Four Introductory Notes

To the Correspondence of Kurt Gödel with

R. Büchi, J. Herbrand, E.Post, and J. von Neumann

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Introductory Note to the Gödel-Büchi Correspondence

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Julius Richard Büchi was born in Porto Allegre, Brazil, on 31 January 1924 to Swiss parents and as a citizen of Zell, Switzerland.[1] He grew up in Switzerland and in 1948 received a doctoral degree in mathematics from the Eidgenössische Technische Hochschule in Zurich; his thesis supervisor was Paul Bernays. After graduation he moved almost immediately to the United States and had a number of academic appointments, among others at the University of Michigan, Ann Arbor. In 1963 he became Professor of Mathematics and Computer Science at Purdue University and retained that position until his death in 1984. Büchi did important work in mathematical logic and, relatedly, theoretical computer science. Dirk Siefkes states in his *1985* that Büchi is "probably best known for using finite automata as combinatorial devices to obtain strong results on decidability and definability in monadic second-order theories and extending the method to infinite combinatorial tools".[2] Büchi's papers were collected by Mac Lane and Siefkes in *Büchi 1990*; his posthumously published book *Finite Automata, their Algebras and Grammars* was edited by Siefkes.

Two letters were exchanged between Gödel and Büchi in November 1957; they throw some additional light on (Gödel's views of) Herbrand's role in the development of the notion of recursiveness. In his Princeton lectures *1934* Gödel presented a general notion of recursiveness, where the individual functions arise simply as unique solutions of systems of equations:

> 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.[3]

Gödel asserted in his lectures that Herbrand had suggested this definition to him in a private communication. Büchi calls this notion *recursive (1)*. It is to be contrasted with the concept *general recursive* that Gödel obtained from it by

[1] More detailed information concerning Büchi's life and work can be found in *S. Büchi 1990*, *Siefkes e.a. 1984*, and *Siefkes 1985*. - As to the logical and historical issues raised by Büchi, cf. the (Introductory Note to the) correspondence between Gödel and Herbrand.

[2] *Siefkes 1985*, p. 7.

restricting the form of equations and by specifying elementary replacement rules to be used in calculating the value of functions. Büchi reports that he did not find the definition of *recursive (1)* in any of Herbrand's papers; he did find, however, in Herbrand's *1931c* "a definition which comes much closer to your definition [[of general recursive]] of 1934." The definition Büchi alludes to allows the introduction of functions f_i of n_i arguments into Herbrand's system of arithmetic together with hypotheses (i.e., defining equations) such that, as Herbrand requires there,

(a) The hypotheses contain no apparent variables;

(b) Considered intuitionistically, they [[the hypotheses]] make the actual computation of the $f_i(x_i, \ldots, x_{ni})$ possible for every given set of numbers, and it is possible to prove intuitionistically that we obtain a well-determined result.[4]

In a footnote to the first occurrence of "intuitionistically" Herbrand explains that this expression means, "when they [[the hypotheses]] are translated into ordinary language, considered as a property of integers and not as mere symbols."

Büchi asks two questions concerning the notion *recursive (1)*: (a) Was the definition actually suggested by Herbrand or did Gödel refer to *1931c*? and (b) Is it known that this notion is much weaker than general recursive? As to the substantive mathematical issue underlying question (b) Büchi had obtained results, in particular, that there are *recursive (1)* predicates that are not general recursive, indeed not even arithmetical. Gödel refers to *Kalmar 1955* for an affirmative answer to (b).[5] Concerning the historical question Gödel reasserts that Herbrand communicated the definition of *recursive (1)* to him in a letter. The definition in Herbrand's *1931c*, Gödel says, "means nothing else but *demonstrably recursive (1)*", where the demonstrations have to be intuitionistic. The actual computation is not, according to Gödel, to proceed according to formal rules, but

[3] *Gödel 1934*, these *Works*, vol. I, p. 368.

[4] *Herbrand 1931c*, pp. 290-1.

[5] The Theorem established by Kalmar (on p. 94) is slightly weaker than Büchi's and states that there is a system of equations with a unique solution ϕ such that ϕ is arithmetically definable but not general recursive.

rather by "any kind of intuitionistic reasoning". "Therefore," he continues, "it is a priori possible that also the non-recursive functions which you mention in your letter might be recursive in this sense." For a further discussion of Gödel's analysis of Herbrand's notion(s) and van Heijenoort's refinements, see the *Introductory Note* to the correspondence with Herbrand, these *Works*, and the literature mentioned there.

References (only new):

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Introductory Note to the Gödel-Herbrand Correspondence

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The correspondence between Kurt Gödel and Jacques Herbrand consists of two remarkable letters that are focused on two fundamental issues, namely, the extent of finitist methods and the effect of Gödel's incompleteness theorems on Hilbert's consistency program. Gödel and Herbrand expressed sharply contrasting views on the latter issue. The correspondence is also intimately linked to a wider discussion of these theorems that involved most directly Johann von Neumann, Paul Bernays, and members of the Vienna Circle. Characterizing the extent of finitist methods is for Herbrand very much a matter of circumscribing the extent of the concept of finitist function.[1] The historically and conceptually fascinating question is, what effect did Herbrand's discussion of finitist functions have on the definition of general recursive functions as given in *Gödel 1934*? Gödel remarked in note 34 of his *1934* that a central part had been suggested by Herbrand in private communication. When queried about this remark by Jean van Heijenoort in a letter of 25 March 1963, Gödel responded on 23 April 1963 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 *1934* presented it. But Gödel was unable to find the letter among his papers.[2] John Dawson discovered the letter in the Gödel Nachlass in 1986, and it became clear that Gödel had misremembered a crucial feature of Herbrand's discussion.[3]

Herbrand was born in Paris on 12 February 1908. At the age of only twenty-three, he died in a mountaineering accident at La Bérarde (Isère) on 27 July 1931.[4] He defended his doctoral thesis *Recherches sur la théorie de la démonstration* on 11 June 1930, spent the academic year 1930/31 in Germany on a Rockefeller Scholarship, and intended to go for the next academic year to Princeton University. In his report to the Rockefeller Foundation he wrote that his stay in Germany extended from 20 October 1930 to the end of July 1931: until

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[1] I take it that Herbrand used "intuitionist" as synonymous with "finitist"; cf. also *Herbrand 1931a*. On pp. 116-8 of his *1985*, van Heijenoort, following Gödel's lead, examines very carefully the possibility of giving "intuitionist" in Herbrand's work a broader interpretation than "finitist". The outcome is inconclusive at best. In my view, the examination does not provide any evidence for such a broader interpretation; see section 2.2 of *Sieg 1994*.

