REMARKS ON FINITISM

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The background of these remarks is that in 1967, in ''Constructive reasoning" [27], I sketched an argument that finitist arithmetic coincides with primitive recursive arithmetic, PRA; and in 1981, in "Finitism" [28], I expanded on the argument. But some recent discussions and some of the more recent literature on the subject lead me to think that a few further remarks would be useful.

The question "What is finitism?" is really two questions, which I did separate in 1981, but perhaps not sufficiently clearly. First, and, in truth, for me most importantly, there is the *conceptual* problem of making sense of the idea of a 'finitist' function or 'finitist' proof of a finitist arithmetic proposition such as $\forall xy[x + y = y + x]$, which seems to refer to the infinite totality of numbers. And secondly, there is the *historical* question of what Hilbert—or perhaps better, Hilbert and Bernays[1]—meant by "finitism". The two questions are not entirely independent, of course, since it was Hilbert and Bernays who originally marked out the conception of finitist mathematics. But, if we take as the central issue the question of the 'finite' in finitism, we may be led to reject some aspects of the Hilbert-Bernays account. Indeed, my analysis rejects the Kantian element in their discussions. Their account of finitist mathematics begins with the restriction to objects representable in intuition or obtained by 'intuitive abstraction'—'formal objects' as Bernays calls them [12, 1]. For Bernays the finiteness of mathematical objects is a *consequence* of their representability in intuition. (See [2, p. 40].) But our problem is, of course, not the finiteness of a number, but the infinity of numbers. There is, I think, a difficulty with Bernays' notion of formal object, where this is intended to extend to numbers so large as, not only to be beyond processing by the human mind, but possibly to be beyond representablity in the physical world. [2, p. 39]. This difficulty ought to be discussed more adequately then

[†]This paper is based on a talk that I was very pleased to give at the conference *Reflections*, December 13-15, 1998, in honor of Solomon Feferman on his seventieth birthday. The choice of topic is especially appropriate for the conference in view of recent discussions we had had about finitism. I profited from the discussion following my talk and, in particular, from the remarks of Richard Zach. I have since had the advantage of further discussions with Zach and of reading his paper 1998; and I use his scholarship here shamelessly for my own purposes. Finally, I want to thank the two anonymous referees of this paper, whose comments have led to several changes in the final version.

[1]See [22, §3.4] for a discussion of Bernays contribution to the conception of finitism.

Meeting

I have sofar done; but I won't take it up here. The point I want to emphasize now is that I don't see in this notion of formal object the means for reasoning about the *totality* of numbers. My argument, which I don't want to repeat or expand upon here, is that the idea of iteration, which is of the essence of the idea of number and, in particular, is the means by which numerical functions are defined and numerical equations are proved, is not found or represented in intuition but is a creature of reason. That we may define a function by primitive recursion on a numerical variable is not a consequence of the representability of numbers in intuition, but rather follows from what we mean by (finite) iteration.

I attempted to answer the first, conceptual, question by taking seriously the notion of an *arbitrary* or *generic* object X of a given finitist type, where a finitist type of the first kind is a product $\mathbb{N} \times \cdots \times \mathbb{N}$, $\mathbb{N}$ being the type of the natural numbers, and a finitist type of the second kind is a product whose factors are numerical equations $m = n$. An objct of the latter type, if there is any, consists of a proof of each of the factors $m = n$ from the axiom $0 = 0$ using the inference $a = b \Rightarrow a' = b'$. My argument is that one can understand the idea of an arbitrary object of a given finitist type independently of that of the totality of objects of that type; and on its basis, we may proceed to construct objects of possibly other finitist types, which may depend on X. Thus, when X is of finitist type of the first kind, we may construct from it other objects $f(X)$ of types of the first kind. I claimed that, when we identify just what means of construction from X are implicit in the idea of such an arbitrary object, they turn out to be precisely those by means of which we define the primitive recursive functions. Likewise, when we identify, for given finitist functions $f(X)$ and $g(X)$, what constructions of a proof of $f(X) = g(X)$ are implicit in the idea of an arbitrary X, they turn out to yield proofs of exactly those equations deducible in PRA.

