THE BULLETIN OF SYMBOLIC LOGIC
Volume 11, Number 2, June 2005

ONLY TWO LETTERS:
THE CORRESPONDENCE BETWEEN HERBRAND AND GÖDEL

WILFRIED SIEG

Abstract. Two young logicians, whose work had a dramatic impact on the direction of logic, exchanged two letters in early 1931. Jacques Herbrand initiated the correspondence on 7 April and Kurt Gödel responded on 25 July, just two days before Herbrand died in a mountaineering accident at La Bérarde (Isère).[1] Herbrand's letter played a significant role in the development of computability theory. Gödel asserted in his 1934 Princeton Lectures and on later occasions that it suggested to him a crucial part of the definition of a general recursive function. Understanding this role in detail is of great interest as the notion is absolutely central. The full text of the letter had not been available until recently, and its content (as reported by Gödel) was not in accord with Herbrand's contemporaneous published work. Together, the letters reflect broader intellectual currents of the time: they are intimately linked to the discussion of the incompleteness theorems and their potential impact on Hilbert's Program.

Introduction. Two important papers in mathematical logic were published in 1931, one by Jacques Herbrand in the ***Journal für reine und angewandte Mathematik*** and the other by Kurt Gödel in the ***Monatshefte für Mathematik und Physik***. At age 25, Gödel was Herbrand's elder by just two years. Their work dramatically impacted investigations in mathematical logic, but also became central for theoretical computer science as that subject evolved in the fifties and sixties. The specific techniques of resolution and unification derive from ideas in Herbrand's work, whereas the very notion of computability in the form of general recursiveness was introduced in Gödel's work three years later, with reference to Herbrand.

Herbrand's 1931-paper established the consistency of a fragment of arithmetic by elementary meta-mathematical means. These means were chosen to be "finitist" in the spirit of Hilbert's Program, which Herbrand was pursuing. The program aimed to secure or guarantee the internal coherence of modern mathematics. Finitist consistency proofs for formal theories were the means to that end and Herbrand's were the most far-reaching that had

Submitted November 28, 2004; accepted November 30, 2004.

[1]As Gödel sent his letter to the Paris address of Herbrand's parents, it is almost certain that Herbrand never read the letter.

been obtained at the time. Gödel's 1931-paper, in contrast, showed that sufficiently strong formal theories, even for arithmetic, have two general features: they are syntactically incomplete, and they cannot prove their own consistency. The first fact is the First Incompleteness Theorem, the second the Second. The Second Theorem points to limits of Hilbert's consistency program, whereas the First shows that the totality of arithmetic truths cannot be captured in formal theories.

Related to these papers are two letters, which were exchanged between Herbrand and Gödel in early 1931. The letters are linked to a wider discussion on the foundations of mathematics that involved leading mathematicians, logicians, and philosophers, for example Johann von Neumann, Hilbert's collaborator Paul Bernays, and members of the Vienna Circle such as Rudolf Carnap. The letters throw a distinctive light on this discussion, as Herbrand and Gödel focus in a very open, non-ideological way on two central issues: (i) the extent of finitist or, at the time synonymously, intuitionist methods and (ii) the effect of the incompleteness theorems on Hilbert's Program. For contemporary readers of the letters there is a third issue: Gödel remarked in his 1934 Princeton Lectures and at later occasions that Herbrand had suggested to him a central part of the definition of general recursiveness in "a private communication." The conceptually fascinating question is, (iii) what did Herbrand really suggest, and how did his suggestion affect Gödel's definition?

There is a bit of mystery surrounding this private communication. Jean van Heijenoort queried Gödel in 1963 about his remark, in part because there was a discrepancy between Gödel's report on Herbrand's suggestion and Herbrand's published remarks on related issues. Gödel responded that the suggestion had been communicated to him in a letter of 1931, and that Herbrand had made it in *exactly the form* in which his lecture notes presented it. But Gödel was unable to find Herbrand's letter among his papers. John Dawson discovered the letter in the Gödel Nachlass in 1986, and it became clear that Gödel had misremembered.

It is often the case that particular documents reflect broader intellectual currents, and that the analysis of such documents reveals central aspects vividly and in novel ways. This observation certainly holds for these letters. The broader intellectual currents will be sketched in Part 1, which is entitled *Immediate Context: Incompleteness*. Part 2 presents *Herbrand's Issue* and is followed in Part 3 by *Gödel's Response*(*s*). Part 4 looks at the *Future Impact: Computability*. I try to draw an informative vignette that is illuminated by a rich past and radiates into a complex future.

