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The Collected Papers of Gerhard Gentzen. by M. E. Szabo

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## BOOK REVIEWS

*The Collected Papers of Gerhard Gentzen.* M. E. SZABO, editor. Amsterdam: North-Holland, 1969. 350 p. \$20.20.

This volume contains not altogether felicitous translations of material submitted for publication by Gentzen. There are ten articles, two of which were withdrawn before publication (these are reviewed, with some technical detail, in Appendixes I and II below). It is fair to say that Gentzen's work is the source of the bulk of existing proof theory, roughly speaking that part which deals with *syntactic transformations* to various "normal forms" (to be contrasted with *functional interpretations*, which will be discussed in Appendix III). Consequently it is not surprising that the precise details of Gentzen's expositions are largely superseded by later work; not only the technical details of the arguments but also—in fact principally—the exact formulation of the (metamathematical) results. There are three broad areas of change.

Formally speaking, where at Gentzen's time the existence of *some* solution was of interest, today we recognize that solutions satisfying *stronger* additional conditions are needed (for an adequate answer to the original problem). Examples occur throughout this review.

The discovery of these "stronger" conditions will generally require a new point of view, analysis of our philosophical position; and, correspondingly, their proper formulation will require the introduction of new (metamathematical) notions. This applies to most, though not all results that Gentzen originally formulated as *consistency results*, in connection with Hilbert's program; cf. Appendix II.

The passages in the original expositions that are of principal interest to the modern reader (who is not primarily an historical scholar) are Gentzen's "undeveloped" informal ideas in his introductions or marginal comments and his reservations. This much is typical of the writings of pioneers. But I think that, in Gentzen's case, these ideas are also of principal interest *sub specie aeternitatis*, specifically his ideas on a *theory* of proofs, where proofs are principal objects of analysis, and not a mere tool; in contrast to Hilbert's *Beweistheorie* (the "proof theory" of current texts in mathematical logic).<sup>1</sup>

<sup>1</sup>To avoid misunderstanding I should perhaps say that I was slow to take in this part of Gentzen's work; as a young student I looked for less subtle formulations, in terms of functional interpretations of sets of theorems, suppressing explicit attention to derivations altogether. At least for some of us there is great

## HILBERT'S PROGRAM: PRELIMINARIES

Certainly until about ten years ago and probably even now, Gentzen has been best known among the general (logical) public for his consistency proof for formal arithmetic and thus his contribution to Hilbert's program. Indeed, Gentzen himself seems to have been completely preoccupied with this program in his later writings. It would be very wrong to underestimate the superficial attractions of this program or to attribute them, in a vulgar spirit, to Hilbert's "prestige"; all the principal critics—Brouwer, Poincaré, Russell—were well known too. More likely, the general attraction was connected with positivistic views which welcomed Hilbert's aim of eliminating all philosophical problems, or at least separating them from scientific practice. But the fact remains that, at least for a thoughtful person, there are genuine doubts concerning the *significance of Hilbert's program*, that is, of consistency proofs. And, psychologically speaking, these doubts are obstacles to understanding not only Gentzen's later consistency proofs, but also his theory of first-order predicate logic (whose consistency is not problematic at all). Since the matter is quite central—and quite inadequately treated in the translator's introduction—let me make some distinctions here (I shall return to the subject at the end of this review after discussing Gentzen's own work on it).

Of course we know principles whose consistency is simply dubious and therefore a consistency proof, by any correct means, is *needed* in the perfectly ordinary sense of the word. Also we have cases where the concepts that originally suggested the formal rules considered, are *obviously* less elementary than the methods used in a consistency proof, even if the sense of 'elementary' is not always easy to analyze convincingly. For example, Gentzen's own consistency proof (110–114) for a *subsystem* of usual arithmetic provides a significant *reduction* compared to the usual concept of model (a concept which justifies equally the full system of arithmetic). This kind of situation is common at a certain stage of research, for example in mathematics; without in the least doubting the legitimacy or, as one sometimes says, reliability of our concept of real number say, a mathematician may look for an "algebraic" proof of a theorem in analysis, recognize his achievement, without being able to analyze explicitly