Concerning the conceptual question, some doubts have been raised about the identification of finitism with PRA. For example, Kreisel [20] takes finitism to include quantifier-free induction up to any ordinal below ϵ_0 and Ignjatović [18, p. 323] writes that my analysis of finitistic reasoning is not beyond any doubt. I have criticized Kreisel's conception in [28]. In footnote 5 (p. 323) Ignjatović writes: "Of course, one cannot rule out the possibility that any basis sufficient to justify what is formalized in (PRA) and which satisfies some necessary closure properties in order to be acceptable as an epistemologically distinguished system of methods, is also sufficient to justify ϵ_0-induction." But the basis on which [28] derived PRA is the finitist types; the claim is that, to go beyond PRA requires the introduction of higher types, e.g. of numerical functions and proofs. One would think that this restriction on the types of objects admitted would satisfy the condition of being "an epistemologically distinguished system of methods".

Regarding Gödel's position(s) on the conceptual question, the evidence is not entirely sraightforward. In [4, p. 198], he explicitly denies that his incompleteness theorems undermine Hilbert's attempt to obtain *finitary* consistency proofs, and it seems reasonably clear that he is referring to the conceptual rather than the historical question of the meaning of finitism.[2] In view of this, it would seem that he was open at that time to the possibility that finitism extends beyond PRA. On the other hand, in his notes for lectures in 1933 [5] and in 1938 [6], he explicitly attributes to Hilbert the aim of establishing consistency in PRA. Moreover, in the first of these he refers to PRA as the first in an ascending series of layers of intuitionistic or constructive mathematics [11, p. 51] and, in the second, he refers to it as finitary number theory, the lowest level of a hierarchy of finitist systems [11, p. 93]. In [7, p. 281], however, he refers to Hilbert's definition of finitism as "the mathematics in which evidence rests on what is *intuitive*" and so rejects as finitist in this sense the higher levels of "finitist systems", such as intuitionism and his own system T of primitive recursive functions of finite type.

Kreisel [21, p. 506] accepts the identification of finitism (at least as it is described in the beginning of [16]) with PRA, but doubts my argument for it. He writes of [27] that "its central point seems to be that the evidence of each proof has, in some essential way, a strictly *finite* character. As it stands the analysis ... is unconvincing since the understanding of any *one* rule goes beyond this." I would not like to talk about the evidence of a proof, but simply of the proof (i.e. evidence for the proposition). The 'some essential way' in which the proof $f(X)$ has a finite character is that it appeals only to what is implicit in the idea of an arbitrary object X of the given finitist type $\mathbb{N}\times\cdots\times\mathbb{N}$ and does not appeal to higher types of objects, such as numerical functions, proofs, etc. Whether or not this gives the proof a *strictly* finite character, it does seem to me to provide a sense in which we can say that certain numerical functions or proofs of equations, which apply to an infinite number of objects, are finite. It is certainly true that if we finitistically construct $f(X)$ from an arbitrary number X, then the construction applies to obtain $f(0), f(1)$, etc.— and, if we understand the former, then presumably we understand $f(n)$ for each given n. But that is not to say that the validity of the construction $f(X)$ or our understanding of it depends on the validity or our understanding of each of the infinite number of instances of it.

Of course, an analysis of the notion of finitism cannot be presented as a theorem. It is, rather, analogous to Turing's analysis of the notion of a computable real: at one end is an intuitive notion and at the other, its explication in terms of a precise mathematical concept. There are however at least two crucial differences between Turing's analysis and the analysis of the notion of

[2] He writes of "Hilbert's formalistic viewpoint": "For this viewpoint presupposes only the existence of a consistency proof in which nothing but finitary means of proof is used, and it is conceivable that there exist finitary proofs that *cannot* be expressed in the formalism of P (or M or A)."

finiteness. The most prominent difference concerns the relative importance of the notions; the other difference is that Turing's analysis has stood up over a long period of time.[3] Far too little attention has been paid to the explication of the idea of finitism for one to feel that my analysis is beyond doubt. But sofar I have seen no serious discussion of it; and so I'm not going to discuss this question further now.