1. Immediate Context: Incompleteness. The sketch of the context has the structure of concentric spheres with the letters at their center. The first sphere reflects Hilbert's proof theory that began to be pursued in a programmatically coherent form in 1922. Consistency proofs were to be given for formal theories, in which mathematics can be developed, and the proofs were to use only finitist means. The development of proof theory was embedded in the loud foundational dispute of the 1920s between Hilbert's "Finitism," Brouwer's "Intuitionism," and the "Logicism" that had been inspired by the investigations of Frege, Russell, and Whitehead; this is the second sphere. The third sphere represents the substance of the foundational dispute and reflects the intellectual tensions between Dedekind and Kronecker, which are related to the emergence of set theory in the second half of the 19th century. This emergence, in turn, is connected to a new systematic self-understanding of mathematics and a thorough reexamination of its role in the sciences; one can correctly speak of a transformation of classical mathematics into a new subject of axiomatically formulated abstract theories.

The three outermost spheres can and will remain in the background, whereas the innermost one has to be described more thoroughly. Before doing that, I want to make one additional remark. Hilbert appears in all the spheres; he defends vigorously the modern conception of mathematics and yet tries to mediate the Kronecker-Dedekind tensions by his consistency program. In lectures and publications from 1922 and 1923, he established the consistency of an elementary part of arithmetic. Ackermann and von Neumann extended this result in 1924/25 but difficulties were encountered when it was attempted to extend results further. These difficulties were first thought to be of a "technical" mathematical sort, but instead were revealed by the incompleteness theorems as "conceptual" philosophical ones.

Hilbert had initiated not only proof theoretic investigations, but also broader meta-mathematical studies of logic in lectures as early as the winter term of 1917/18. One interesting result, obtained in collaboration with Bernays, was the semantic completeness of sentential logic. In his Bologna talk of 1928, Hilbert posed the semantic completeness issue for full first-order logic as one in a list of problems. Gödel solved it positively in his doctoral dissertation of 1929. Another problem, also formulated by Hilbert in Bologna, concerned the syntactic completeness of first-order arithmetic and Hilbert expected a positive result here as well. However, Gödel obtained a negative result in the summer of 1930. He reported it in late August to friends in Vienna and a couple of weeks later, briefly and understatedly, during a roundtable discussion at a conference in Königsberg. That's where Gödel's and Herbrand's paths were indirectly linked.

Herbrand, too, was deeply influenced by Hilbert's foundational enterprise and wrote a thesis entitled *Recherches sur la théorie de la démonstration*, which he defended on 11 June 1930. Its important main result is with us as *Herbrand's Theorem*. Herbrand had developed strong interests in modern algebra, which was flourishing in Germany; he actually spent the academic year 1930/31 there on a Rockefeller Scholarship (and intended to spend the next year at Princeton University). In his final report to the Rockefeller Foundation he wrote that his stay in Germany extended from 20 October 1930 to the end of July 1931. Until the middle of May 1931 he was in Berlin, then spent a month in Hamburg and the remaining time in Göttingen. In these three cities he worked mainly with von Neumann, Artin, and Emmy Noether. Concerning his stay in Berlin he went on to say: "In Berlin I have worked in particular with Mr. von Neumann on questions in mathematical logic, and my research in that subject will be presented in a paper to be published soon in the ***Journal für reine und angewandte Mathematik***." The paper he alluded to is his 1931-paper, in which he compared, as his friend Claude Chevalley put it, his own results with those of Gödel, i.e., the incompleteness theorems. He had learned of the first theorem from von Neumann shortly after his arrival in Berlin. In a letter of 3 December 1930, he wrote to Chevalley:

> The mathematicians are a very strange bunch; during the last two weeks, whenever I have seen von Neumann, we have been talking about a paper by a certain Gödel, who has produced very curious functions; and all of this destroys some solidly anchored ideas.

This sentence opens the letter. Having sketched Gödel's argument and reflected on the result, Herbrand concluded the logical part of his letter by: "Excuse this long beginning; but all of this has been haunting me, and by writing about it I exorcise it a little."