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satisfaction in the idea of some development of our opinions (which presupposes some defects in our former views!). So, occasionally, I may be overcritical of my earlier, more orthodox aims. In this connection, the reader may compare T. S. Eliot's analysis of his critical essays as "an immature youngish man," thirty years after they were written; on page viii of the Preface to his *Essays on Elizabethan Drama* (New York: Harcourt Brace Jovanovich, 1960).

what is essential about it. Such an analysis is a subject for *further* research.

Hilbert, the founder of the consistency program for the usual mathematical principles, always stressed their overwhelming reliability, and, in particular, the fact that they have nothing to do with the “reasoning” that leads to the paradoxes.<sup>2</sup> As a corollary, we have the obvious conclusion which, admittedly, Hilbert failed to stress:

If ordinary mathematical reasoning is really so reliable then the value of Hilbert’s consistency program cannot possibly consist in increasing significantly the *degree of reliability* (of ordinary mathematics).

It is therefore clear that we must be prepared for the possibility that *the analysis of the significance of a consistency proof may be more difficult than the proof itself*.<sup>3</sup> The problem at issue is not merely to find *some* significance; one can be sure that work that somebody of Gentzen’s caliber finds interesting, constitutes, in the ordinary sense of the words, a contribution to our knowledge. The question is whether the contribution has the specific—sometimes called “epistemologically interesting”—character that one expects from a “consistency proof” of formal arithmetic. It may fairly be said that, for the logician, an explicit analysis of this specific character is a principal *open* problem. And it is this problem which dramatic horror stories of lurking paradoxes *hide*. (Thus these stories are theoretically indefensible and practically disastrous since, as history shows, they have attracted a lot of logical cripples to the subject, apart from a few exceptionally thoughtful people.) I attach some weight to the remark above concerning Gentzen’s *general* judgment. To show that it is consistent with doubts about his particular judgment (on the epistemological value of the consistency proof) I shall mention below some striking *mathematical* applications of his work which more than justify the “raw” impression that something of interest has been achieved.

<sup>2</sup> E.g., p. 158 of *Gesammelte Abhandlungen*, vol. III (Berlin: Springer, 1935). As a corollary, the metamathematical methods to be used in the consistency proofs are not primarily distinguished by “having nothing to do with the paradoxes” (in contrast to the quite unconvincing passage on page 10, 1.13–1.14 of the translator’s introduction).

<sup>3</sup> I believe that this applies to the particular case of Gentzen’s consistency proofs for full arithmetic, considered at the end of this review and in Appendix II; particularly if we remember a modern proof using (quantifier-free)  $\epsilon_0$ -induction and  $\epsilon_0$ -recursion to describe the (infinite) proof figures involved and to establish their relevant properties. This generalizes, almost word for word, Gentzen’s older work on predicate logic.

Let me note in passing that Gentzen was quite well aware of the fact that an *epistemologically* interesting reduction can be achieved, not only by “proof-theoretic” methods but also by spotting a particularly simple model of an axiom system. This is in contrast to people, preoccupied with proof theory, who even today often fail to see this significance of various elementary models of *subsystems of set theory*.<sup>4</sup> By example if not by design Gentzen helps us to see and so correct this methodological error, in the paper on the simple theory of types (214–222) which is, otherwise, little more than a routine exercise. I suppose it is fair to say that when one speaks of the simple theory of types one thinks of some infinite domain of individuals (because it is in that connection that the principal applications are made). Gentzen’s consistency proof exploits the fact that the “official” axioms for the simple theory of types simply do not require such an infinite domain. However, as always, he has interesting remarks; on page 214, he emphasizes the simple point that Russell’s paradox does not involve the axiom of infinity but rather the absence of a type structure: so much for the cliché that the infinite is essentially or even obviously involved in the paradoxes. Incidentally, this cliché<sup>5</sup> is not always avoided by Gentzen himself, for example, in his two informal essays on the concept of infinity (223–233) and the then-current state of foundations (234–251). These present mainly Hilbert’s and Brouwer’s views, but without Hilbert’s heavy rhetoric or Brouwer’s impatience. (Almost unique among gifted logicians, Gentzen, from the word go, always wrote in a thoughtful and hence placid and relaxed style.) Also he expounds the implications of technical results such as the Skolem-Löwenheim theorem (241–242) carefully; but the exposition is a bit longwinded, and certainly not comparable to a standard modern account. In short, the two systematic essays are superseded for the reason mentioned earlier in connection with formal work: even though this may not be generally realized, quite a lot has happened, since Gentzen’s days, in foundations, not