[2] The exchange between Gödel and van Heijenoort is also published in these *Works*.

[3] The background and the content of the Herbrand-Gödel correspondence was first described in *Dawson 1993*. The crucial feature Gödel had misremembered concerns the computability of finitist functions; see the discussion in the last part of this Note.

[4] For biographical details, see *Chevalley 1934* and *Chevalley and Lautmann 1931*.

the middle of May 1931 he had been in Berlin, then for a month in Hamburg, and for the remainder of the time in Göttingen. In these three cities, he had mainly worked with von Neumann, Artin and Emmy Noether.[5] Concerning his stay in Berlin he continued later on: "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'."[6] The paper he alluded to is his *1931c*, *Sur la non-contradiction de l'arithmétique*, notably comparing his own results with those of Gödel, as his friend Claude Chevalley put it.[7]

Indeed, Herbrand had learned of the incompleteness theorems from von Neumann shortly after his arrival in Berlin.[8] 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 see 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.[9]

This sentence opens the letter. Having sketched Gödel's arguments and reflected on the results, Herbrand concluded the logical part of his letter with: "Excuse this long beginning; but all of this has been pursuing me, and by writing about it I exorcise it a little."[10] When Herbrand wrote to Gödel on 7 April 1931 he had actually read the galleys of *Gödel 1931*; von Neumann had received them at the beginning of January 1931, but it seems that Herbrand had obtained access to

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[5] The remark in the report reads in French: "Dans ces trois villes, j'ai travaillé surtout avec M. von Neumann, Artin et Fr. Noether."

[6] A Berlin, j'ai surtout travaillé avec M. von Neumann, sur des questions de Logique Mathématique, et mes recherches dans cette branche seront exposeés dans un mémoire qui paraitra prochainement dans le 'Journal für reine und angewandte Mathematik'.

[7] *Chevalley 1934*, p. 25.

[8] For von Neumann's role in the early discussion of Gödel's theorems, see the Introductory Note to his correspondence with Gödel.

[9] Les mathématiciens sont une bien bizarre chose; voici une quinzaine de jours que chaque fois que je vois [von] Neumann nous causons d'un travail d'un certain Gödel, qui a fabriqué de bien curieuses fonctions; et tout cela détruit quelques notions solidement ancreés.

[10] Excuse ce long début; mais tout cela me poursuit, et de l'écrire m'en exorcise un peu.

them only more "recently" through Bernays, with whom he also had contact during his stay in Berlin.[11]

On the very day he wrote to Gödel, Herbrand sent a note as well to Bernays, enclosed a copy of his letter to Gödel and contrasted his consistency proof with that of Ackermann (which he ascribed 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.[12]

The central issue of the letter to Gödel is formulated for Bernays as follows: "I also try to show in this letter how your results can agree with these of Gödle [sic]."[13] All of this information puts into sharper focus the remark in Herbrand's *1931b*, which, according to Goldfarb's introductory note to that item in *Herbrand 1971*, was submitted to Hadamard at the beginning of 1931.

> 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. [[Herbrand is here alluding to *Gödel 1931*.]] In fact, I am at the present time preparing an article in which I will explain the relationships between these results and mine [[this article is *1931c*]].[14]

It seems quite clear that Herbrand's attempt to come to a thorough understanding of 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 *1931c*.

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[11] During the twenties Bernays spent the semester breaks mostly in Berlin with his family. Gödel had sent the galleys to Bernays' Berlin address in early January, but Bernays received them only in mid-January in Göttingen; see Bernays' letter to Gödel of 18 January 1931.

[12] 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. The German text in Herbrand's letter to Bernays reads: "In meiner Arithmetik ist das Axiom der Vollständigen Induktion beschränkt, aber man darf allerlei andere Funktionen benutzen als diejenige die durch einfache Rekursion definiert sind: in dieser Richtung scheint es mir dass mein Theorem etwas weiter geht als das Ihrige."

[13] Cf. previous note, as to the results Herbrand is referring to. The German text is: "Ich suche auch in diesem Brief zu zeigen wie Ihre Ergebnisse mit diesen von Gödle übereinstimmen können."

[14] *Herbrand 1931b*, p. 279. The remarks in double brackets are due to Goldfarb, the editor of *Herbrand 1971*.

At issue is the extent of finitist or, for Herbrand synonymously, intuitionist methods, and thus the reach of Hilbert's consistency program. Herbrand's letter can be understood, as Gödel in his response quite clearly did, to give a sustained argument against Gödel's assertion in his *1931* 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).[15]

Herbrand introduces a number of systems for arithmetic, all containing the axioms (I) for predicate logic with identity and the Dedekind-Peano axioms for zero and successor. The systems are distinguished by the strength of the induction principle, whether it is available for all formulas or just quantifier-free ones, and by the class F of finitist functions for which recursion equations are available. The system with full induction and recursion equations for functions in F is denoted by I+2+3F; if induction is restricted to quantifier-free formulas, the resulting system is denoted by I+2'+3F. The defining axioms for elements f_1, f_2, f_3, ... in F must satisfy, according to Herbrand, the following conditions:

> (I) 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 he considers the set E_1 of addition and multiplication, as well as the set E_2 of all primitive recursive functions from Gödel's *1931*. He asserts that the functions definable by his own "general schema" include many other functions, in particular, the Ackermann function (which he calls the Hilbert function). Furthermore, he argues that one can construct by diagonalization a

[15] *Gödel 1931*, p. 197; in these *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.

finitist function that is not in E, if E is a set of functions satisfying axioms such that "one can always determine, whether or not certain defining axioms are among these axioms".

The fact of the open-endedness of (a finitist presentation of) the concept of 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 every finitist proof, as a matter of fact, can be formalized in a system of the form I+2'+3F 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 I+2'+3F is finitist. He summarizes his reflections by saying in the letter (and in almost identical words on p. 297 of *1931c*):

> 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.

