Kreisel's "unwinding" program

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1 *Introduction.*

Through his own contributions (individual and collaborative) and his extraordinary personal influence, Georg Kreisel did perhaps more than anyone else to promote the development of proof theory and the metamathematics of constructivity in the last forty-odd years. My purpose here is to give some idea of just one aspect of Kreisel's contributions to these areas, namely that devoted to "unwinding" the constructive content of *prima-facie* non-constructive mathematical proofs.[1] This program was the subject of his first remarkable papers in the 1950's, and it has drawn his repeated attention ever since.

Anyone who is familiar with even a small part of Kreisel's writings knows that he inverts the usual ratio of technical work to discussion. He takes much greater pains to explain, at length, the significance of the work than to set it out in an organized step-by-step fashion. His attitude seems to be that if one has the right ideas, the details will look after themselves. And they did amazingly often (or, he could rely on more disciplined collaborators to look after them). However, in the specific area dealt with here, there are several important cases where the expected details are either problematic or simply missing. This can't help but affect my main aim here, which is to assess whether the work on Kreisel's unwinding program, both his own and that of others, lives up to its claims. My overall conclusion is that while the general *theory* of unwinding launched by Kreisel is eminently successful, the supposed *applications* to date are few and far between; moreover, in some prominent cases, their status *even as applications* has to be put in question.

[1]The full range of Kreisel's contributions to proof theory and constructivity certainly deserves exposition and critical evaluation, but providing such would require a *much* more substantial investment of effort than that which was found possible here.

Kreisel's unwinding program was a reaction to Hilbert's consistency program. It aimed to substitute clear mathematical results for what were said to be vague, misplaced, crude foundational goals. But, as with his work on constructivity, Kreisel also sought to replace those by a more sophisticated stance about foundations, to be advanced by the technical results. In the end we must ask, as well, to what extent he was successful in doing so.

I believe that the general direction and character of Kreisel's contributions were influenced considerably by his early studies. Kreisel commenced university studies in mathematics at Trinity College, Cambridge in 1942, at the age of 19. He reports in several places (e.g. *1987*, p. 395) having read the second volume of Hilbert and Bernays' *Grundlagen der Mathematik* in that same year, so the level of his logical sophistication and direction of interests was already exceptional then. In mathematics, Cambridge was noted for its predominant concentration on analysis and number theory, with such famous exemplars as Hardy, Littlewood, and Besicovitch. There were no logicians on the faculty (Russell was long gone), but some reinforcement of Kreisel's interests in logic and foundational matters was to be found in the lectures he attended on the philosophy of mathematics offered by his compatriot, the stormy petrel Ludwig Wittgenstein. According to *Monk 1990* (p. 498), in 1944 Wittgenstein declared Kreisel to be "the most able philosopher he had ever met who was also a mathematician."

During the war, Kreisel joined the British Admiralty in 1943 where he worked on problems of hydrodynamics and naval engineering until 1946. This experience with applied mathematics no doubt honed his skills with classical analysis but may also have encouraged a casual disregard for mathematical fastidiousness. Kreisel returned to Cambridge as a Research Fellow in 1946–48, receiving an M. A. in 1947. During this period he engaged in regular discussions with Wittgenstein on the philosophy of mathematics, but in later years he was extremely critical of Wittgenstein's ideas in this respect (cf. *Monk 1990*, p. 499 and the references to K. on W. *op. cit.* p. 642). For the details of the further progress of Kreisel's career, cf. his C. V. in this volume.

2 *The interpretation of non-finitist proofs.*

The title of this section is taken from that of the fundamental paper for Kreisel's program (K. P.) for unwinding non-constructive proofs; it appeared in *The Journal of Symbolic Logic* in two parts, *Kreisel 1951/52.* This work modifies Hilbert's program (H. P.) in several important respects but initially retains some of the same language in a way that does not adequately convey the shifts of emphasis. Later, in *Kreisel 1958*, improved ways were used for describing what is to be accomplished, and thus *1958* supersedes *1951/52* to a certain extent. However, to respect the historical progression of ideas, we shall not mix the two, but return to the 1958 paper in Section 6 below.

For both H. P. and K. P. there is a basic syntactic distinction between free-variable (quantifier-free) formulas A_0 and those formulas A which contain bound variables. H. P. restricts attention to decidable A_0 with *free individual variables* $x, y, \ldots$ ranging over the natural numbers. Kreisel allows consideration of A_0 containing, in addition, *free function variables* $f, g, \ldots$; these are supposed to be decidable for each substitution instance by specific numerals and computable functions. A_0 is said to be *verifiable* if it is correct for each such substitution instance.

A necessary, but clearly not sufficient, condition on finitist proofs is that they consist of verifiable free variable formulas. Kreisel is careful to avoid claiming any characterization of finitist proofs (*1951* p. 242, ftn. 2).[2] At any rate, proofs which contain formulas with bound variables (even just individual variables) are *non-finitist* on their face. The Hilbert program aimed to establish the consistency of formal systems for arithmetic, analysis, etc., by finitist reasoning. For systems containing a modicum of arithmetic, this would insure that free variable formulas $A_0(x_1, \ldots, x_n)$ established in such formalisms by non-finitist proofs (i. e. which contain some formulas with bound variables) are verifiable and, indeed, finitistically provable. However, even where H. P. succeeds, this tells us nothing about proofs of formulas A with bound variables. The "problem of non-finitist proofs" that Kreisel poses in *1951/52* is how to give "finitist sense" to such formulas.

To make this more precise, and to avoid the problem of saying just what is finitist (skirted as indicated above), Kreisel introduces the idea of an *interpretation*, which applies to systems Σ within an effectively specified formal language $\mathcal{L}(\Sigma)$; Σ itself need not be effectively given (for reasons to be explained below). An interpretation of Σ is taken to be an effective association with each formula A in $\mathcal{L}(\Sigma)$ of a sequence of free variable formulas $A_0^{(n)}$ such that:

[2]But in *Kreisel 1960* he would later propose a formal characterization of the notion of finitist proof; cf. *Kreisel 1987*, p. 396 for one of his more recent assessments of that venture.

(I_1) for each proof of A in Σ we can find an n such that $A_0^{(n)}$ is verifiable,

(I_2) for each proof of $\neg A$ in Σ and each n we can find a substitution instance which makes $A_0^{(n)}$ false.

There is also an obvious third condition which relates the interpretation of B to that of A when B is proved from A in Σ.