Concerning the second, historical, sense of the question "What is finitism?", there has been some serious discussion: I mention in particular Zach's paper [31]. Hilbert nowhere gives a precise characterization of what he means by this; and, indeed, in [16] the authors, in discussing the extent to which finitist methods include the principles formalized in first-order arithmetic (PA), write

> To be sure, even as we have formulated it, this question is not precise; for we have not introduced the expression "finit" as a sharply delimited term, but only as a designation of a methodological guideline which, to be sure, enables us to definitively recognize certain kinds of concept formations and inferences as finitist, certain others as not finitist, but which provides no precise dividing line between those which satisfy the demands of finitist methods and those which do not. [16, p. 361]

However, there is general agreement, well supported by Hilbert's own writings, that he regarded the kinds of concept formation and inferences formalized in primitive recursive arithmetic (PRA) as finitist.[4] The substantive issue seems to be whether he was *committed* to there being concepts and modes of inference which are not formalizable in PRA. I will confine my attention to this question.

There are four sources of data relevant or at least thought to be relevant to the issue: various passages in the two volumes of *Grundlagen der Mathematik* [16, 17], the reference to Ackermann's function in "Über das Unendliche" [12] and Hilbert's so-called 'ω-rule' in "Grundlegung der elementaren Zahlenlehre" [14].

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[3] This is true both in the sense that his analysis of human computation and his argument that such computation can be carried out by a Turing machine has survived subsequent analysis and in the sense that the class of Turing-computable functions has turned out to contain all the functions that one might, from some other point of view, deem computable. For a discussion of the first sense, see [25]

[4] See in particular [16, p. 325 (330)]. Niebergal and Schirn [23] argue that the conception of finitism in [16] extends the original conception that Hilbert had in the 1920's. This view seems to be partly based on the view that finitism in the earlier period was concerned only with metamathematics and so not with numbers. But, of course, for Hilbert the numbers themselves were syntactic objects and so arithmetic was for him was a special case of metamathematics. The authors also seem to believe that Hilbert's finitism was restricted to making statements about particular syntactical objects and admitted no general statements (Π^0_1 statements, as they refer to them). Aside from the fact that they are then unable to give a convincing account of what the statement of consistency for a formal system such as PA would be (see pp. 297-302), [13] explicitly states the 'contentual' principle of mathematical induction as a finitist principle. One premise of this principle is surely a general proposition.

Grundlagen I

Zach mentions one passage in §7 of Volume I which he takes as evidence for the view that the authors took finitism to be more extensive than PRA.

> We may, however, admit certain *extensions of the schema of recursion* as well as of the induction schema, without taking away what is characteristic of the method of recursive number theory. [p. 325 (p. 330)][5]

The subsequent discussion is, first, of various forms of recursion and induction, e.g. simultaneous recursions, which reduce to the primitive forms. But they then go on to introduce examples of nested double recursive definition, namely the enumeration $f_x(y)$ of the primitive recursive functions of y and the Ackermann function, which they point out are unlike the previous examples in not being reducible to primitive recursion.[6] Did they at that time regard such nested double recursions, too, as partaking of what is characteristic of the method of recursive number theory? And, if so, does that imply that they thought at that time that such definitions are finitistically valid? Presumably Zach believes that "what is characteristic of the method of recursive number theory" refers to its finitistic character. But what immediately precedes the above passage is

> The distinction of recursive number theory from intuitive number theory consists in its formal constraints; its only method of concept formation, aside from explicit definition, is the schema of recursion, and also the methods of deduction are strictly circumscribed.