The conjectures and claims are strikingly similar to those von Neumann communicated to Gödel in his letters of 29 November 1930 and of 12 January 1931. We know of Gödel's response to von Neumann's dicta not through a letter from Gödel, but rather through the minutes of the meeting of the Schlick Circle that took place on 15 January 1931. These minutes report what Gödel viewed as questionable, namely, 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.[16]

In response to Herbrand's letter, Gödel makes more explicit his reasons for questioning the formalizability of finitist considerations in a single formal system, say in *Principia Mathematica*. He agrees with Herbrand on the indefinability of the concept "finitist proof". However, even if one accepts

[16] The minutes are found in the Carnap Archives of the University of Pittsburgh. Part of the German text is quoted in *Sieg 1988*, note 11, and more fully in *Mancosu 1999*, pp. 36-7. For other accounts of early reactions to Gödel's results, see *Dawson 1985* and *Mancosu 1999*. Interestingly, *Bernays 1933* uses "von Neumann's conjecture" to infer that the incompleteness theorems impose fundamental limits on proof-theoretic investigations.

Herbrand's very schematic presentation of finitist methods and the claim that every finitist proof can be formalized in a system of the form I+2′+3F, 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.

It seems that Gödel had changed his views significantly by late December 1933 when he gave an invited lecture to the Mathematical Association of America in Cambridge, Massachusetts. In the handwritten text for this lecture, Gödel's **1933o*, he sharply distinguishes intuitionist from finitist arguments, the latter constituting the most restrictive form of constructive mathematics. He also insists that the known finitist arguments given by "Hilbert and his disciples" can all be carried out in a certain system A.[17] In turn, he asserts, proofs in the system A "can be easily expressed in the 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".[18] The direct consequence of this observation and the second incompleteness theorem is that classical arithmetic cannot be shown to be consistent by finitist means. Gödel had anticipated that consequence by stating earlier: "But unfortunately the hope of succeeding along these lines [of trying to establish consistency by means that satisfy the restrictive demands of system A, WS] has vanished entirely in view of some recently discovered facts."

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[17] The restrictive characteristics of the system A are formulated on pp. 23 and 24 of **1933o*: (i) universal quantification is restricted to totalities whose elements can be generated by a "finite procedure"; (ii) negation cannot be applied to universal statements; (iii) notions have to be decidable and functions must be calculable. As to condition (iii), Gödel claims, "such notions and functions can always be defined by complete induction"; cf. Note 19 below and also Gödel's own Note 3 of *1934*.

Nevertheless, Gödel formulates on the next page of his **1933o* a theorem of Herbrand's as the most far-reaching among interesting partial results in the pursuit of Hilbert's consistency program: "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 result, mentioned in Herbrand's letter as Remark 2 (on p. 3), can be understood in just this way; it foreshadows of course the central result of Herbrand's *1931c*. Gödel conjectures that Herbrand's method might be generalized, but emphasizes again (on p. 27) 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".[19]

There is one prima facie puzzling remark in Herbrand's letter, when he claims in point 3: "In general, if we want to apply your methods to an arithmetic that has the functions of a set F, we need a larger set of functions. (This can be proved precisely: it is very easy.)" At the end of point 5, Herbrand refers in a parenthetical remark to this issue; he maintains that it is the function obtained by diagonalization that forces the consideration of larger classes of functions. Gödel finds point 3 "not completely comprehensible"; after all, he adds, a consistency proof forces us to go beyond the system being studied, but the proof of the

[18] *Gödel *1933o*, p. 26; in these *Works*, vol. III, p. 52. This issue is discussed also in Feferman's *Introductory Note* to **1933o*, these *Works*, vol. III, pp. 40-42, and in the correspondence with Bernays, in particular in Gödel's letter of 24 January 1967.

[19] This systematic context allows us to calibrate the strength of the system A in **1933o* and, thus, Gödel's views about the extent of finitist methods at this time. In Gödel's judgment, Herbrand had given a finitist consistency proof for a theory of arithmetic with quantifier-free induction and a large class F of calculable functions that included the Ackermann function; Gödel was thoroughly familiar with that theory, as he used it – with full induction – in his *1933e*. The system A is consequently stronger than primitive recursive arithmetic.

From the details of the consistency proof it is clear that the functions in F must be available in the finitist theory, and that in particular the Ackermann function is finitist. Gödel was at this time not alone in considering the Ackermann function as a finitist one; Herbrand obviously did, and so did von Neumann as witnessed by his letter of 29 November 1930 to Gödel. Indeed, Mark Ravaglia makes in his doctoral dissertation the case that Hilbert and Bernays view (in their *1934*) extensions of "rekursive Zahlentheorie" by Ackermann-type functions as finitist.

In part III of his Lecture at Zilsel's, **1938a*, Gödel distinguishes three constructive systems that all satisfy the most stringent constructivity requirements, and it is here that he introduces another system, also called A, that clearly is primitive recursive arithmetic. He claims on p. 3 that Hilbert "wanted to carry out the proof [[of consistency]] with this". (I do not have a conjecture, why Gödel changed his views.)

statement (*) "If the system is consistent, then the proposition given by me is unprovable." can be given in the system. Herbrand, as if anticipating Gödel's rejoinder, claims in *1931c* (on p. 296) that for the proof of just this statement one needs the function enumerating all elements of F. Consequently, Gödel's argument cannot be carried out in the system.

But, to carry out Gödel's argument, we have to number all objects occurring in proofs; we are thus led to construct the [enumeration, WS] function of two variables $f_y(x)$; this justifies what we were saying above, namely, that it is impossible, in an arithmetic containing the hypotheses C′, to formalize Gödel's argument about this arithmetic.[20]

Herbrand's specific assumption – that a finitist metamathematical description of an arithmetic like his, even when restricted to a definite set of recursion equations, uses necessarily an enumeration function - is not correct.[21] However, the implicitly underlying general point is worth emphasizing: the proof of (*), and thus the proof of the second incompleteness theorem, is based delicately on additional assumptions concerning the proof predicate. Those assumptions were formulated as *derivability conditions* in the second volume of Hilbert and Bernays' *Grundlagen der Mathematik*. In his own further reflections on the generality of his theorems, Gödel seems to focus exclusively on the analysis of "mechanical procedures" or "effective calculability", i.e., a general characterization of formal theories; but as we will see below that is not quite correct either.