The best known example of an interpretation is provided by Herbrand's Theorem for the classical 1st order predicate calculus Σ. This associates with each formula A a sequence of formulas $A_0^{(n)}$ each of which is a finite disjunction of substitution instances of the quantifier-free matrix of A when A is taken in prenex form; details of the association will be recalled in the next section. It turns out that Herbrand's Theorem applies to extensions Σ of the predicate calculus by arbitrary verifiable formulas; these need not be effectively given.

Remark. The above notion of interpretation is too broad to be really useful as a general theoretical tool. As Kreisel points out, every usual formal system Σ with decidable proof predicate $\mathrm{Prf}_\Sigma(x, y)$ ("x is the number of a proof in Σ of the formula with number y") admits a *trivial interpretation* of each formula A by a single formula $A_0(x)$, namely $\neg\ \mathrm{Prf}_\Sigma(x, \ulcorner \neg A \urcorner)$. One would want the $A_0^{(n)}$ in an interpretation to have a closer contentual relation to A, and to be informative about that content. In this respect, the modified notion of interpretation introduced in *Kreisel 1958* is an improvement; cf. Sec. 6 below. At any rate, (I_1), (I_2) serve for initial orientation to what is to be accomplished.

3 *The no counterexample interpretation.*

The main result of *Kreisel 1951/52* is to provide an informative recursive interpretation of classical 1st order arithmetic and its extension by verifiable free variable formulas. This is the so-called *no counterexample interpretation* (n. c. i.). Its form is illustrated by Π^0_3 formulas A, which in any case cover the specific applications to be discussed later. So let us consider

(1) $\quad A \equiv \forall x \exists y \forall z R(x, y, z)$

with R quantifier-free. The n. c. i. is to be contrasted with the "naive" interpretation of A, which proceeds via its *Skolem normal form*, the 2nd order formula

$(1)_S \quad \exists f \forall x, z R(x, f(x), z)$.

The naive interpretation seeks to find a computable f satisfying $(1)_{\mathrm{S}}$ when (1) is proved. Simple recursion-theoretic examples show that, in general, this cannot be done when $\forall x \exists y \forall z R(x, y, z)$ is proved in arithmetic.[3]

In the classical predicate calculus, Skolem normal form is the appropriate one to consider for *satisfiability*; its dual, the *Herbrand normal form*, in this case

$(1)_{\mathrm{H}} \quad \forall x, f \exists y R(x, y, f(y)),$

is the more appropriate one for *validity* and hence (by completeness) for derivability. This is equivalent to

(2) $\neg \exists x, f \forall y \neg R(x, y, f(y))$

which, by Skolem form, is equivalent to

(3) $\neg \exists x \forall y \exists z \neg R(x, y, z),$

i.e. to A itself. A pair x, f such that $\forall y \neg R(x, y, f(y))$ would provide a *counterexample* to A; hence (2) and so, also $(1)_{\mathrm{H}}$ can be read as asserting that *there is no counterexample to A*. The Herbrand form and its obvious generalization to arbitrary prenex A is the common formal starting point of both Herbrand's Theorem (H. T.) and Kreisel's n. c. i. for arithmetic.

Let us recall, briefly, some details of H. T. [4] For Σ the classical predicate calculus, extend the language $\mathcal{L}(\Sigma)$ by new free function variables $f, g, \ldots$. Then it is shown that A is derivable in Σ just in case the 1st-order version of its Herbrand form, i. e.

(4) $\exists y R(x, y, f(y))$

is derivable; moreover, that — according to H. T. — holds just in case there is a finite disjunction of substitution instances

(5) $R\left(x, t_1, f\left(t_1\right)\right) \vee \ \ldots \ \vee R\left(x, t_k, f\left(t_k\right)\right)$

which is tautologous. Note that the t_i in (5) are built up by the function symbols in R together with f from the variable x and constants in R. These can be assumed to be ordered in such a way that when new variables $z_1, \ldots, z_k$ are substituted for $f(t_1), \ldots, f(t_k)$, resp., the $t_1, \ldots, t_k$ are transformed into terms $s_1, \ldots, s_k$, such that the free variables of $s_i (i = 1, \ldots, k)$ are contained in $\{x, z_1, \ldots, z_{i-1}\}$. That is, we have a disjunction

(6) $R(x, s_1(x), z_1) \vee R(x, s_2(x, z_1), z_2) \vee \ldots \vee R(x, s_k(x, z_1, \ldots, z_{k-1}), z_k)$

which is tautologous when A is provable; moreover, A is derivable from each instance of (6) by Herbrand's direct rules for the predicate calculus. Note

[3] The formula $\forall x \exists y \forall z [T(x, x, y) \vee \neg T(x, x, z)]$ using Kleene's T predicate provides one example; Kreisel gives an example from elementary analysis in Appendix I to *1952*.

[4] Cf., e. g., *Shoenfield 1967*, pp. 52–55.

that the formulas (6) lie back in the original language $\mathcal{L}(\Sigma)$.

Granted these facts about H. T., Kreisel shows straightforwardly in *1951* that the sequence $A_0^{(n)}$ of all possible disjunctive substitution instances of the form (6) constitutes an interpretation (in his sense, as described in sec. 2) of the predicate calculus. He then goes on to give, as a second such interpretation, the n. c. i. for the predicate calculus, by returning to the form (4). Basically, this is to regard the choice of y satisfying $R(x, y, f(y))$ for arbitrary x, f as given by explicitly defined *functionals*

(7) $y = F(f, x)$[5]

which are found through those disjunctions (5) that are tautologous. Namely, y may be taken to be $t_i(\equiv t_i(x, f))$ for the first i such that $R(x, t_i, f(t_i))$ holds. The n. c. i. for the predicate calculus simply associates with each A of the form (1) the sequence $A_0^{(n)}$ of all possible formulas of the form

(8) $R(x, F(f, x), f(F(f, x)))$

in which the F are specific explicit functionals built from terms of $\mathcal{L}(\Sigma)$ using definition by cases.[6] Kreisel sketches a proof of this in *1951* for arbitrary prenex A, using, instead of H. T., the Hilbert ε-calculus and results about the Hilbert substitution method (for it) from *Hilbert and Bernays 1939*.

