used in Köhler [13].

10. Sieg points out that Gödel's change of mind can be dated to 1933; see CW, Vol. V, p. 8.

11. Von Neumann repeats his position in a letter to Carnap dated June 6, 1931:

    I am today of the opinion that

    1. Gödel has shown the unrealizability of Hilbert's program
    2. There is no more reason to reject intuitionism (if one disregards the aesthetic issue, which in practice will also for me be the decisive factor) Therefore I consider the state of the foundational discussion in Königsberg to be outdated, for Gödel's fundamental discoveries have brought the question to a completely different level. (I know that Gödel is much more careful in the evaluation of his results, but in my opinion on this point he does not see the connections correctly).

    (For the full original text see Mancosu [17].)

12. Sieg (CW, Vol. V, p. 332) points out that what Gödel found problematic here was "the claim that the totality of all intuitionistically correct proofs is contained in one formal system" and refers to the minutes kept by Rose Rand of the discussion following Gödel's presentation of his results to the Schlick Circle (see also Mancosu [17] for the full text of the discussion).

13. This is not the place for providing all the evidence required to make my case. I simply want to point out that more work needs to be done to settle the issue.

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