This issue leads naturally to a discussion of the role this correspondence played for the origins of recursion theory. From the very beginning, Gödel attributed to Herbrand the inspiration for the definition of general recursive function in his 1934 Princeton Lectures. In those lectures Gödel strove, as indicated even by their title *On undecidable propositions of formal mathematical systems*, to make his incompleteness results less dependent on particular formalisms. In the introductory §1 he discussed the notion of "a formal mathematical system" in some generality and required that

[20] *Herbrand 1931c*, p. 296. The hypotheses C′ are "a definite group of schemata of type C", i.e., a definite group of recursion equations for the functions in F – that allows a finitist determination of which recursion equations are involved.

the rules of inference, and the definitions of meaningful formulas and axioms, be constructive; that is, 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 $A_1, \dots, A_n$, and there shall be a finite procedure for determining whether a given formula A is a meaningful formula or an axiom.[22]

He used, as 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".[23] But a notion that would suffice *in principle* was really needed, and Gödel attempted to arrive at a more general notion. He considered 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 added in footnote 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.[24]

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

If ϕ denotes an unknown function, and $\psi_1, \dots, \psi_k$ 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

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[21] See *Rose 1984* for a contemporary presentation of such theories.
[22] *Gödel 1934*, p. 1; in these *Works*, vol. I, p. 346.
[23] *Gödel 1934*, p. 19; in these *Works*, vol. I, p. 361.
[24] *Gödel 1934*, p. 3; in these *Works*, vol. I, p. 348. Gödel added later: "This statement is now outdated; see the Postscriptum, pp. 369-371." He refers to the Postscriptum appended to the lectures for *Davis 1965*. – It should also be emphasized that Gödel did not intend to formulate (a version of) Church's Thesis; cf. *Davis 1982*, p. 8. It is of interest to note, however, that already in *1933o*, p. 24, Gödel asserts that functions that "can be calculated for any particular element" can always be defined by complete induction.

are equated, then, if the resulting set of functional equations has one and only one solution for ϕ, ϕ is a recursive function.[25]

He 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 being the outermost symbol and, secondly, that "for each set of natural numbers $k_1, \ldots, k_l$ there shall be exactly one and only one m such that $\phi(k_1, \ldots, k_l) = m$ is a derived equation". The rules that were allowed in giving derivations are simple substitution and replacement rules. This proposal was taken up for systematic development in *Kleene 1936*.

We should distinguish then, as Gödel did, two features: first, the precise specification of *mechanical* rules for deriving equations, i.e., for carrying out computations, and second the formulation of the *regularity condition* requiring calculable functions to be total. That point of view was also expressed by Kleene who wrote in his *1936* with respect to the definition of general recursive function of natural numbers:

It consists in specifying the form of the equations and the nature of the steps admissible in the computation of the values, and in requiring that for each given set of arguments the computation yield a unique number as value.[26]

In his letter to van Heijenoort, dated 14 August 1964, Gödel asserted that "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".[27] As was noticed, 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 his broad

[25] *Gödel 1934*, p. 26; in these *Works*, vol. I, p. 368. *Kalmar 1955* pointed out that the class of functions satisfying such functional equations is strictly greater than the class of general recursive functions; see also the exchange of letters between Gödel and Büchi in these *Works*.
[26] *Kleene 1936*, p. 727.

assessment, but rather to point to the most important step he had taken by disassociating recursive functions from an epistemologically restricted notion of proof.

Gödel later on 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".[28] The very notion of partial recursive function, of course, had been introduced in *Kleene 1938*. At this earlier historical juncture, however, the introduction of an equational calculus with particular computation rules was important for the mathematical development of recursion theory as well as for the underlying conceptual analysis. It brought out clearly what Herbrand, according to Gödel in his letter of 14 August 1964 to van Heijenoort, had failed to see, namely "that the computation (for all computable functions) proceeds by exactly the same rules". In addition, the rules needed are of a remarkably elementary character due to the general symbolic character of the computation steps. It seems that 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 general recursive function.

In a way, the mathematical development of computability theory based on this general analysis provided an important fact for responding to Herbrand's issue concerning the proof of the second incompleteness theorem that was mentioned above. Gödel formulated in section 6 of *1934* a number of "conditions that a formal system must satisfy in order that the foregoing arguments apply", i.e., the arguments for the incompleteness theorems. The very first condition states:

> Supposing the symbols and formulas to be numbered in a manner similar to that used for the particular system considered above, then the class of axioms and the relation of immediate consequence shall be [primitive, WS] recursive.[29]

[27] That is claimed in the letter to van Heijenoort of 23 April 1963.
[28] *Wang 1974*, p. 84.
[29] *Gödel 1931*, p. 19; in these *Works*, vol. I, p. 361.

The condition becomes superfluous, Gödel writes in his 1964 Postscriptum, if formal systems are viewed as mechanical procedures for producing formulas, and if Turing's analysis of such procedures is accepted. The antecedent of this conditional provides the basis for a proof "that for any formal system provability is a predicate of the form *(Ex)x*B*y*, where B is primitive recursive". Together with the introducibility of all primitive recursive functions in elementary number theory, the latter fact is crucial for the detailed proof of the second incompleteness theorem (more specifically, for the verification of the third derivability condition) by Hilbert and Bernays, which "carries over almost literally to any system containing, among its axioms and rules of inference, the axioms and rules of inference of number theory".[30]

Wilfried Sieg[31]

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[30] Postscript to *Gödel 1934*, these *Works*, vol. I, p. 370.
[31] I am most grateful to Cathérine Chevalley, who had provided me already in 1991 with Herbrand's remarkable letter to her father (as well as Herbrand's reports on his stay in Germany); the letter was used in my *1994* to understand better the early reception of Gödel's talk in Königsberg. – I also want to thank John Dawson, Solomon Feferman and, in particular, Charles Parsons for suggestions that led to real improvements of earlier drafts of this Note.

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Introductory Note to the Gödel-Post Correspondence

Wilfried Sieg

September 1, 2001

Shortly after his return from Europe on 15 October 1938, Gödel attended a regional meeting of the American Mathematical Society in New York City.[1] During the meeting, on October 29, he made the acquaintance of Emil L. Post. Post wrote a brief, moving note to Gödel the very same day and an extended letter on the following day. Post reflected on their meeting and, relatedly, on his own work on absolutely undecidable problems that had started in the early twenties and "anticipated" Gödel's first incompleteness theorem. It had been highly emotional for Post to meet the man "chiefly responsible for the vanishing of that dream", i.e., the dream of astounding the mathematical world by his "unorthodox ideas" and by establishing the existence of unsolvable problems. "Needless to say", he emphasized at the end of his second letter, "I have the greatest admiration for your work, and after all it is not ideas but the execution of ideas that constitutes the mark of greatness." Post wrote a third letter on 12 March 1939 after he had read Gödel's abstract *1939* on the relative consistency of the continuum hypothesis. Gödel had sent Post a "sheaf of reprints" in the fall of 1938, but responded to Post's letters only on 20 March 1939.