The n. c. i. for arithmetic takes on a similar shape, only the description of the functionals required as well as the proof are more involved. Here Kreisel makes use of *Ackermann 1940*, which extended to arithmetic the substitution method for the ε-calculus, with termination proved by effective transfinite induction up to Cantor's ordinal ε_0, the first fixed point of $\omega^\alpha = \alpha$.[7] What Kreisel's result shows is that if a prenex formula A is provable in arithmetic then its n. c. i. functionals can be defined using primitive recursive schemata and schemata for effective transfinite recursion up to any ordinal $\alpha < \varepsilon_0$. In the case of a Π_3^0 formula A as in (1), these functionals may specifically be determined by

(9) $F(f, x) = (\mu y)R(x, y, f(y))$

for all (f, x). Since transfinite induction up to each ordinal $\alpha < \varepsilon_0$ is provable in arithmetic, Kreisel's result gives a complete characterization of the n. c. i. functionals for that system.

Kreisel further observes — and stresses repeatedly — that for both the

[5] More mnemonically, one might write $y = Y(f, x)$.

[6] That is, the (if...then...else...) construction.

[7] Such use of induction up to ε_0 had first been applied by Gentzen (*1936*) to obtain a consistency proof of arithmetic by means of a partial extension of his earlier cut-elimination procedure for the predicate calculus.

predicate calculus and arithmetic, the class of functionals needed for the n. c. i. is unaffected by the adjunction of purely universal (i.e. Π^0_1) axioms B. For if A follows from B, then $B \to A$ is provable in the system and hence so also is $\neg B \vee A$; passing to the prenex form for the latter and applying the n. c. i. to that gives the desired result. For example, if $A \equiv \forall x \exists y \forall z R(x,y,z)$ and $B \equiv \forall u S(u)$, with S quantifier-free, then $\neg B \vee A$ goes into $\forall x \exists u, y \forall z\, [\neg S(u) \vee R(x,y,z)]$, and its Herbrand form is $\forall x, f \exists u, y\, [\neg S(u) \vee R(x,y,f(y))]$. This leads to two functionals $F(f,x), G(f,x)$, with $\neg S(G(f,x)) \vee R(x, F(f,x), f(F(f,x)))$. Hence $R\,(x, F\,(f,x), f(F(f,x)))$ holds if $\forall u S(u)$ holds, and indeed, if $S(m)$ holds for some one suitable m (depending on x and f). The functionals F, G lie in the same classes as described before for the predicate calculus, resp. arithmetic. Note that it is the addition of arbitrary (verifiable) Π^0_1 axioms which gives rise to the non-effective systems Σ allowed in Kreisel's notion of interpretation in Sec. 2 above.

The special case in all this of Π^0_2 formulas $\forall x \exists y R(x,y)$ provable in arithmetic (possibly extended by arbitrary Π^0_1 axioms) is of particular interest. Since the variable 'z' in (1) is missing, the functional F in (9) simply reduces to a function

(10) $F(x) = (\mu y) R(x,y)$.

Then F can be defined by function schemata for primitive recursion and ordinal recursion up to ordinals $\alpha < \varepsilon_0$. Since the provable Π^0_2 formulas of arithmetic correspond exactly to its provably recursive functions, one arrives at the following:

> **Theorem** (*Kreisel 1951/52*). *The provably recursive functions of arithmetic are exactly those which are ordinal recursive of order* $< \varepsilon_0$. *Moreover, the same holds of any consistent extension of arithmetic by* Π^0_1 *axioms.*

4 *Proofs of the n. c. i.*

While the nature of the n. c. i. and results for it are clear enough, the proofs are quite another matter. Kreisel's own proof of the n. c. i. for arithmetic requires familiarity with the ins and outs of Hilbert's ε-calculus and with its application in *Ackermann 1940*. For those few brought up on *Hilbert and Bernays 1939* (as, e.g., Kreisel himself) this would not be an obstacle. But for the rest of us, the proof in *Kreisel 1951/52* does not invite detailed study; it just looks like a thicket. I myself have never tried to wade through it, and don't know anyone who has.[8] The review by J. Barkley Rosser, *J. Symbolic Logic* 18 (1953), pp. 78–79, is extremely critical on a number of scores, to begin with that "...it is very difficult reading. In the first place, the subject matter is of considerable complexity. In the second place, there are many errors, too many to permit a complete listing... Most are typographical... ." Then, "[T]he word 'finitist' is itself used in an unfamiliar sense... and furthermore this sense is never carefully explained... . Apparently 'finitist' means 'constructive' with the additional restriction that no bound variables shall occur." As we shall see, Kreisel would deal with this objection in his *1958* paper.

Finally, in his review Rosser registered disappointment with the nature of the n. c. i. itself, pointing to provable Σ^0_2 sentences $\exists x \forall y R(x,y)$ in particular. For these, he said, one would wish to find a specific constant k such that $R(k,y)$ holds for all y, and that doing so "...would be of great value, since it would enable one to transform one of the existing non-constructive proofs of Siegel's theorem into a constructive proof, which is badly needed." Instead, the n. c. i. only gives an $x = F(f)$ such that $R(F(f), f(F(f)))$ holds for all f. Presumably, Rosser was here referring to a theorem of C. L. Siegel guaranteeing that certain kinds of diophantine equations have at most a finite number of solutions. Kreisel would deal with this general objection some thirty years later in his paper *1982*, as will be discussed in Sec. 7 below.

The first new proof of the n. c. i. for arithmetic was obtained via Gödel's functional (*"Dialectica"*) interpretation of intuitionistic arithmetic (*Gödel 1958*), as was pointed out by Kreisel himself in his paper *1959*. This route breaks up the work into several more understandable steps: first (i) the negative translation of classical into intuitionistic arithmetic, then (ii) the application of Gödel's interpretation to the translations of prenex formulas, and finally (iii) ordinal analysis of the resulting functionals. The first two steps are quite direct, and show that the n. c. i. functionals for arithmetic, which lie at type level 2 in the type hierarchy, are generated by the schemata

[8] I asked my colleague, Grigori Mints, an expert on Hilbert's ε-substitution method, whether he had ever studied Kreisel's proof. He said that he had not because by the time he learned of the significance of the results, there were more understandable proofs available [see below]. After writing the above, I learned from Charles Parsons that back in the 50's he had managed to work his way through the proof in *Kreisel 1951/52*, though only by relying on *Ackerman 1940* to help fill in the details.

for Gödel's primitive recursive functionals of finite type.[9] The third step, which shows that every type 2 primitive recursive functional is ordinal recursive of ordinal $< \varepsilon_0$, is established in *Schwichtenberg 1975*, following earlier work of *Tait 1967*.