In view of this, I am inclined to take "what is characteristic of" its method to be the kind of definitions (recursion equations) and rules of inference (free-variable induction principles) that are involved. Just preceding the above passages, the authors state explicitly that recursive number theory, *as it had been developed up to that point*, is finitistically valid. Insofar as they can be reduced to PRA, this would then be so of what can be obtained by the various principles of definition and proof that they subsequently introduce in §7. However, nothing they write points clearly to an acceptance of the diagonal function or the Ackermann function as finitist. On the other hand, they don't explicitly reject them as finitist, either.

Grundlagen II

There is no doubt that in Volume II there is evidence that the authors regarded finitism as extending beyond PRA. Thus, Zach quotes a passage referring back to the passage just quoted from Volume I, in which they write

[5] Page references to both volumes of *Grundlagen der Mathematik* are to the first edition, with the corresponding page references to the second edition in parentheses.

[6] They prove that $f_x(x) + 1$ is not primitive recursive.

> Certain methods of finitist mathematics which go beyond recursive number theory (in the original sense) have been discussed already in §7 [of [16]], namely the introduction of functions by nested recursion and the more general induction schema. [17, p. 340 (354)]

But the question is whether or not this reflects their view of finitism in earlier writings, up to the publication of Volume I, which were unaffected either by Gödel's incompleteness theorems or by the Gentzen's success in proof theory using methods that go beyond PRA. Already in his introduction to [16], written in March 1934, after the completion of the text, in response to Gödel's incompleteness theorems, Hilbert spoke of the necessity for using the finitist standpoint in a sharper (*schärferen*) way than was required for the treatment of elementary formalisms. This would seem to indicate that he had in mind some resource that he regarded to be already contained in the finitist conception, rather than some more extensive conception, going beyond finitism. On the other hand, in the Introduction to Volume II, Bernays, in reference to the new methods needed to prove the consistency of less elementary formalisms such as PA, speaks of an *extension* of the finitist standpoint. I don't know whether this form of expression constitutes a recognition that these new methods go beyond finitism as originally conceived or whether, like Hilbert, he is refering to new resources contained within the original conception.

Further complicating the issue, on p. 224 the authors write about "contentual finite number theory, which", they say, "indeed is formalized through recursive number theory"; and here they refer back to Volume I. This would seem to contradict the later statement, quoted above, that finitism has no precise boundary; however they go on to write that

> The original narrow concept of a finitist proposition in the field of number theory admits as finitist number theoretic propositions only those which can be expressed in the formalism of recursive number theory [PRA], *possibly* including symbols for certain computable functions ... or which are capable of a stricter interpretation by a proposition of this form. (My emphasis) [p. 348 (362)]

This would seem to leave the boundary of finitism, as conceived in Volume I, indeterminate without commiting it to more than PRA.

But we also see here the expression "the original narrow concept of a finitist proposition", suggesting a wider conception that is still, on their view, finitist. Indeed, they go on to discuss certain propositions (e.g. implications with universal antecedents) the employment of which "appears not to be a violation of the basic ideas of the methodology (methodischen Grundgedanken) of finitist proof theory." In fact, they suggest that, having taken this step, one can extend the methods of finitist proof theory even further.

Certainly, from my own point of view, this amounts to a clear transgression of finitist mathematics, which admits as a limiting case, only, proofs of statements (i.e. functions of type) $\forall x[s(x) = t(x)]$. The authors are now contemplating functions of types $\forall x F(x) \Longrightarrow \forall x G(x)$ *which take functions (or proofs) as arguments*. At this stage, I don't know what the authors mean by "finitism": I certainly can't understand what it has to do with finite.

Zach does quote from a letter from Bernays to Gödel from 1970:

> These nested recursions ... appear to me to be finite in the same sense as the primitive recursions, i.e., if one regards them as statements of computation procedures where one can recognize that the function defined by the respective process satisfies the recursion equations (for every system of numerical values for the arguments). Indeed, the computation of the value of a function according to a nested recursion, when the numerical values of the arguments are given, comes down to the application of several primitive recursions, the number of which is determined by a numerical argument.