At the time of his meeting with Gödel, Post was a faculty member at the City College of New York. He had been appointed there in 1935 and remained at the institution until his death in 1954. His education was also deeply connected with City College. Born in the Polish town of Augustow on 11 February 1897, Post emigrated with his parents to New York in May of 1904. He attended Townsend Harris High School, a free secondary school for gifted students located on the campus of City College, and received his B.S. from City College in 1917.

[1] For a fuller discussion of Gödel's circumstances at the time, see *Dawson 1997*; the meeting between Post and Gödel is described on pp. 130-2. *Davis 1994* provides information on Post's life and work, in particular about the enormous difficulties Post had to face, from his severe mental illness to the restrictive working conditions under which his scientific endeavors had to be pursued. -- The biographical facts in this Note stem from these two sources.

Special thanks to Martin Davis, John Dawson, Solomon Feferman, and Charles Parsons for helpful e-correspondence concerning a draft of this Note; John Dawson helped me to locate information on the mathematician Jesse Douglas mentioned in the Postscript of Post's letter of 29 October 1938.

From 1917 to 1920 he was a graduate student at Columbia University, finishing with a thesis under the direction of Cassius Keyser. The thesis was published as *Post 1921* and concerned the propositional calculus in Whitehead and Russell's *Principia Mathematica*. Post established the semantic completeness[2] of the calculus; he went on to generalize the "postulational method" and the "truth-table development" for finitely many, arbitrary propositional connectives. The former generalization was the starting-point for Post's investigation of symbolic logics that led to the anticipation of Gödel's result. Post's *1941* gives an account of this work; the paper was rejected for publication and appeared only in *Davis 1965*.

The central new mathematical result of *1941* - the reduction of arbitrary canonical systems to systems in normal form - was presented in *Post 1943*. Post's Thesis[3] secures then the reduction for *all* symbolic logics. Post describes in his letter how this reductive result led him first to the discovery of an absolutely unsolvable problem and then to the realization of the incompleteness of all symbolic logics. Post emphasizes that a particular proposition can be seen to be undecidable, i.e., "... a particular enunciation of the logic, determined by the logic, and of course the entscheidung problem [sic] and the method of proving the above contradiction, was such that neither it nor its negative was asserted in the logic." These very sketchy considerations are detailed in section 2 of *Post 1944*; that section is entitled "A form of Gödel's theorem".

Post formulates then in his letter what he takes to be the main point of Gödel's Theorem: "... the existence of an undecidable proposition in each logic sufficiently general and yet a 'symbolic logic'." This is a formulation of sufficient generality, such as Gödel himself had been aiming for. Its rigorous mathematical

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[2] This is the Fundamental Theorem in section 3 of *Post 1921*. - Hilbert and Bernays established the (Post) completeness of the propositional calculus in lectures of 1917/18 and Bernays's Habilitationsschrift of 1918. Some of the results of the latter were published only in *Bernays 1926*; cf. *Sieg 1999* and *Zach 1999*.

[3] In *Davis 1982*, p. 21, Post's Thesis is formulated roughly as follows: any set of strings on some alphabet that can be generated by a finite process (thus any symbolic logic) can be generated by canonical productions and, using the reductive result, by normal productions.

version depends for its adequacy on Post's Thesis.[4] Indeed, Post's proof of the incompleteness result relies on it by identifying symbolic logics with normal systems. The remainder of the letter is devoted to the underlying methodological problem and how its difficulty, together with the personal and professional reasons hinted at in Note 1, prevented Post from publishing his considerations.

The thesis has, according to Post, "but a basis in the nature of physical induction". He believes that that is true not only for his own work, but for "any work". Such a perspective had been taken in *1936* where Post presents his *formulation 1*, a model of computation essentially identical with Turing's. On p. 105 of that paper he asserts:

> The writer expects the present formulation to turn out to be logically equivalent to recursiveness in the sense of the Gödel-Church development. Its purpose, however, is not only to present a system of a certain logical potency but also, in its restricted field, of psychological fidelity. In the latter sense wider and wider formulations are contemplated. On the other hand, our aim will be to show that all such are logically reducible to formulation 1. We offer this conclusion at the present moment as a *working hypothesis*. And to our mind such is Church's identification of effective calculability with recursiveness.

In a footnote to the last sentence, Post claims that the actual work of Church and others has carried "this identification considerably beyond the working hypothesis stage". He warns, however, that calling the identification a definition may blind us "to the need of its continual verification" by considering, quasi-empirically, wider and wider formulations and reducing them to formulation 1. The success of this research program, he says in the main text, would "change the hypothesis not so much to a definition or an axiom but to a *natural law*".

Post, in his letter to Gödel, states that the quasi-empirical work supporting the "induction" could be extended to cover the system of *Gödel 1931* and to

[4] Gödel emphasized in the *Postscriptum* to his *1934* the need of Turing's penetrating analysis for being able to formulate his incompleteness theorems for *all* formal systems (that are consistent

obtain, in this way, the incompleteness theorem specifically for that system without appeal to the thesis. That this could be done for the system of *Principia Mathematica* itself Post claims to have seen in the twenties. Post did not then pursue the inductive avenue, because he thought he saw a way of properly analyzing "all finite processes of the human mind" and thus a possibility of establishing the theorem "in general and not just for Principia Mathematica". Post adds in parentheses after "human mind" in the last quote "something of the sort of thing Turing does in his computable number paper".[5] How closely related Post's foundational considerations were to those of Turing can be seen from later developments reflected in *Post 1947*, *Turing 1950*, and *Turing 1953*. Post used in *1947* a description of Turing machines by production systems to show the unsolvability of the Thue problem (established independently in *Markov 1947* using quite directly Post's normal systems). Turing employed in *1950* the same techniques to extend Post's result. In his semi-popular *1953*, he formulated a version of his thesis in Post's way: all puzzles (i.e., combinatory problems) can be transformed into substitution puzzles (i.e., Post's normal systems); then he gave a perspicuous presentation of solvable and unsolvable problems via substitution puzzles.