As an aside, the history of the development of Gödel's functional interpretation is of incidental interest here, as explained in *Feferman 1993.* Gödel had arrived at this interpretation by 1941, when he lectured on it at Yale University, though he did not publish the work until 1958. In the 1930s, Gödel had explained several possible routes to obtain a constructive consistency proof for arithmetic going as little as possible beyond finitist methods. In a lecture he gave in 1938 for an informal seminar organized in Vienna by Edgar Zilsel, he explained a way of looking at Gentzen's consistency proof for arithmetic which is a clear anticipation of the n. c. i. Shorthand notes for this lecture were found in Gödel's *Nachlass* and transcribed as part of the Gödel editorial project a few years ago. These lecture notes appear in the third volume of Gödel's *Collected Works* (*1994*), together with an illuminating introductory note by Wilfried Sieg and Charles Parsons.

For those versed in Gentzen-style proof theory, the most direct and perhaps cleanest route to the n. c. i. for arithmetic make use of effective infinitary versions of cut-elimination. An excellent exposition of such is to be found in *Schwichtenberg 1977*, with application to the provably recursive functions of arithmetic and the n. c. i. *op. cit.* pp. 884–892.[10]

5 *Application to Littlewood's theorem.*

The final part of *Kreisel 1952*, pp. 51–65, is supposed, in his words, to provide an application "...of the ideas of the [n. c. i.] to a theorem of analytic number theory whose interpretation has given trouble", namely the result of Littlewood in 1914 that the functions of integers

(1) $\psi(x) - x$, and

(2) $\pi(x) - \mathrm{li}(x)$,

change sign infinitely often. Here $\psi(x)$ is the log of the l. c. m. of numbers $\leq x$, $\pi(x)$ is the number of primes $\leq x$ and $\mathrm{li}(x)$ is the logarithmic integral $\int_0^x (1/\log u)du$ (treated as an improper integral at $u = 1$), which is asymptotic

[9]Cf. *Troelstra 1990* p. 225 and, for more details with a variant of Gödel's interpretation, *Shoenfield 1967* pp. 223–227.

[10]Cf. also *Girard 1987* pp. 481–482.

to $\pi(x)$. Littlewood's theorem for (1) is evidently in the form $\forall x \exists y R(x, y)$ with R (primitive) recursive; the same holds for (2) by use of suitable recursive approximations for $\mathrm{li}(x)$. Littlewood's result was surprising since as far as had been calculated, $\pi(x) < \mathrm{li}(x)$; cf. p. 7 of *Ingham 1932*[11], which shows representative values up to $x=$ 10 billion.

Littlewood's proof of his theorem exposited in *Ingham 1932*, Ch. V, is non-constructive: in both (1) and (2) existence of a $y > x$ is demonstrated for which a sign change occurs; a bound for y is given explicitly in case the Riemann Hypothesis (R. H.) is false, but a different argument for the existence of y is used if R. H. is true, and no explicit bound for y emerges in that case (see also *Littlewood 1948*). As Ingham put it (op. cit. p. 7), "Littlewood's theorem is a pure 'existence theorem' and we still know no numerical value of x for which $\pi(x) > \mathrm{li}(x)$." That situation was shortly remedied by S. Skewes in 1933 who used *a different proof* of Littlewood's theorem for $\pi(x) - \mathrm{li}(x)$, assuming R. H., to put a bound of $10_3(34)$ on the first change of sign, where $10_1(n) = 10^n$ and $10_{i+1}(n) = 10^{10_i(n)}$; later work by others from the 1960s on (beginning with *Lehman 1966*) lowered this bound considerably (cf. *Ingham 1990* p. ix). What Kreisel claimed to do in *1952*, pp. 51–52, was analyze *the original proof* of Littlewood's theorem to show how one could extract recursive bounds, using the idea of the n. c. i. applied to statements of the form $\forall u S(u) \rightarrow \forall x \exists y R(x, y)$, where $\forall u S(u)$ is a Π^0_1 equivalent of the R. H.[12] Informally, this was to revolve around the feature of the n. c. i. that $S(m)$ would only be needed for finitely many m; as Kreisel put it (*1952* p. 54), "if the conclusion...holds when the Riemann hypothesis is true, it should also hold when the Riemann hypothesis is nearly true: not *all* zeros [of the Riemann zeta function] need lie on $\sigma = 1/2$, but only those whose imaginary part lie below a certain bound...and they need not lie *on* the line $\sigma = 1/2$, but near it."

The nature of this application is, however, obscure in a number of respects, despite the claim made for it in an introductory section to *Kreisel 1951*, p. 247: "Since [Littlewood's] proof was not developed in a formal system there can be no question of applying the results of the present work to it in a precise sense. But if one examines the official proof in [*Ingham 1932*]

[11] Ingham's famous 1932 expository monograph on the distribution of prime numbers has been reprinted without change in 1990; that edition contains an additional foreword by R. C. Vaughan with supplementary up-to-date information and references.

[12] Curiously, it is not explicitly stated in *Kreisel 1952* that the R. H. can be put in Π^0_1 form; this was only brought out in his 1958 paper discussed in Sec. 6 below.

(and the usual proofs of the standard theorems on complex variables used)... it might fairly be said, I think, that it applies the principles of [the n. c. i.] in a straightforward manner without introducing 'new ideas' of proof." First of all, even though the original proof was not presented in a formal system, one would want to know whether it can be formalized in arithmetic, at least in principle. Indeed, Kreisel says (*1952*, p. 52) that we could be sure of finding the required bounds if the proof were written out in one of the extensions of arithmetic that he considers. He says that he has "...discussed elsewhere [no reference given] how proofs in large parts of the theory of functions of a complex variable can be presented in [the system] Z_μ [of arithmetic]. Here we shall only give a method of constructing rational approximations to zeros of computable regular functions. This will enable us to deal with the present problem if we remember our familiar principle [about the n. c. i. for implications with Π^0_1 hypotheses]." He then launches into a description of such methods which seem closely related to the work on constructive approximations to zeros of analytic functions from *Kreisel 1952a*. The work on this purported application then goes on to sketch some modifications of the proof of Littlewood's theorem in *Ingham 1932*, Ch. V, making use of those constructive methods of approximation.[13] However, no further indication is given as to whether or how any of this is to be formalized in arithmetic.[14] Nor is any statement made as to what precise conclusion one could draw if it were taken for granted that it could all be thus formalized, so that one could apply the n. c. i. for arithmetic to it. Thus its status as an application of the logical work is in question. Finally — setting aside the logical aspects (or lack thereof) — just considered as a piece of work in analytic number theory, no explicit bounds are extracted that one can point to, certainly none that would satisfy the mathematicians interested in such matters. Indeed, one is hard put to say exactly what the conclusion of this work is; of this, more in the next section.