<sup>4</sup> Because of my current interests I am perhaps reading too much into remarks on pp. 136 and 200 (which occur in another context). But Gentzen can be read to say that mathematical practice does not use anything like all the inferences in the “corresponding” formalization of the branch of mathematics considered and that a formalization of abstract branches of mathematics should limit the use of the general concept, not code the objects by integers; as we should say now, we should use weak existential axioms formulated in the language of set theory or type theory.

<sup>5</sup> The translator commits the much more blatant blunder (in the quite injudicious selection of quotations for his introduction) of alleging that intuitionism is preoccupied with finiteness (19); this is plainly contradicted on p. 10 in the reference to the abstract character of the intuitionistic notion of implication, a point which Gentzen himself stressed repeatedly and forcefully; cf. Appendix I.

only in technical logic. When all is said and done, the value of *systematic* expositions is limited principally by the objective state of a subject; and the author's talent, provided only he is competent, will mainly affect the literary form.

THE HEART OF THE MATTER: GENTZEN'S IDEAS OF  
A THEORY OF PROOFS

As I see it now, thirty-five years later,<sup>6</sup> the single most striking element of Gentzen's work occurs already in his doctoral dissertation (68–69): he looks for and is able to see significant differences in different formalizations (of predicate logic). Specifically, he had some conception of (logical) proof which allowed him to distinguish significantly between different formal systems with the same set of theorems, and between different derivations in a given system of the same end formula. (The best known of these distinctions concerns so-called "normal" or "cut-free" systems, variants in terminology reflecting different views on what is essential about these systems.) To put it simply, the novelty compared with Frege's analysis is this: *Proofs, expressed by formal derivations, are principal objects of study; not mere tools for analyzing the consequence relation or the validity predicate* (defined model-theoretically in the case of classical logic). In other words, the study concerns the *process* of reasoning, not only its results, that is, the theorems proved.

Gentzen's original aim was not to solve some flashy "clearly defined" problem, but, as he puts it, to stay close to actual reasoning. Obviously he had no intention to be faithful to the vagaries of actual language where one often uses different words for the same idea to make a phrase phonetically memorable (for example we use 'classes of sets' instead of 'sets of sets'). Certainly, if others had set themselves the same "problem" they might have floundered hopelessly. But as Grillparzer said: Wenn zwei das Gleiche tun, so ist es doch nicht Dasselbe (which means, freely translated: It's not what you do, it's the way you do it). And Gentzen succeeded; at least he provided the *germs for a theory of proofs*.

Before going into details it is necessary to say a word about the possibility or, perhaps better, the plausibility of such a theory of proofs. Whatever the merits of the case, it is a fact that we are not accustomed to thinking of proofs or of other *intensional objects* as

<sup>6</sup> Guided by D. Prawitz's reading of Gentzen, in the monograph: *Natural Deduction: A Proof Theoretical Study* (Stockholm: Almqvist & Wiksell, 1965) and, more explicitly, "Ideas and Results in Proof Theory," in *Proceedings of the 2nd Nordic Logic Symposium*, J. E. Fenstad, ed. (Amsterdam: North-Holland, 1971). Prawitz himself has traced the ideas to Gentzen's work, where they are not very explicit; see, however, 5.31 on pp. 80–81.

material for theoretical study. So we certainly do not have the kind of intellectual experience that would be needed to inspire confidence in such a study. (Under these conditions there will *always* be people who overstate the case by manufacturing reasons why there is no sense in the study, presumably on Dr. Jowett's principle: It isn't knowledge if I don't know it.) So let me make some quite general *elementary distinctions* which, incidentally, will also be useful for other topics treated later on in this review.