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and include a modicum of elementary number theory).

[5] The reference is of course to *Turing 1936* and, more particularly, to Turing's argument in section nine of that paper; as to the analysis of that argument, see *Sieg 2001* and literature quoted there. In his own *1943* Post writes in note 18: "Since the earlier formal work made it seem obvious that the actual details of the outline [of the proof of his version of the incompleteness theorem, WS] could be supplied, the further efforts of the writer were directed towards establishing the universal validity of the basic identification of generated sets with normal sets."

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Introductory Note to the Gödel-von Neumann Correspondence*

Wilfried Sieg

September 4, 2001

* Many thanks go to Sam Buss and John Dawson for providing me with information on *Buss 1995* and *Clote e.a. 1993*, respectively *Hartmanis 1989*. Solomon Feferman and Charles Parsons suggested substantive and stylistic improvements.

The correspondence between Johann von Neumann and Gödel opens with an extraordinary letter from von Neumann, written on 20 November 1930. In early September of that year von Neumann had met Gödel at a congress in Königsberg and was informed about a theorem Gödel had just discovered – (a form of) the first incompleteness theorem. Von Neumann was deeply impressed; he turned his attention to logic again and gave lectures on proof theory in the winter term of 1930-31. As can be gathered from Herbrand's letter to Claude Chevalley,[1] von Neumann was preoccupied with Gödel's result and, as he put it in his own letter, with the methods Gödel had used "so successfully in order to exhibit undecidable properties." In reflecting on this result and Gödel's methods, von Neumann arrived at a new result that seemed remarkable to him, namely, that the consistency of a formal theory is unprovable within that theory, if it is consistent. He formulates this "new result" in the letter to Gödel and claims, less precisely, that the consistency of mathematics is unprovable; this strong interpretation of - what we know as -the second incompleteness theorem was to become be a point of contention between Gödel and von Neumann.

In his next letter of November 29, von Neumann acknowledges the receipt of a "Separatum" and a letter from Gödel.[2] It is most likely that the separatum was a copy of the abstract *1930b* that had been presented to the Vienna Academy of Sciences on 23 October 1930 and already contained the classical formulation of the second incompleteness theorem.[3] Von Neumann states in his response: "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." Their differing views on the impact of this result for Hilbert's consistency program is discussed below. Two additional topics of scientific interest are addressed in later correspondence: (i) the relative

[1] Cf. the *Introductory Note* to the Gödel-Herbrand correspondence; Herbrand wrote the letter on 3 December 1930.

[2] Unfortunately, it seems that this letter and two others in this early correspondence have not been preserved: von Neumann acknowledges in his letter of 12 January 1931 that he had received two letters from Gödel. (The von Neumann Archives in the Library of Congress do not contain these letters.)

[3] Gödel had by this time completed his *1931*; indeed, the paper had been submitted for publication on 17 November 1930. Von Neumann acknowledged receipt of the galley proofs of *Gödel 1931* in his letter of 12 January 1931. From *Mancosu 1999* it is clear that Gödel had not sent the galleys before the end of December 1930; see the letters between Hempel and Kaufmann quoted there on pp. 35-36.

consistency of the axiom of choice and the generalized continuum hypothesis, in letters from 1937 through 1939, and (ii) the feasibility of computations (related to the now famous P vs. NP problem), in Gödel's last letter to von Neumann in 1956.

As von Neumann's life and work are well-known, only the briefest biographical sketch is presented.[4] Born on 28 December 1903 in Budapest (Hungary), von Neumann grew up in a wealthy Jewish family and attended the excellent Lutheran Gymnasium in Budapest from 1914 to 1921. He then entered the University of Berlin as a student of chemistry, but switched in 1923 to the Eidgenössische Technische Hochschule in Zürich, where he earned three years later a diplom degree in that subject. He obtained, also in 1926, a doctoral degree in mathematics from the University of Budapest. Von Neumann spent the academic year 1926-27 in Göttingen supported by a Rockefeller Fellowship. He was Privatdozent in Berlin (1927-29) and Hamburg (1929-30). In 1930 he was appointed visiting lecturer at Princeton University with the agreement that he would be back in Berlin for the winter term of 1930-31. In 1931 he was promoted to professor of mathematics at Princeton and became two years later one of the six mathematics professors at the newly founded Institute of Advanced Study, together with J.W. Alexander, A. Einstein, M. Morse, O. Veblen, and H. Weyl; he kept that position for the remainder of his life. Returning to the correspondence with Gödel, it obviously started while von Neumann was staying in Berlin during the winter term of 1930-31.

In the 1920s von Neumann contributed to the foundations of mathematics not only through a series of articles on set theory (*1923, 1925, 1926, 1928, 1928a, 1929*) but also very specifically to Hilbert's emerging finitist consistency program through his paper *Zur Hilbertschen Beweistheorie*. Though published only in 1927, the paper had already been submitted for publication in July of 1925. In it von Neumann established the consistency of a formal system of first-order arithmetic with quantifier-free induction; he also gave a detailed critique of the consistency

[4] For accounts see the collection of essays *Birkhoff e.a. 1958* (in particular the accessible description by Ulam on pages 1-49) and the book *Macrae 1992*.

proof in *Ackermann 1924*. What is of interest in the context of his early correspondence with Gödel is the general strategic attitude he took towards proof-theoretic research. It is expressed in the following quote from the introduction to his *1927*, where he formulates four guiding ideas of Hilbert's proof theory. (Note that "intuitionist" and "finitist" were evidently synonymous for von Neumann.) Viewing an intuitionistic consistency proof for classical formal theories as the crucial aim, he articulates the final guiding idea as follows:

> Here one has always to distinguish sharply between two different ways of "proving": between the formalized ("mathematical") proving within a formal system and the contentual ("metamathematical") proving about the system. While the former is an arbitrarily defined logical game (that must be, however, to a large extent analogous with classical mathematics), the latter is a chaining of immediately evident contentual insights. This "contentual proving" has consequently to be carried out completely within the intuitionistic logic of Brouwer and Weyl: proof theory is to rebuild classical mathematics so-to-speak on an intuitionistic basis and in this way reduce strict intuitionism ad absurdum. [5]