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[13] This presumes familiarity on the reader's part with the exposition loc. cit.

[14] Later work by a number of researchers, including H. Friedman, G. Mints, and S. Simpson, has shown that the bulk of classical analysis of the sort applied in *Ingham 1932* can in fact be carried out in systems conservative over primitive recursive arithmetic (PRA); cf. *Feferman 1988* for references. Further direct evidence for the formalization in PRA of complex analysis applied to number theory is provided by *Cegielski 1992*.

6 *Pushing the program.*

Kreisel's program (K. P.) is formulated in his paper *1958* in an improved way, without the misleading reference to finitism, as follows (p. 155): "*To determine the constructive (recursive) content or the constructive equivalent of the non-constructive concepts and theorems used in mathematics*, particularly arithmetic and analysis." This is meant to replace Hilbert's program (H. P.) calling for finitist or (more generally) constructive consistency proofs, which he says has two defects: "(1) Since the notion of constructive proof is vague, the whole formulation of the program is vague... (2) The formulation does not cover too well the actual substance of the material contained in [*Hilbert-Bernays 1939*]; e. g. the ε-theorems for the predicate calculus go far beyond establishing mere consistency...". Kreisel adds in (1) that though the problem of providing an exact formulation of what constitutes a finitist or constructive proof is an interesting one for the logician, H. P. is not attractive to the mathematician because of its vagueness. Regarding the use of 'constructive' in the statement of K. P., which might similarly be questioned, he says that this may be formulated in a precise way using his notion of *recursive interpretation* (*1958*, p. 160). That is an improved version of the notion of interpretation from *1951*, as follows: A recursive interpretation of a system Σ in a fragment Σ_0 consists of two recursive functions $A \mapsto \langle A_0^{(n)} \rangle$ and $p \mapsto \pi(p)$ such that

(RI$_1$) if A is a formula of Σ then the $A_0^{(n)}$ are formulas of Σ_0, and if p is a proof of

A in Σ then $\pi(p)$ is a proof of some $A_0^{(n)}$ in Σ_0,

(RI$_2$) A can be proved from each $A_0^{(n)}$ in Σ,

(RI$_3$) if $A \equiv \forall x \exists y R(x, y)$ then $A_0^{(n)}$ is $R(x, F_n(x))$ where the F_n are an enumeration of

the provably recursive functions of Σ.

Once more, H. T. for the predicate calculus and the n. c. i. for the predicate calculus and for arithmetic provide examples of recursive interpretations. These are explained in a general way, though some details about the n. c. i. for the predicate calculus with equality are presented in the Appendix to *1958*. The rest of that paper is devoted to a description of examples — treated in other publications — of applications of recursive interpretations to results in mathematics and logic. On the mathematical side, two are in algebra, namely to Hilbert's 17th problem and Hilbert's Nullstellensatz, and one is in arithmetic, namely the application to $\pi(x) - \mathrm{l}i(x)$ discussed in the preceding section; I shall not go into the logical applications here.

Both the algebraic applications make use of H. T. or the ε-theorems with equality. In the case of Hilbert's 17th problem, this is used to extract uniform primitive recursive bounds on the number and degrees in the representation as sums of squares of rational functions, of a positive semi-definite polynomial over a real field — from Artin's result guaranteeing such a representation. (As he reported at the Cornell logic conference in 1957, Kreisel had also obtained the same sort of bounds by a direct analysis, without use of logic, of Artin's original proof.) This interesting application is discussed in detail in Delzell's contribution to the present volume, and will not be dealt with further here. The application to the Nullstellensatz appears to be rather straightforward, granted elimination theory for algebraically closed fields. (However, see *Kreisel 1992* p. 33 ftn. 6 which refers to a correction of a "sloppy aside on proof-theoretic aspects of the Nullstellensatz" in his *1958* paper and also belittles his own work on that by comparison with more recent sharp results by algebraists.) For both theorems, a comparison is made in *1958*, p. 167, with closely related results obtained by model-theoretic methods.

So we turn again to the earlier application to Littlewood's theorem; here there are several supplements to the sketch in *1952* discussed in the preceding section. First of all (cf. ftn. 12 above) it is now stated explicitly that R. H. is equivalent to a statement in Π^0_1 form, by an argument in outline based on the methods of *Kreisel 1952a*.[15] Secondly, the statement of Littlewood's theorem is shown to be in Π^0_2 form by use of recursive approximations to $\mathrm{li}(n)$. Finally, the way in which the n. c. i. for arithmetic is to be applied is explained more clearly. Once more, it is stated that proofs in analytic number theory, and in particular that of Littlewood's theorem, can be formalized in arithmetic "...to yield the bounds, at least *in principle*" and that "if one examines proofs in analytic number theory *with a view* to a formalization in Z, one does not run into difficulties... If one is interested in finding bounds, one will naturally formalize the given informal proof in as constructive a manner as possible." (*Kreisel 1958*, p. 171). But one can't expect to do this in a mechanical manner; Kreisel says that he does not conceive of mathematical logic as "the mathematics for morons."

Nevertheless, once more, no precise conclusion is stated as to exactly what

[15] Another proof that R. H. is Π^0_1, using only some particular classical results about the Riemann zeta function, is given in full detail in *Davis, Matijasevič and Robinson 1976*.

can be drawn from application of the n. c. i. for arithmetic to Littlewood's theorem. This is all the more disappointing, as in the interim *Skewes 1955* had published what Kreisel (op. cit. p. 170) calls an *ad hoc* solution which gives "the same bound as the method of [*Kreisel 1952*]". Indeed, Skewes makes use of a weakening (H) of R. H.[16], under which he obtains an explicit bound x_1, for the first change of sign, as $x_1 = \exp\exp\exp(7.703)$ (or $e_3(7.703)$) and, under its negation (NH), an explicit bound x_2 as $10_4(3)$ and (slightly better) also as $e_4(7.705)$. Despite the close relationship, *Skewes 1955* does not refer to *Kreisel 1952*, and *Kreisel 1958* does not refer to Skewes' specific bounds.