But one should note, certainly with admiration but also as a caution, that Bernays wrote this in a letter when he was 82 years old. And, in any case, his reasoning here is defective. To understand his point about the computation of the Ackerman function $f(x,y)$ amounting to the computation of x primitive recursive functions of y, let $f(x,y)$ instead be the enumeration $f_x(y)$ of the primitive recursive functions of y mentioned above. Now, it is true that, for any particular number m, we can construct $f_m[Y]$ from arbitrary Y; but how are we to construct $f_X[Y]$ from arbitrary X and Y? On my analysis, a *particular* primitive recursive function is finitist, but not an 'arbitrary' one f_X. The construction or computation of $f_X[Y]$ from X and Y requires, not the arbitrary iterations X and Y, but the transfinite iteration up to $\omega^X \times Y$, which can make no claim to finiteness.[7]

Über das Unendliche

My original foray into history concerned a reference to the Ackermann function by Hilbert in [12], which Kreisel cites in [21, fn 42, p. 514] as evidence against my thesis that finitism is PRA. I responded to this in [28]; but the matter seems not to be entirely laid to rest. So let me expand on it and convince you that *this* issue is not at all open!

In his paper, Hilbert wanted to sketch, as an application of his proof theory, a proof of the continuum hypothesis CH, and a key to understanding what he intends is to remember that his proof theory involved formalizationj in the ϵ-calculus and then proving the eliminability of all ϵ-terms. He assumed that a certain formal system Σ, in which CH can be formulated, is complete; and so it sufficed to prove that $\neg CH$ cannot be derived in Σ. Σ is a many-sorted theory containing the types N of the second number class and Z of the first number class. The negation of CH can be expressed by

[7] See [26] for the analysis of nested double recursions (i.e. nested recursions on ω^2) or, more generally, nested α-recursions.

$$\forall F : N \Longrightarrow Z^Z \exists g \forall \alpha [F(\alpha) \neq g]$$

(To improve readability, I am using arrows and exponentiation interchangeably.) Using ϵ-symbols, this takes the form

$$\forall F : N \Longrightarrow Z^Z \forall \alpha [F(\alpha) \neq t(F)]$$

where $t = t(F)$ is a term for a numerical function, built up by means of the ϵ-operator from F. He now invokes his Lemma I, which he states to be a conseqence of his general principle that every mathematical problem can be solved:

> If a proof of a proposition contradicting the continuum theorem is given in a formalized version with the aid of functions defined by means of the transfinite symbol ϵ ..., then in this proof these functions can always be replaced by functions defined, without the use of the symbol ϵ, by means merely of ordinary and transfinite recursion ... .[8]

By ordinary and transfinite recursions, he means recursions on Z and on N, respectively. So now we may assume t to be defined by recursions on Z and N and explicit definitions from F. The recursions involved cannot all be assumed to be of the form of primitive recursions; but Hilbert believes that all the recursions involved in the proof can be reduced to primitive recursions in the impredicative sense, if we admit the transfinite hierarchy $\langle N_\alpha \mid \alpha \in N \rangle$ of function types, where $N_0 = N$. *The Ackermann function is introduced at this point as a simple example of a numerical function 'defined by recursion' but not by predicative primitive recursion, and which can then be defined by impredicative primitive recursion on Z.* Call the functions obtained by impredicative primitive recursion relative to this hierarchy $N - PR$. So, $t(F)$ is an $N - PR$ function of F. Now we may define another hierarchy $\langle Z_\alpha \mid \alpha \in N \rangle$, where $Z_0 = Z$, obtaining also the class of $Z - PR$ functions.[9] The class of $Z - PR$ functions has the power of N and so we can take F to be an enumeration of all the $Z - PR$ numerical functions, itself defined by recursion on N. So $t = t(F)$ is a $N - PR$ numerical function. Hilbert gives an argument to show that any $N - PR$ numerical function and, in particular, t is $Z - PR$ and hence in the range of the enumeration F—a contradiction.