The strategic goal of proof-theoretic research, as interpreted by von Neumann, also shaped his talk at the *Second Conference for Epistemology of the Exact Sciences*. The conference was held in Königsberg from 5 to 7 September 1930, and on the first day of the congress von Neumann talked about Hilbert's finitist standpoint in a plenary session, where Carnap and Heyting presented the logicist, respectively intuitionist position.[6] On the next day Gödel described the

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[5] *von Neumann 1927*, pp. 2-3. The German text is this: "Hierbei muß stets scharf zwischen zwei verschiedenen Arten des "Beweisens" unterschieden werden: Dem formalisierten ("mathematischen") Beweisen innerhalb des formalen Systems, und dem inhaltlichen ("metamathematischen") Beweisen über das System. Während das erstere ein willkürlich definiertes logisches Spiel ist (das freilich mit der klassischen Mathematik weitgehend analog sein muß), ist das letztere eine Verkettung unmittelbar evidenter inhaltlicher Einsichten. Dieses "inhaltliche Beweisen" muß also ganz im Sinne der Brouwer-Weylschen intuitionistischen Logik verlaufen: Die Beweistheorie soll sozusagen auf intuitionistischer Basis die klassische Mathematik aufbauen und den strikten Intuitionismus so ad absurdum führen."

[6] Gödel reviewed the published versions of these presentations in *Gödel 1932e,f,* and *g*. Waismann had actually also given a paper in the plenary session, entitled *Das Wesen der Mathematik: Der Standpunkt Wittgensteins;* his talk was not published.

From the letters between von Neumann and Carnap, quoted in *Mancosu 1999*, we know that their Königsberg talks were published (in the form they were) only to reflect the situation before Gödel's results. Von Neumann writes, in his letter to Carnap of 7 June 1931:

"Ich halte daher den Königsberger Stand der Grundlagendiskussion für überholt, da Gödels fundamentale Entdeckungen die Frage auf eine ganz veränderte Plattform gebracht haben. (Ich weiss, Gödel ist in der Wertung seiner Resultate viel vorsichtiger, aber m. E. übersieht er die Verhältnisse an diesem Punkt nicht richtig.)

Ich habe mit Reichenbach mehrfach besprochen, ob es unter diesen Umständen überhaupt Sinn hat, mein Referat zu publicieren – hätte ich es 4 Wochen später gehalten, so hätte es ja wesentlich anders gelautet. Wir results of his dissertation.[7] The plenary session was complemented on 7 September by a roundtable discussion concerning the foundations of mathematics. That discussion was chaired by Hans Hahn, and its participants included Carnap, Heyting, and von Neumann, but also three additional scholars, namely, Arnold Scholz, Kurt Reidemeister, and Gödel. A shortened and edited transcript of this discussion was published as *Hahn e.a. 1931* in *Erkenntnis.* Gödel was invited by the editors of the journal to expand on the very brief remarks about the first incompleteness theorem he had made during the discussion; the resulting note was added as a *Nachtrag* to the transcript (see *Gödel 1931a*). According to *Dawson 1997*, Gödel had already discussed the new discovery with Carnap and Waismann before the conference in Vienna, on 26 August 1930:

> The main topic of conversation was the plan for their upcoming journey to the conference in Königsberg, where Carnap and Waismann were to deliver major addresses and where Gödel was to present a summary of his dissertation results. But then, Carnap tersely noted, the discussion turned to "Gödel's discovery: incompleteness of the system of *Principia Mathematica;* difficulty of the consistency proof."[8]

This provides a sketch of the background for the meeting at which von Neumann made the acquaintance of Gödel. In his *1981*, Wang reports (Gödel's view) about the encounter with von Neumann:

> In September 1930, Gödel attended a meeting at Königsberg (reported in the second volume of *Erkenntnis*) and announced his result [[i.e., the first incompleteness theorem, WS]]. R. Carnap, A. Heyting, and J. von Neumann were at the meeting. 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

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kamen schliesslich überein, es als eine Beschreibung eines gewissen, wenn auch überholten Standes der Dinge doch niederzuschreiben."
In a note to the last sentence von Neumann adds: "Ich möchte betonen: *Nichts* an Hilberts Ansichten ist *falsch.* Wären sie durchführbar, so würde aus ihnen durchaus das von ihm Behauptete folgen. Aber sie sind eben undurchführbar, das weiss ich erst seit Sept. 1930."

[7] The abstract of Gödel's talk is *1930a*, the draft of his talk presumably *1930c*. Dawson's *1990* and *1997*, and also Mancosu's *1999*, describe the early reception of the incompleteness theorems.

[8] *Dawson 1997*, p. 68.

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.[9]

As to the discovery of the second incompleteness theorem, we thus clearly know that Gödel did not have it in Königsberg and that, in contrast, the abstract *1930b* contains its classical formulation. The abstract was presented by Hahn on 23 October 1930 to the Vienna Academy of Sciences. The full text of Gödel's *1931* was submitted for publication to the editors of Monatshefte on 17 November 1930.

There is genuine disagreement between Gödel and von Neumann on how the second incompleteness theorem affects Hilbert's finitist program. Von Neumann states his view strongly in his letters to Gödel of 29 November 1930 and 12 January 1931. (As to other views, cf. the Introductory Note to the correspondence with Herbrand and the exchange with Bernays, in particular, the letters of 24 December 1930, 18 January 1931, 20 April 1931, and 3 May 1931.) In his letter of 29 November to Gödel, von Neumann writes:

> 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 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.

From the general fact of the unprovability of a system's consistency within the system, he concludes that "There is no rigorous justification of classical mathematics." In the second letter, after having received the galleys of Gödel's *1931*, he writes even more forcefully:

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[9] *Wang 1981*, pp. 654-5. The *Introductory Note* to the correspondence with Wang, these *Works*, describes on pp. XX-YY the interaction between Gödel and Wang on which this paper is based.

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 intuitionism is not affected by that 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 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!