It is only in two much later papers that Kreisel states explicit bounds on the first change of sign N of $\pi(x) - \mathrm{li}(x)$ resulting from his 1952 work. First, in *1968* (p. 362), he writes that this gives roughly $\exp\exp\exp\exp 8$ (or $e_4(8)$) as a bound for N, while by comparison, *Lehman 1966* gives $(1.65)10^{1165}$. He goes on to say that: "In view of these improvements, the present value of [*Kreisel 1952*] ... consists not in the bounds themselves, but only in analyzing the *general nature* of these problems; it separates what bounds are got from quite general considerations and what improvements need special study. This type of analysis is a typical logical contribution ...". Later, in his paper *1977*, Kreisel says: "[A]n essentially routine application of proof theory (ε-theorems or cut-elimination) applied to Littlewood's original proof, extracts a bound for N; cf. [*Kreisel 1952*]. Of course this cannot be expected to give optimal bounds, for which further ideas are needed, and it doesn't. It gives a bound of about

$$10^{10^{10^{34}}} \text{ compared to } (1.65)10^{1165}$$

found in [*Lehman 1966*]." (*Kreisel 1977*, p. 114) There's something fishy about all this, since the bound $10_3(34)$ shown here is the same as that obtained in *Skewes 1933* (under R. H.) by his alternative proof of Littlewood's theorem, and is different from the bound claimed in *Kreisel 1968*. Moreover, no mention is made in either paper of the still different bound in *Skewes 1955* which Kreisel had stated in the above quote from *1958* to be the same as that obtained in his 1952 paper.

[16]Similar in character to that of *Kreisel 1952*. As to the order of priority, it should be noted that, according to *Littlewood 1948* p. 169, Skewes had obtained a numerical bound "free of hypotheses" in 1937 and that the method used was to be found in Skewes' Ph. D. thesis deposited in the Cambridge University Library; apparently only the bound was improved in *Skewes 1955*. *N.B.* This footnote was omitted in *Littlewood 1953*.

7 *Applications to finiteness theorems.*

In his retrospective report *1987*, Kreisel says that by the mid 1950s he had come to assume — "wrongly" — that the potential for striking mathematical applications of proof theory was low. In the following twenty years, in his technical work Kreisel concentrated on a number of other aspects of proof theory and constructivity, with influential side forays into other parts of logic, such as predicativity and generalized recursion theory. But he says that he learnt in the late 1970s of many areas "where mathematicians wanted to unwind proofs of Π^0_2 theorems, but were not able to do so without logical guidance." (*1987*, p. 395). At the same time he began to formulate general results on unwinding Σ^0_2 theorems which led to applications to finiteness theorems in number theory; these are the matters to which the present section is devoted.

Recall from the description of H. T. in Sec. 3 above that if

(1) $\exists x \forall y R(x, y)$

is proved in the predicate calculus then there are terms $s_i(y_1, \ldots, y_{i-1})$, $i = 1, \ldots, k$ such that

(2) $R(s_1, y_1) \vee R(s_2(y_1), y_2) \vee \ \ldots \ \vee R(s_k(y_1, \ldots, y_{k-1}), y_k)$

is tautologous. Of special interest in number theory are statements asserting that a certain set $\{x | C(x)\}$ is finite, which we write in the form (1) as:

(3) $\exists x \forall y (C(y) \rightarrow y < x)$.

Then the corresponding Herbrand disjunction may be rewritten as

(4) $C(y_1) \wedge \ldots \wedge C(y_k) \rightarrow y_1 < s_1 \vee y_2 < s_1(y_1) \vee \ldots \vee y_k < s_k(y_1, \ldots, y_{k-1})$.

Kreisel observed that in such a case, if we have effective control of the s_i relative to their variables y_j $(j < i)$ then we can provide a bound on the *number* of y's such that $C(y)$ and — in some cases — on their *size*. For the latter, an obvious sufficient condition is that we have a number c such that $s_i(y_1, \ldots, y_{i-1}) \leq c$ for $i = 1, \ldots, k$, but we could rarely expect to meet this in practice. More interesting and potentially useful is when we have control over the *growth* of the s_i, leading to a bound on the number of y's such that $C(y)$, as illustrated in the following.

> **Theorem** (*Kreisel 1982*). *Suppose $C(y)$ is a number-theoretical predicate and that $s_i(y_1, \ldots, y_{i-1})$ $(i = 1, \ldots, k)$ are functions such that (4) holds for all natural numbers $y_1, \ldots, y_k$. If $c_i (i = 1, \ldots, k)$ are such that*
>
> *(i)* $s_1 \leq c_1$, *and*

(ii) $\quad s_i(y_1, \ldots, y_{i-1}) \leq c_i + \max(y_1, \ldots, y_{i-1}) \quad$ *for* $1 < i \leq k$,

then $\Sigma_{i=1}^{k} c_i$ *is a bound for the number of* y*'s such that* $C(y)$.

The proof of this theorem is quite easy, taking only eight lines in Kreisel's 1982 paper (p. 41). Another proof of the same length is given in *Luckhardt 1989* along with some other results using variant growth conditions.

It should be noted that it is not required in Kreisel's theorem (or its variants) that the statement of finiteness be proved in the predicate calculus or any other given formal system, nor that the s_i are terms in a formal language, nor that C is decidable. Thus the only role of Herbrand's theorem here has been to suggest appropriate forms to consider for applications. This is exactly what one finds in the work of *Luckhardt 1989* on "Herbrand analyses" of two proofs of the famous theorem of *Roth 1955* (also known as the Thue-Siegel-Roth theorem) about diophantine approximations.[17] Roth's theorem states that if α is an irrational algebraic number then for any $\varepsilon > 0$, there are at most a finite number of $q > 1$ such that for some (unique) integer p with $(p, q) = 1$,

(5) $\quad \left|\alpha - \frac{p}{q}\right| < \frac{1}{q^{2+\epsilon}}.$

The theorem guarantees that suitably related diophantine equations have only a finite number of solutions. For each specific α and $\epsilon = 1/n$ (say), Roth's theorem takes the logical form

(6) $\quad \exists m \forall q (C(q) \rightarrow q < m)$

where

(7) $\quad C(q) \equiv q > 1 \wedge (\exists! p\epsilon)[(p, q) = 1 \wedge |\alpha - pq^{-1}| < q^{-2-\epsilon}]$,