How did von Neumann, in November of 1930, know of a result that was to be published only in 1931? I alluded to an answer when I mentioned that Gödel reported on his first incompleteness theorem at the ***Second Conference for Epistemology of the Exact Sciences*** held from 5 to 7 September 1930 in Königsberg. On the very last day of the conference, a roundtable discussion on the foundations of mathematics took place to which Gödel had been invited. Hans Hahn, Gödel's dissertation advisor, chaired the discussion and its participants included Carnap, Heyting, and von Neumann. Toward the end of the discussion, Gödel made brief remarks about the first incompleteness theorem. This is the background for a more personal encounter with von Neumann in Königsberg; Wang reported Gödel's view about this encounter in his *1981*:

> Von Neumann was very enthusiastic about the result and had a private discussion with Gödel. In this discussion, von Neumann asked whether number-theoretical undecidable propositions could also be constructed in view of the fact that the combinatorial objects can be mapped onto the integers and expressed the belief that it could be done. In reply, Gödel said, "Of course undecidable propositions about integers could be so constructed, but they would contain concepts quite different from those occurring in number theory like addition and multiplication." Shortly afterward Gödel, to his own astonishment, succeeded in turning

> the undecidable proposition into a polynomial form preceded by quantifiers (over natural numbers). At the same time but independently of this result, Gödel also discovered his second theorem to the effect that no consistency proof of a reasonably rich system can be formalized in the system itself.[2]

This makes clear that Gödel did not yet have the second incompleteness theorem at the time of the Königsberg meeting; on 23 October 1930 Hahn presented, however, an abstract containing its classical formulation to the Vienna Academy of Sciences. The full text of Gödel's 1931-paper was submitted to the editors of ***Monatshefte*** on 17 November 1930. Three days later, von Neumann wrote to Gödel and characterized Gödel's first result as "the greatest logical discovery in a long time." He went on to sketch a proof of the second incompleteness theorem, at which he had arrived independently of Gödel. Gödel responded almost immediately, and von Neumann assured him in his next letter that he would not publish on the subject "as you have established the theorem on the unprovability of consistency as a natural continuation and deepening of your earlier results." However, there emerged a disagreement between Gödel and von Neumann on how this theorem affects Hilbert's finitist program.

2. Herbrand's issue. Gödel insisted in his paper that the second incompleteness theorem does not contradict Hilbert's "formalist 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 (or of M and A).[3]

Having received the galleys of Gödel's paper, von Neumann writes in a letter of 12 January 1931:

> I absolutely disagree with your view on the formalizability of intuitionism. Certainly, for every formal system there is, as you proved, another formal one that is (already in arithmetic and the lower functional calculus) stronger. But that does not affect intuitionism at all.

Denoting first order number theory by A, analysis by M, and set theory by Z, von Neumann continues:

> Clearly, I cannot prove that every intuitionistically correct construction of arithmetic is formalizable in A or M or even in Z—for

[2] *Wang 1981*, pp. 654–5. Parsons' *Introductory Note* to the correspondence with Wang in Gödel's ***Collected Works*** vol. V describes in section 3.2 the interaction between Gödel and Wang on which this paper is based.

[3] *Gödel 1931*, p. 197; in ***Collected Works***, vol. I, p. 195. P is the version of the system of ***Principia Mathematica*** in Gödel's 1931 paper, M is the system of set theory introduced by von Neumann, and A is classical analysis.

> intuitionism is undefined and undefinable. But is it not a fact, that not a single construction of the kind mentioned 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 utmost surprise, should be the case, then the formalization should work in M or Z!

Herbrand had sharpened this line of argument by the time he wrote to Gödel on 7 April 1931. In the meantime he had discussed the incompleteness phenomena extensively with von Neumann, and he had read the galleys of *Gödel 1931*, which Bernays had given to him. On that very day, April 7, he also sent a note to Bernays and enclosed a copy of his letter to Gödel. In the note he first contrasts his consistency proof with that of Ackermann, which he attributes mistakenly to Bernays:

> In my arithmetic the axiom of complete induction is restricted, but one may use a variety of other functions than those that are defined by simple recursion: in this direction, it seems to me, my theorem goes a little farther than yours.[4]

He then formulates the central issue to Bernays as follows: "I also try to show in this letter how your results can agree with these of Gödle [sic]." This information puts Herbrand's remark to Hadamard (made in early 1931) into sharper focus.