Hilbert goes on to say that, even aside from the holes in the argument that need filling, the proof would require "a recasting strictly faithful to the finitist attitude". Clearly what he has in mind is the formalization Σ of the theory of the second number class N, including the theories of the $N - PR$ and $Z - PR$ functions, and the translation of the proof of $\neg CH$ in the ϵ-calculus into a

[8] This assertion has of course never been proved. In [13] he asserts that it is dispensible for his proof of CH; but it isn't clear to me how he intended to establish this.

[9] This is Gödel's hierarchy extended into the transfinite.

proof in Σ of a contradiction. Since he presumably believed that a finitist proof of the consistency of Σ could be obtained, he could conclude that there can be no proof of $\neg CH$ and, hence, by completeness, CH is true.

Hilbert's so-called *ω-Rule*

There is also a prevailing confusion concerning Hilbert's paper "Die Grundlegung der elementaren Zahlenlehre" [14], in which he proposes the following "Schlussregel":

> If it is proved that the formula
> $$A(z)$$
> is a true numerical formula for each given numeral z, then the formula
> $$\forall x A(x)$$
> may be admitted as an initial formula.

Gödel reviewed this paper and, in the introductory note to the review in [9], Feferman discusses the question of whether or not Hilbert was here reacting to Gödel's incompleteness theorems. But, more to the point, he notes that the principle in question is mis-named the ω-rule. In the first place, the formula $A(x)$ is to be quantifier-free; in fact, it is to be the formalization of a finitistically meaningful property of numbers. But, secondly, in spite of Hilbert calling it a "Schlussregel", it is not even a restricted case of the ω-rule. Rather, it provides a criterion under which the universal formula $\forall x A(x)$ may be taken as an initial formula—i.e. an *axiom* in a deduction. Hilbert is describing a system Σ, which is obtained from the formal system of elementary number theory, with definition by primitive recursion included, by admitting such axioms. Of course, Σ is not itself a formal system—*unless we accept the thesis that finitism* $= PRA$ or some other formal system. But it is misleading to call it a *quasi-formal* system, at least as that term was introduced by Schütte for deductions admitting the ω-rule. One might note also that it is precisely this system Σ which Gentzen described in [3] and to which he applied his consistency proof.

Conclusion

Sofar, on the evidence I have seen, I remain inclined towards the view that there was no commitment to finitist principles that go beyond PRA in Volume I or in earlier writings. In Volume II, by their own admission, the authors are including more under the term "finitist" than was originally included. Perhaps further evidence will settle the historical question decisively.[10]

[10]I confess that, up to the time of my talk at Feferfest and aside from noting the passage from p. 224 quoted above and recalling in a general way that the authors had extended the notion of finitism, I hadn't really looked thoroughly at Volume II to see what light it

REFERENCES

[1] P. Bernays, *Die Philosophie der Mathematik und die Hilbertsche Beweistheorie*, ***Blätter für deutsche Philosophie***, vol. 4 (1930-31), pp. 326–367, Reprinted in [2]. A translation by P. Mancosu appears in[22], pp. 234-265.

[2] ———, ***Abhandlungen zur philosophie der mathematik***, Darmstadt: Wissenschaftliche Buchgesellschaft, 1976.

[3] G. Gentzen, *Die Widerspruchfreiheit der reinen Zahlentheorie*, ***Mathematische Annalen***, vol. 112 (1936), pp. 493–565.

[4] K. Gödel, *Über formal unentscheidbare Sätze der* Principia Mathematica *und verwandter Systeme I*, ***Monatshefte für Mathematik und Physik***, vol. 38 (1931), pp. 173–198.

[5] ———, *The present situation in the foundations of mathematics*, ***[11]***, *1933o.