First of all, without a shadow of doubt the distinction between different formal systems with the same set of theorems, in terms of the proofs *expressed* by their derivations, is meaningful.<sup>7</sup> As a matter of historical fact: what else were formal rules intended to do? or how else were they found?

Secondly, quite convincing *isolated* uses of the distinction are familiar from the philosophical literature. For example, in Wittgenstein's mixed bag of remarks on the foundations of mathematics<sup>8</sup> the distinction is used to isolate what is "lost" in the reduction of arithmetic to logic.

The real issue is whether the distinction lends itself to a *systematic* theoretical study and, perhaps most importantly, *whether we want a theory*. After all, not all of our experience lends itself to theoretical study; for example the "accidental" facts around us do not (and note that their objectivity is not in doubt merely because we have no theory!). And also where a theory *is* possible, it does not necessarily help us understand the world around us; for example recondite theories of the optical properties of matter (in terms of their chemical composition) do not inasmuch as shades of color though most striking to the eye are not, as one says, "physically significant." And so, I think, even today we should regard the project of a theory of proofs as a calculated risk.

As a last general remark: let us not forget that Gentzen was a pioneer! He has drawn our attention to the possibility of a theory, and he has provided some concrete proposals, and results. It would be quite romantic to expect that he was right on all details. As

<sup>7</sup> Following current terminology I use 'express' for the relation between a formal expression  $E$  and the intensional object meant by  $E$ , and 'denote' for the case when we suppress the intensional features of the object, for example in model theory. But, heuristically, it is probably more important to remember the *similarities* between the two relations: (i) here, a derivation  $d$  expresses the proof  $\mathbf{d}$  (for a given interpretation of the symbolism); (ii) a formula  $F$  denotes its realization  $\mathbf{F}$  (for a given realization of its language) in model theory.

<sup>8</sup> *Bemerkungen über die Grundlagen der Mathematik* (Oxford: Basil Blackwell, 1956) and section 8 of my review, *British Journal for the Philosophy of Science*, ix, 34 (August 1958): 135–158.

Prawitz has shown in his monograph (*op. cit.*), I think convincingly, Gentzen's belief (69) that systems of natural deduction were not suited for an elegant statement of his main results was unfounded. Also he may well have been wrong in supposing, implicitly, that a formulation is better if it applied to both intuitionistic and classical systems; this overlooks the point that, at least *prima facie*, the intuitionistic systems may lend themselves to a more delicate proof-theoretic study because the logical operations are intended to be interpreted in terms of operations (on proofs).<sup>9</sup>

After these generalities, we can now return to the specific matter of a theory of proofs. How then are we to set about discovering whether we "want" such a theory, and how are we to "calculate the risks" involved? We cannot expect to rely here on *ordinary* (mathematical) experience; no more than a physicist can expect to build up a theory on the basis of ordinary familiar experience; he has to extend the latter by "artificial" experiments and special observations, including experiments that were done in the past in connection with some false or inadequate theory. Evidently *vast experience in current proof theory is ready-made material which corresponds to the physicist's experiments and observations*. Here we have a body of discoveries, of formal facts, by which a theory of proofs can be tested.