> Recent results (not mine) show that we can hardly go any further: it has been shown that the problem of consistency of a theory containing all of arithmetic (for example, classical analysis) is a problem whose solution is impossible. In fact, I am at the present time preparing an article in which I will explain the relationships between these results and mine.

It is quite clear that Herbrand's attempt to analyze the relationship between Gödel's theorems and ongoing proof theoretic work, including his own, prompted the specific details in his letter to Gödel as well as in his paper.

At issue is the extent of finitist or, synonymously for Herbrand, intuitionist methods and thus the reach of Hilbert's consistency program. Herbrand's letter has to be understood (and Gödel in his response quite clearly did) as giving a sustained argument against Gödel's assertion that the second incompleteness theorem does not contradict Hilbert's "formalist viewpoint." Herbrand introduces a number of systems for arithmetic, all containing the axioms for predicate logic with identity and the Dedekind-Peano axioms

[4]Bernays, in his letter to Gödel of 20 April 1931, pointed out that Herbrand had misunderstood him in an earlier discussion: he, Bernays, had not talked about a result of his, but rather about Ackermann's consistency proof.

for zero and successor. The systems are distinguished by the strength of the induction principle and by the class F of finitist functions for which recursion equations are available. The system with induction for all formulas and recursion equations for the functions in F is denoted here by **F**; if induction is restricted to quantifier-free formulas, I denote the resulting system by **F***. The axioms for the elements $f_1, f_2, f_3, \ldots$ in F must satisfy according to Herbrand the following conditions:

> (1) The defining axioms for f_n contain, besides f_n, only functions of lesser index.
> (2) These axioms contain only constants and free variables.
> (3) We must be able to show, by means of intuitionistic proofs, that with these axioms it is possible to compute the value of the functions univocally for each specified system of values of their arguments.

As examples for classes F, Herbrand considers the set E_1 of addition and multiplication, as well as the set E_2 of all primitive recursive functions. He asserts that many other functions are definable by his "general schema," in particular, the non-primitive recursive Ackermann function. He also argues that one can construct by diagonalization a finitist function that is not in E, if E satisfies axioms such that "one can always determine, whether or not certain defining axioms are among these axioms."

This fact of the open-endedness of any finitist presentation of the concept "finitist function" is crucial for Herbrand's conjecture that one cannot prove that all finitist methods are formalizable in ***Principia Mathematica***. But he claims that, as a matter of fact, every finitist proof can be formalized in a system of the form **F*** with a suitable class F that depends on the given proof and, thus, also in ***Principia Mathematica***. Conversely, he insists that every proof in the quantifier-free part of **F*** is finitist. He summarizes his reflections by saying in the letter and with almost identical words in *1931*:

> It reinforces my conviction that it is impossible to prove that every intuitionistic proof is formalizable in Russell's system, but that a counterexample will never be found. There we shall perhaps be compelled to adopt a kind of logical postulate.

What is the direct consequence of the second incompleteness theorem?—The reader may recall that, under general conditions on a theory **T**, **T** proves the conditional ($\mathrm{con}_{\mathbf{T}} \rightarrow \mathrm{G}$); $\mathrm{con}_{\mathbf{T}}$ is the statement expressing the consistency of **T**, and G is the Gödel sentence.[5] G states its own unprovability and is, by the first incompleteness theorem, not provable in **T**. Consequently, G would be provable in **T**, as soon as a finitist consistency proof for **T** could

[5]The general conditions on **T** include, of course, the representability conditions for the first theorem and the Hilbert-Bernays derivability conditions for the second theorem.

be formalized in **T**. That's why the issue of the formalizability of finitist considerations plays such an important role in this discussion.

3. Gödel's response(s). Herbrand's conjectures and claims are much more detailed than those von Neumann communicated to Gödel in his letters of November 1930 and January 1931. We know of Gödel's response to von Neumann's dicta not through letters from Gödel, but rather through the minutes of a meeting of the Schlick or Vienna Circle that took place on 15 January 1931. According to these minutes, Gödel viewed as questionable the claim that the totality of all intuitionistically correct proofs is contained in one formal system. That, he emphasized, is the weak spot in von Neumann's argumentation.[6]

When answering Herbrand's letter, Gödel makes more explicit his reasons for questioning the formalizability of finitist considerations in a single formal system like ***Principia Mathematica***. He agrees with Herbrand on the indefinability of the concept "finitist proof." However, even if one accepts Herbrand's very schematic presentation of finitist methods and the claim that every finitist proof can be formalized in a system of the form **F***, the question remains "whether the intuitionistic proofs that are required in each case to justify the unicity of the recursion axioms are all formalizable in ***Principia Mathematica***." He continues:

> Clearly, I do not claim either that it is certain that some finitist proofs are not formalizable in ***Principia Mathematica***, even though intuitively I tend toward this assumption. In any case, a finitist proof not formalizable in ***Principia Mathematica*** would have to be quite extraordinarily complicated, and on this purely practical ground there is very little prospect of finding one; but that, in my opinion, does not alter anything about the possibility in principle.