We know of Gödel's response to von Neumann's dicta not through a letter from Gödel, but rather through the minutes of the meeting of the Schlick Circle that took place on 15 January 1931. These minutes report what Gödel viewed as questionable, namely, 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.[10] However, we also know that by December of 1933 Gödel had changed his view as follows: Finitism, considered by Gödel as the strictest form of constructive mathematics, is *narrower* than intuitionism and (its practice) can be captured in a formal system. Thus he argues, alluding to the second incompleteness theorem, the hope of succeeding along the lines proposed by Hilbert "has vanished entirely in view of some recently discovered facts". That change is made explicit in his talk *1933o* to the Mathematical Association of America.[11]

Von Neumann's admiration for Gödel's work is expressed directly in his very first letter of 20 November 1930, when he calls the first incompleteness

[10] The minutes are found in the Carnap Archives of the University of Pittsburgh. Part of the German text is quoted in *Sieg 1988*, note 11, and more fully in *Mancosu 1999*, pp. 36-7. Interestingly, *Bernays 1933* uses "von Neumann's conjecture" to infer that the incompleteness theorems impose fundamental limits on proof theoretic investigations.

[11] Cf. **1933o*, these *Works*, volume III, pp. 51-2 and also the Introductory Note to the correspondence with Herbrand.

theorem "the greatest logical discovery in a long time." That admiration is also reflected, for example, in his decision to talk about the incompleteness theorems when he lectured at Princeton in the fall of 1931. Kleene reports in his *1987b* (on page 491) that through this lecture "Church and the rest of us first learned of Gödel's results." Von Neumann's friend Ulam states in his *Adventures of a Mathematician*:

> When it came to other scientists, the person for whom he [[von Neumann]] had a deep admiration was Kurt Gödel. This was mingled with a feeling of disappointment at not having himself thought of "undecidability." For years Gödel was not a professor at Princeton ... Johnny would say to me, "How can any of us be called professor when Gödel is not?"[12]

The letters from 13 July 1937 through 17 August 1939 are mainly focused on practical issues surrounding the publication of Gödel's work on the relative consistency of the axiom of choice and the generalized continuum hypothesis. After von Neumann finally had the opportunity to study Gödel's lectures thoroughly, he wrote on 22 April 1939:

> I would like to convey to you, most of all, my admiration: You solved this enormous problem with a truly masterful simplicity. And you reduced to a minimum the unavoidable technical complications of the proof details by a presentation of impressive persistence and drive. Reading your investigations was really a first-class aesthetic pleasure.

It is quite impressive that von Neumann studied Gödel's investigations in sufficient detail to make also some "critical remarks"; perhaps even more impressive is his earlier letter of 28 February 1939 in which he directs Gödel to the paper *1938a* by Kondô that contains, in his view, "quite remarkable and surprising results on higher projective sets". He asks Gödel, "Are such matters not important for your further investigations on the continuum hypothesis ...?" Gödel responds in his letter of 20 March 1939 by saying with reference to his *1938*: "The result of Kondô is of great interest to me and will definitely allow an important simplification in the consistency proof of 3. and 4. of the attached

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[12] *Ulam 1976*, p. 80.

offprint." (For an explanation of the nature of these results, see Solovay's *Introductory Note to 1938-1940*, these *Works*, volume II, in particular pages 14-15.)

During the following 17 years, it seems, von Neumann and Gödel did not exchange letters; after all, they were colleagues at the Institute for Advanced Study. In the spring of 1955, von Neumann took a leave from the Institute and moved from Princeton to Washington, D.C., in order to work as a member of the Atomic Energy Commission to which he had been appointed by President Eisenhower. In the preface to von Neumann's posthumous *1958*, his widow Klara reports that von Neumann was diagnosed with bone cancer in August 1955. His health deteriorated quickly. By January 1956 he was confined to a wheelchair, though he still attended meetings and worked in his office. There was also some hope that X-ray treatment might be helpful. Klara von Neumann writes that by March of 1956, however, "... all false hopes were gone, and there was no longer any question of Johnny being able to travel anywhere. ... In early April Johnny was admitted to Walter Reed Hospital; he never left the hospital grounds again until his death on February 8, 1957."

Gödel wrote his last letter to von Neumann on 20 March 1956. He had heard, so he states in this letter, that von Neumann had undergone a radical treatment and was feeling better. "I hope and wish," Gödel continues, "that your condition will soon improve even further and that the latest achievements of medicine may, if possible, effect a complete cure." Then he formulates a striking mathematical problem and asks for von Neumann's view on it. It concerns the feasibility of computations and is closely connected to the problem that has caught, independently, the attention of mathematicians and computer scientist, the P versus NP problem.[13] For Gödel it is the question "how significantly *in*

[13] This is the question, whether the class P of functions computable in polynomial time is the same as the class NP of functions computable non-deterministically in polynomial time. For a very good introduction to the rich and multifaceted problems that fall into the NP category, see *Garey and Johnson 1979*.

Part of the letter was already published in *Hartmanis 1989*; the full German letter and its English translation are found in the Preface to *Clote e.a. 1993*. In both papers Gödel's question is related in informative ways to contemporary work in computational complexity. All the mathematical issues raised in Gödel's letter are addressed and resolved in *Buss 1995*. In particular, Buss shows that indeed $\phi(n) \geq Kn$, for some constant K and infinitely many n, and that the n-symbol provability question raised by Gödel is NP-complete for predicate logic and, surprisingly, even for sentential logic.

general for finitist combinatorial problems the number of steps can be reduced when compared to pure trial and error." The context in which he locates the general issue is noteworthy. Consider the question, whether a formula F in the language of first-order logic has a proof of length n, i.e., n is the number of symbols occurring in the proof. A suitably programmed Turing machine can answer this question. If $\psi(F,n)$ is the number of steps an "optimal" machine must take to obtain the answer and $\phi(n)=\max_F \psi(F,n)$, then the important question is how rapidly $\phi(n)$ grows. Gödel remarks that it is possible to prove that $\phi(n) \geq Kn$, for some constant K. If there were a machine such that $\phi(n)$ would grow essentially like Kn (or even Kn^2), Gödel suggests, "that would have consequences of the greatest significance. Namely, this would clearly mean that the thinking of a mathematician in case of yes-and-no questions could be completely replaced by machines, in spite of the unsolvability of the Entscheidungsproblem." In the next-to-last paragraph Gödel mentions Friedberg's recent solution of Post's problem and returns then to an issue that had been underlying much of the foundational discussion of the twenties: In what formal framework can one develop classical analysis? Gödel reports that Paul Lorenzen has built up the theory of Lebesgue measure within ramified type theory.[14] "But," Gödel cautions, "I believe that in important parts of analysis there are impredicative inference methods that cannot be eliminated."

In the face of human mortality, Gödel thus chose to raise and discuss eternal mathematical questions.

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