C can be shown to be decidable and hence (6) is in Σ_2^0 form, though as mentioned above, that information is not needed for the applications. At any rate (6) is a *prima facie* candidate for an Herbrand analysis, i. e. for the identification of suitable $s_i(q_1, \ldots, q_{i-1})$ $(1 \leq i \leq k)$ satisfying

(8) $\quad C(q_1) \wedge \ldots \wedge C(q_k) \rightarrow q_1 < s_1 \vee q_2 < s_2(q_1) \vee \ldots \vee q_k < s_k(q_1, \ldots, q_{k-1})$

for all $q_1, \ldots, q_k$. What Luckhardt does in his 1989 paper is to extract such s_i from two proofs of Roth's theorem, namely Roth's own in 1955 and that of *Esnault and Viehweg 1984*. In both cases, by careful examination of the growth conditions, he is able to improve the bounds previously obtained on the number of q's with $C(q)$. In particular, Luckhardt obtains a bound from

[17] The idea for this specific direction of application was already advanced by Kreisel in 1970; cf. *Kreisel 1977* pp. 114–115.

the second proof which is polynomial of low degree in n and $\log d$, where $n = 1/\epsilon$ and $d =$ degree α. It turns out that the same bounds were obtained independently by Bombieri and van der Poorten (*1988*) without any (explicit) Herbrand analysis.

8 *Assessment of the mathematical applications.*

Besides the applications of proof theory discussed or indicated above, Kreisel mentions several areas, "...L-functions, Galois cohomology, ergodic theory, topological dynamics" that he says he learned about in the late seventies where (as quoted above) "mathematicians wanted to unwind proofs of Π^0_2-theorems, but were not able to do so without logical guidance". No references are given for these and I have not been able to chase them all down. There is a brief discussion in *Kreisel 1990*, pp. 247–248 of the question of obtaining a lower bound for $L(1)$, where L is the Dirichlet L-function. One has a standard non-constructive proof that $L(1) > 0$; Kreisel refers loc. cit. to a *modified proof* which can be unwound by hand "since it has been done." (For this, some indications are to be found in *Kreisel 1981*, p. 139 ftn. 2, and *1981a*, pp. 150–152.)

The mentioned application to topological dynamics is presumably that made by *Girard 1987* to extract bounds from two forms of the *Furstenberg and Weiss 1978* proof (by those methods) of the famous combinatorial theorem of van der Waerden (*1927*) on arithmetic progressions in partitions of the natural numbers. That theorem asserts the existence for any p and k of an n such that if $\{0, \dots, n-1\}$ is partitioned into k classes $C_1, \dots, C_k$, at least one C_i contains an arithmetic progression of length p. Girard first applies cut-elimination to an "ad hoc" modified form of the Furstenberg-Weiss proof (op. cit. pp. 237–251). He later applies the n. c. i. to the Furstenberg-Weiss proof closer to that originally given (op. cit. pp. 483–496). The unwindings are shown to lead to bounds for $W(p, k)$, the least n as a function of p and k, in a sub-recursive hierarchy. In the case of the first, modified proof, this is at the level of the Ackermann non-primitive recursive function; that is the same bound as obtained by inspection from the original van der Waerden proof. In the case of the second proof, the bound for W extracted is at a somewhat higher level.

Discussion. What can we say now about these various claimed applications of proof theory to the unwinding of *prima-facie* non-constructive mathematical proofs? Here I would raise several questions:

1. Have there been enough applications so that one can speak of a definite direction of work, with a clear past and promising future?
2. In what sense are these really applications of proof theory?
3. Do the applications provide the kind of specific information sought by mathematicians?

As to 1, I think it is fair to say the number of applications is still disappointingly small. Moreover, the nature of the past applications is mixed (to the extent that one understands their "nature" at all), and the prospects for the future are far from clear. This is not an existing or emerging direction of work that one would propose for a thesis topic without a great deal of hesitation.

Concerning 2, here again the history is mixed. As we have seen, the unwinding of Littlewood's theorem, such as it is, is not really an application of the n. c. i., nor is the use of "Herbrand analysis" in the proofs of the Roth theorem an application of Herbrand's theorem. On the other hand, Kreisel's first treatment of Artin's solution of Hilbert's 17th problem did involve a genuine application of Herbrand's theorem or the ε-theorems[18], and Girard's treatments of the Furstenberg-Weiss proof are genuine applications of cut-elimination and the n. c. i. Of course, it is recognized that one does not have, in each of these cases, a matter of blindly formalizing existing proofs in some formal system and mechanically applying the transformations provided in principle by the relevant proof theory. Rather, these applications are "genuine" on their face because they apparently involve steps that correspond to those transformations in a significant way. On the other hand, there is no integral involvement of proof theory in the purported applications to the Littlewood and Roth theorems. The discussion in *Luckhardt 1989*, pp. 206–261, of the latter is pertinent. Luckhardt there makes the following points (among others): (i) Herbrand's theorem (H. T.) is *not* necessary in principle ("nicht prinzipiell nötig")[19] in his applications; (ii) knowledge of H.

[18]Cf. the discussion in *Delzell* (Part I, this volume).

[19]In fact, as I have stressed in Sec. 7, the use of H. T. was merely to suggest the *form* of the statements to be considered; the theorem itself is not used at all.

T. provides the concepts needed to interpret existing mathematical proofs as falling under a logical pattern; and (iii) applications to specific cases are not simply obtained by substitution into a gross ("plump") scheme.[20] With 'n. c. i.' or 'cut-elimination' or 'normalization' equally well substituted here for 'H. T.', I think (i)–(iii) pretty much speak for themselves in response to question 2 for most of the purported applications.

Finally, as to the third question, in the cases that one can make comparisons at all, the answer is: Yes ... but. There is no need to repeat the ambiguous outcome on the Littlewood theorem described at the conclusion of Sec. 6. And, as mentioned at the end of the preceding section, Luckhardt's improvement in the bound from the Esnault-Viehweg proof was obtained independently by Bombieri and van der Poorten without any appeal to logic. Finally, in the case of the van der Waerden theorem, the main question that had interested combinatorists was whether the function W could be given a primitive recursive bound, i. e. essentially lower than the original Ackermann function bound. Here the striking result is that obtained by *Shelah 1988* by a novel elementary combinatorial proof, with no use of logic, that gives a bound for W at level 4 in the "fast-growing" hierarchy.[21] For a typical appreciation by a combinatorist of that improvement, see the review by Joel Spencer of Shelah's paper in the *J. Symbolic Logic* 55 (1990), pp. 887–888.