At this point, there is a stalemate between Herbrand's "logical postulate" that no finitist proof outside of ***Principia Mathematica*** will be found, and Gödel's "possibility in principle" that one might find such a proof.

By late December 1933 when he gave an invited lecture to the Mathematical Association of America in Cambridge (Massachusetts), Gödel had changed his views significantly. In the text for his lecture, *Gödel 1933*, he sharply distinguishes intuitionist from finitist arguments, the latter constituting the most restrictive form of constructive mathematics. He insists that the known finitist arguments given by "Hilbert and his disciples" can all be carried out in a certain system **A**.[7] Proofs in **A**, he asserts, "can be easily expressed in the

[6]Gödel did respond to von Neumann, but his letters seem to have been lost. The minutes are found in the Carnap Archives of the University of Pittsburgh.

[7]The restrictive characteristics of the system A are formulated on pp. 23 and 24 of *1933*: and include the requirement that notions have to be decidable and functions must be calculable. Gödel claims, that "such notions and functions can always be defined by complete induction."

system of classical analysis and even in the system of classical arithmetic, and there are reasons for believing that this will hold for any proof which one will ever be able to construct." This observation and the second incompleteness theorem imply, as sketched above, that classical arithmetic cannot be shown to be consistent by finitist means. (The system **A** is similar to the quantifier-free part of Herbrand's system **F***, except that the provable totality for functions in F is not mentioned. Gödel's reasons for conjecturing that **A** contains all finitist arguments are not made explicit.)

Gödel discusses then a theorem of Herbrand's, which he considers to be the most far-reaching among interesting partial results in the pursuit of Hilbert's consistency program. He does so, as if to answer the question "How do current consistency proofs fare?" and formulates the theorem in this lucid and elegant way: "If we take a theory which is constructive in the sense that each existence assertion made in the axioms is covered by a construction, and if we add to this theory the non-constructive notion of existence and all the logical rules concerning it, e.g., the law of excluded middle, we shall never get into any contradiction." The proof theoretic result mentioned in Herbrand's letter can be understood in just this way and foreshadows, of course, the central result of Herbrand's *1931*. Gödel conjectures that Herbrand's method might be generalized, but emphasizes that "for larger systems containing the whole of arithmetic or analysis the situation is hopeless if you insist upon giving your proof for freedom from contradiction by means of the system **A**." As the system **A** is essentially the quantifier-free part of **F***, it is clear that Gödel now takes Herbrand's position concerning the impact of the second theorem on Hilbert's Program.

Nowhere in the correspondence does the issue of *general* computability arise. Herbrand's discussion, in particular, is solely trying to explore the limits of consistency proofs that are imposed by the second theorem. Gödel's response focuses also on that very topic. It seems that he subsequently developed a more critical perspective on the very character and generality of his theorems.[8] This perspective allowed him to see a crucial open question and to consider Herbrand's notion of a finitist function as a first step towards an answer.

4. Future Impact: Computability. The crucial open question that remained in Gödel's mind was this: For which formal theories do the incompleteness theorems hold? Just for the systems **PM**, **ZF**, and "related systems"? What

Definition by complete induction is to be understood as definition by recursion, which is by no means restricted to primitive recursion. That is made explicit in section 9 of Gödel's *1934*, where "an example of a definition by induction with respect to two variables simultaneously" is discussed; an example that defines a function "that is not in general recursive in the limited sense of §2," i.e., not primitive recursive.