[6] ———, *Lecture at Zilsel's*, ***[11]***, *1938a.

[7] ———, *Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes*, ***Dialectica***, vol. 12 (1958), pp. 280–287, Reprinted with an Englsh translation in [10]. [8] is a revised version.

[8] ———, *On an extension of finitary mathematics which has not yet been used*, ***[10]***, 1972, Revised version of [7].

[9] K. Gödel, ***Collected works, vol. i***, Oxford: Oxford University Press, 1986.

[10] ———, ***Collected works, vol. ii***, Oxford: Oxford University Press, 1990.

[11] ———, ***Collected works, vol. iii***, Oxford: Oxford University Press, 1995.

[12] D. Hilbert, *Über das Unendliche*, ***Mathematische Annalen***, vol. 95 (1926), pp. 161–90, Translated by Stefan Bauer-Mengelberg in [29, 367-92].

[13] ———, *Die Grundlagen der Mathematik*, ***Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität 6 (1928)***, (1927), Translated in [29] pp. 464-479.

[14] ———, *Die Grundlegung der elementaren Zahlenlehre*, ***Mathematische Annalen***, vol. 104 (1930), pp. 485–494, Reprinted in part in [15].

[15] ———, ***Gesammelte abhandlungen***, vol. 3, Berlin: Springer, 1935.

[16] D. Hilbert and P. Bernays, ***Grundlagen der mathematik i***, Berlin: Springer-Verlag, 1934, The second edition was published in 1968.

[17] ———, ***Grundlagen der mathematik ii***, Berlin: Springer-Verlag, 1939, A second edition was published in 1970.

[18] A. Ignjatović, *Hilbert's program and the omega rule*, ***The Journal of Symbolic Logic***, vol. 59 (1994), pp. 322–343.

[19] A. Kino, J. Myhill, and R. Vesley (editors), ***Intuitionism and proof theory***, Amsterdam: North-Holland, 1970.

[20] G. Kreisel, *Ordinal logics and the characterization of informal notions of proof*, ***Proceedings of the International Congress of Mathematicians, Edinburgh***, (1958), pp. 289–299.

[21] ———, *Principles of proof and ordinals implicit in given concepts*, ***[19]***, (1970), pp. 489–516.

[22] P. Mancosu (editor), ***From brouwer to hilbert: The debate on the foundations of mathematics in the 1920's***, Oxford: Oxford University Press, 1998.

[23] K-G. Niebergall and M. Schirn, *Hilbert's finitism and the notion of infinity*, ***In [24]***, (1998), pp. 271–306.

might shed on the earlier conception of finitism; and, in particular, although it had been pointed out to me by Howard Stein shortly after the publication of [28], I had forgotten the reference to nested double recursive functions in the passage on p. 354 until it was pointed out again by Zach. I have since sought expiation by resigning all of my memberships in historical associations.

[24] M. Schirn (editor), ***The philosophy of mathematics today***, Oxford: Clareendon Press, 1998.

[25] W. Sieg, *Calculations by man and machine*, ***Technical report***, Carnegie Mellon, 2000.

[26] W.W. Tait, *Nested recursion*, ***Mathematische Annalen***, vol. 143 (1961), pp. 236–250, .

[27] ———, *Constructive reasoning*, ***[30]***, (1967), pp. 185–199.

[28] ———, *Finitism*, ***Journal of Philosophy***, vol. 78 (1981), pp. 524–556.

[29] J. van Heijenoort (editor), ***From frege to gödel: A source book in mathematical logic***, Cambridge:Harvard University Press, 1967.

[30] B. van Rootselaar and J.F. Staal (editors), ***Logic, methodology and philosophy of science iii***, Amsterdam: North-Holland, 1968.

[31] R. Zach, *Numbers and functions in Hilbert's finitism*, ***Taiwanese Journal for Philosophy and History of Science***, vol. 10 (1998), pp. 33–60.