As a postscript to the discussion of questions 1–3 above, one should mention the recent work of *Kohlenbach 1993, 1993a*, which concerns applications of a variant of Gödel's functional interpretation to results in Chebycheff approximation theory. The results in question fall under the general form of uniqueness theorems:

(1) $\forall u \epsilon U \forall v_1, v_2 \epsilon V_u (G(u, v_1) = \inf\{G(u,v) : v \epsilon V_u\} = G(u, v_2) \rightarrow v_1 = v_2)$

where U, V are complete separable metric spaces, V_u is compact in V and $G : U \times V \rightarrow$ is continuous. Kohlenbach's applications extract from classical proofs of results of the form (1) effective "moduli of uniqueness" Φ, satisfying:

(2) $$\forall u \epsilon U \forall v_1, v_2 \epsilon V_u \forall n \epsilon \Big[\prod_{i=1}^{2} \Big(G(u, v_i) - \inf\{G(u,v) : v \epsilon V_u\} \leq 2^{-\Phi u n} \Big) \rightarrow d_V(v_1, v_2) \leq 2^{-n} \Big],$$

Note that this is an *a priori* estimate since Φ does not depend on v_1, v_2. There is no question that such unwindings are a genuine application of logic

[20] See also *Kreisel 1990*, pp. 250–253, for further discussion along these lines.

[21] That is, at level f_4 where $f_1(x) = 2x$, $f_{i+1}(x) = f_i^{(x)}(1)$.

and, on the face of it, provide the kind of specific information of interest to mathematicians. Indeed, Kohlenbach's work apparently yields improvement of known estimates in connection with Chebycheff approximation. It is obviously premature to say *how* interesting this will be to the mathematicians in that field, and whether this direction of work augurs well for the future of applications of unwinding more generally.

9 *K. P. versus H. P.*

Kreisel's retrospective report (*1987*) begins: "Like many others but particularly Gödel [*1931*] and Gentzen [*1936*] (on p. 564) who expressed their reservations discretely [sic] I was repelled by Hilbert's exaggerated claim for consistency as a sufficient condition for mathematical validity or some kind of existence." Kreisel's assimilation of Gödel and Gentzen to his view in this respect is misleading, to say the least. Certainly Gentzen was totally committed to the Hilbert program, though he was more cautious than Hilbert about what we may claim to be achieved by a finitist consistency proof. And there is considerable evidence that Gödel took certain relativized forms of the Hilbert program (with consistency as its main aim) seriously all through his career. This is to be found both in his published work and in unpublished lectures whose texts have been found in Gödel's *Nachlass* and which appear in the third volume of his *Collected Works*.[22,23]

Be that as it may, Kreisel goes on to say, in the passage in question, "[B]ut unlike most others I was not only attracted by the logical wit of consistency proofs (which I learnt in 1942 from Hilbert-Bernays Vol. 2) but also by the so to speak philosophical question of making explicit the additional knowledge provided by those proofs (over and above consistency itself)." He says that his answers took two forms: "(i) particular applications to mathematical proofs...[and] (ii) general formal criteria such as functional interpretations to replace the incomparable condition of consistency; 'incomparable' because the aim of functional interpretations is meaningful without restriction on metamathematical methods." In other words, the ("philosophical") aim was to substitute clear-cut mathematical results for inconclusive philosophical ones; the appeal of these moves (i) and (ii) is evident. But despite

[22]Forthcoming, at the time of this writing; cf. also *Feferman 1993*.

[23]*Kreisel 1958a* and *1968* also formulated a relativized reductive form of H. P.; see *Feferman 1988* for my own version which took that as its point of departure.

the hoopla, the results in direction (i) — at least to date — have been largely disappointing. What about direction (ii)? Here I count the move a success, with such prime exemplars as the n. c. i. in *Kreisel 1951* and Gödel's functional interpretation in his paper *1958* together with its many interesting extensions to systems of analysis and down to fragments of arithmetic (cf. *Troelstra 1990* and *Feferman 1993*), as well finally as the many results, using these methods and cut-elimination, classifying the provably recursive functions of various systems of arithmetic, analysis and set theory.[24] While these results may only (or primarily) be of interest to logicians, they are certainly presented in mathematically understandable terms independent of any normative — and hence (possibly) disputable — foundational doctrines. This direction of work is surely a success story and a going industry.[25] Apparently Kreisel has lost interest in it (perhaps for that very reason); for example, he is contemptuous of the "flashy precision" of ordinal analyses, which are one of the main technical tools in the characterization of the provably recursive functions of various theories. One can't fault him for losing interest in a subject that he helped launch years back; but, with or without his approval, this direction of work as a whole is sure to march on.

Finally, what about the foundational motivations for proof theory which (i) and (ii) were intended to replace? Here, I find Kreisel truly ambivalent. Over the course of his career he has certainly taken the philosophy of mathematics seriously and has devoted considerable thought and writing to it. And, while he railed constantly against the perpetuation of simple-minded traditional doctrines, substantial portions of his own work were motivated by more sophisticated foundational — as opposed to mathematical — concerns.[26] Indeed, in the period from the 1950's on when logic as technology was overtaking logic as a foundational tool, Kreisel led the way in promoting foundational concerns as the driving force in the pursuit of proof theory and constructivity, and those of us who followed him did so for exactly that reason. In more recent years, though, Kreisel has become increasingly dismissive of any attempt at systematic philosophical efforts, at least as practiced by everybody else, and perhaps even himself. I'm afraid that here, also, we will continue to be inspired by those original foundational aims while subjecting

[24]Cf. e. g. *Takeuti 1987* and its appendices by Pohlers and myself for a survey of various such results.

[25]The impressive results described in *Rathjen 1994* give further evidence for this assessment.

[26]Kreisel's influential *Survey of Proof Theory* (*1968*) provides a prime example.

their development to his perennial insistence on critical examination — and re-examination, and re-examination. It is in that spirit that this piece is dedicated to Kreisel.

Acknowledgements. I wish to thank C. Delzell, A. B. Feferman, G. Mints, P. Odifreddi, and W. Sieg for their useful comments on a draft of this article.

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