[8]How much the interaction with Church in 1933/34 contributed to this perspective, we can only speculate; see my paper *Sieg 1997*.

is the extension of "related system"? For a fully satisfactory answer one needs a general and rigorous definition of "formal theories." Gödel points to their central features in §1 of his Princeton Lectures by saying that the rules of inference and the notions of formula and axiom have to be given constructively, i.e.,

> for each rule of inference there shall be a finite procedure for determining whether a given formula B is an immediate consequence (by that rule) of given formulas $A_1, \ldots, A_n$, and there shall be a finite procedure for determining whether a given formula A is a meaningful formula or an axiom.[9]

To a similar discussion of formal theories in the Cambridge Lecture he added the remark that the rules of inference are purely formal, i.e., "refer only to the outward structure of the formulas, not to their meaning, so that they could be applied by someone who knew nothing about mathematics, or by a machine."[10]

Gödel strove in his Princeton Lectures to make his results less dependent on particular formalisms. That is indicated even by their title *On undecidable propositions of formal mathematical systems*. He used, as he had done in his *1931*, primitive recursive functions and relations to present syntax, viewing the primitive recursive definability of formulas and proofs as a "precise condition, which in practice suffices as a substitute for the unprecise requirement of §1 that the class of axioms and the relation of immediate consequence be constructive." A notion that would suffice *in principle* was needed, however, and Gödel attempted to arrive at a more general notion.

In his subsequent reflections, Gödel focused on the "computability" of number theoretic functions. He considers the fact that the value of a primitive recursive function can be computed by a finite procedure for each set of arguments as an "important property" and adds in note 3:

> The converse seems to be true if, besides recursions according to the scheme (2) [i.e., primitive recursion as given above], recursions of other forms (e.g., with respect to two variables simultaneously) are admitted. This cannot be proved, since the notion of finite computation is not defined, but it can serve as a heuristic principle.[11]

What other recursions might be admitted is discussed in the last section of the Notes under the heading "general recursive functions." Gödel describes in it the proposal for the definition of a general notion of recursive function that (he thought) had been suggested to him by Herbrand:

---

[9] *Gödel 1934*, p. 346.

[10] *Gödel 1933*, p. 45.

[11] Gödel emphatically rejected in the sixties (in a letter to Martin Davis) that this formulation anticipates a form of Church's Thesis: he was not convinced that his notion of recursion was the most general one.

> If ϕ denotes an unknown function, and $\psi_1, \ldots, \psi_k$ are known functions, and if the ψ's and ϕ are substituted in one another in the most general fashions and certain pairs of resulting expressions are equated, then, if the resulting set of functional equations has one and only one solution for ϕ, ϕ is a recursive function.

Gödel went on to make two restrictions on this definition. He required, first of all, that the left-hand sides of the equations be in a standard form with ϕ as the outermost symbol and, secondly, that "for each set of natural numbers $k_1, \ldots, k_n$ there shall be exactly one and only one m such that $\phi(k_1, \ldots, k_n) = m$ is a derived equation." The rules that were allowed in derivations are simple substitution and replacement rules.

We should distinguish with Gödel two novel features in this definition: first, the precise specification of *mechanical* rules for deriving equations, i.e., for carrying out numerical computations; second, the formulation of the *regularity condition* requiring computable functions to be total, but without insisting on a (finitist) proof. In his letter to van Heijenoort of 14 August 1964, Gödel asserts, "it was exactly by specifying the rules of computation that a mathematically workable and fruitful concept was obtained." When making this claim Gödel took for granted that Herbrand's suggestion had been "formulated *exactly* as on page 26 of my lecture notes, i.e., without reference to computability." As was noticed earlier, Gödel had to rely on his recollection, which, he said, "is very distinct and was still very fresh in 1934." On the evidence of Herbrand's letter, it is clear that Gödel misremembered. This is not to suggest that Gödel was wrong in viewing the specification of computation rules as extremely important, but rather to point to the absolutely crucial step he had taken, namely, to disassociate general recursive functions from the epistemologically restricted notion of intuitionist proof in Herbrand's sense.

Later on, Gödel dropped the regularity condition altogether and emphasized, "that the precise notion of mechanical procedures is brought out clearly by Turing machines producing partial rather than general recursive functions." At this earlier historical juncture the introduction of the equational calculus with particular computation rules was important for the mathematical development of recursion theory as well as for the underlying conceptual motivation. It brought out clearly, what Herbrand—according to Gödel in his letter to van Heijenoort—had failed to see, namely "that the computation (for all computable functions) proceeds by exactly the same rules." Gödel was right, for stronger reasons than he put forward, when he cautioned in the same letter that Herbrand had *foreshadowed*, but not *introduced*, the notion of a general recursive function.[12]

[12] Van Heijenoort analyzed Gödel's differing descriptions of Herbrand's published proposals and the suggestion that had been made to him in the "private communication." References to this work and a discussion in light of the actual letter are found in my paper *Sieg 1994*; see in particular section 2.2 and the Appendix.

Concluding remarks. What impact the introduction of the notion of general recursive function had on the development of computability theory is an equally fascinating story, which leads to a very satisfying conceptual analysis; the issue of "what precisely is finitism" is by contrast still open. The former issue was not obtained along Gödelian lines by generalizing recursions, but by a quite different approach due to Alan Turing and, to some extent, Emil Post. They focused on symbolic processes underlying numerical computations instead of those computations themselves. This led to the foundations of theoretical and, via Turing's universal machine, also of practical computer science. Consequently, those foundations emerged from what were, at the time, quite obscure quasi-philosophical issues.

The general moral is, of course, that broad foundational questions can inspire concrete mathematical work, and that concrete mathematical work can call for philosophical analysis. There can be an extremely fruitful, but also subtle and delicate interplay between wide-open conceptual reflections and hard-nosed technical investigations. All of this is necessary for arriving at balanced positions. The historical evolution of the particular issues at hand confirms this and helps us to grasp their complexity. We see, finally, three specific and important points drawn from that evolution, listed in order of their increasing significance: (i) the Gödel-Herbrand notion of general recursive function is really Gödel's; (ii) in the early 1930s finitist mathematics was viewed as going significantly beyond primitive recursive arithmetic; (iii) at that time, finitist mathematics was viewed as coextensive with intuitionist mathematics. Each point is counter to broadly held contemporary views and, indeed, undermines deeply held convictions concerning our logical past.[13]

REFERENCES

[1929] K. GÖDEL, ***Über die Vollständigkeit des Logikkalküls***, doctoral dissertation, University of Vienna, 1929.

[1930] ———, *Einige metamathematische Resultate über Entscheidungsdefinitheit und Widerspruchsfreiheit*, ***Anzeiger der Akademie der Wissenschaften in Wien***, vol. 67 (1930), pp. 214–5.

[1931] ———, *Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I*, ***Monatshefte für Mathematik und Physik***, vol. 38 (1931), pp. 173–98.

---

[13]This essay is based on the Introductory Notes, which I wrote for the correspondence between Gödel and Herbrand, respectively von Neumann, and which are published in volume V of Gödel's ***Collected Works***. Versions were presented at Haverford College (in October 2002), at the Institute for Advanced Study at the University of Bologna (in November 2003), and at the Special Session on Gödel at the Annual Meeting of the Association for Symbolic Logic at Carnegie Mellon University (in May 2004). I am grateful to Jeremy Avigad, John Dawson, Solomon Feferman, Rossella Lupacchini and William W. Tait for perceptive suggestions, which helped to improve the paper.

[1933] ———, *The present situation in the foundations of mathematics*, ***Collected Works***, vol. III, (Oxford University Press, 1995), 1933, pp. 45–53.

[1934] ———, *On undecidable propositions of formal mathematical systems*, ***Collected Works***, vol. I, (Oxford University Press, 1986), 1934, (mimeographed lecture notes, taken by Stephen C. Kleene and J. Barkley Rosser), pp. 346–69.

[1930] J. HERBRAND, ***Recherches sur la théorie de la démonstration***, doctoral dissertation, University of Paris, 1930.

[1931] ———, *Sur la non-contradiction de l'arithmétique*, ***Journal für die reine und angewandte Mathematik***, vol. 166 (1931), pp. 1–8.

[1929] D. HILBERT, *Probleme der Grundlegung der Mathematik*, ***Atti del Congresso internazionale dei matematici, Bologna 3–10 settembre 1928***, vol. 1, 1929, pp. 135–41.

[1994] W. SIEG, *Mechanical procedures and mathematical experience*, ***Mathematics and mind*** (A. George, editor), Oxford University Press, 1994, pp. 71–117.

[1997] ———, *Step by recursive step: Church's analysis of effective calculability*, this BULLETIN, vol. 3 (1997), pp. 154–80.

[1981] H. WANG, *Some facts about Kurt Gödel*, ***The Journal of Symbolic Logic***, vol. 46 (1981), pp. 653–9.

CARNEGIE MELLON UNIVERSITY
PITTSBURGH, PA 15213, USA
E-mail: ws15+@andrew.cmu.edu