The idea that we "want" a theory of proofs with new concepts is also consistent with the fact that the achievements of existing proof theory are not well known. Anyone familiar with this subject *knows* its interest; but in terms of current concepts, we were not able to *say what we know intelligibly and memorably* (except by means of literary devices). So new notions, to be provided by a theory of proofs, are needed to convey this interest. Certainly, during the last twenty years, attempts have been made to formulate individual results intelligibly, either in terms of epistemological notions such as *constructivity* or by reference to mathematical *applications*, for example in algebra. But, speaking for myself, I believe one would have treated these reformulations as neat but marginal corollaries if Gentzen's own ideas on a theory of proofs and, even more, Prawitz's development of them had been understood at the time.<sup>10</sup>

To get the *flavor of the potentialities* of a theory of proofs, let me restate one of the best known results of proof theory, Gentzen's

<sup>9</sup> For detailed results in support of this remark (for example, in connection with the role of the so-called "subformula property"), see Appendix I or §2a (i) *in fine* of: "A Survey of Proof Theory II," *Proceedings of the 2nd Nordic Logic Symposium, op. cit.*

<sup>10</sup> This applies e.g., to my "Mathematical Significance of Consistency Proofs," *Journal of Symbolic Logic*, xxii, 2 (June 1958): 155–182; see also "A Survey of Proof Theory," *ibid.*, xxxiii, 3 (September 1968): 321–388, SPT for short.

*normalization*, also called: *cut-elimination theorem for predicate logic*. (The name *Hauptsatz*, that is, principal theorem, is used by people who want to stress that they cannot say in what the interest of this theorem consists.) The *formal facts* involved are stated most strikingly for Prawitz's formulations of systems of natural deduction:

Given any formal derivation  $d$ , we apply "normalization" steps consisting in the *contraction* of the introduction of a logical symbol followed by its immediate elimination. Then it is shown that each sequence of such steps *terminates* in a "normal" form (to which no normalization step can be applied); further, the normal form is independent, up to congruence, of the order when several contractions are possible. This normal form is denoted by  $|d|$ .

When *proofs* are treated as independent objects of study the obvious succinct formulation is this:

To every derivation  $d$  there is a normal derivation  $|d|$  that *expresses the same proof* as  $d$ . (Thus "normal" derivations provide *canonical* representations, roughly<sup>11</sup> as the numerals provide canonical representations for the natural numbers.) And natural-deduction systems are distinguished at least to the following extent: the particular normalization steps for which we get normal forms that are independent of order, *evidently* preserve the proof expressed by the derivation (to which the step is applied).

Let us now compare this with the usual statement, which might be called a "normal-form" theorem (in contrast to "normalization" theorem, since it does not refer to particular normalization steps at all):

- (\*) Every formal theorem of predicate calculus also has a normal (cut-free) derivation.

Quite naively, it is disturbing to establish the (metamathematical) result (\*) by proving the termination of some normalization *procedure*. Since there is no reference to such a procedure in (\*), what have its details to do with (\*)? *Either* we should give an independent significance or "meaning" to these details—that is, we should state a *different* theorem—or we should give a proof of (\*) that does not introduce these extraneous details. Amusingly, such a proof was essentially in the literature even before Gentzen appeared on the scene! It involves only notions of unquestionable interest, namely, the model-theoretic notions of *soundness* and "semantic" *completeness* (that is, completeness for validity).

<sup>11</sup> In the case of derivations it is not so evident that noncongruent normal derivations express different proofs; this doubt does not affect the restatement of the normalization theorem.

All derivations (of the systems considered) are sound; that is, the formal theorems are valid. Also, inspection of Gödel's own completeness proof shows that he actually establishes completeness of normal (or cut-free) derivations. So (\*) follows.

It was quite obvious that Gentzen's work established "more." Confronted with this brute fact, people thrashed about for something to say. As so often, they brought in some doctrinaire considerations, in particular objections to the "nonconstructive" character of the model-theoretic proof of (\*). Of course we weren't all hoodwinked by this maneuver. But to *show* its error I need some technical results.

*Refinement of the model-theoretic proof of (\*)*. The philosophically interested reader who has some technical background will, I think, find the discussion below (and a closely related one in Appendix I) rewarding. The basic point is quite simply this.

Granted that (many) traditional philosophical notions such as the epistemological notion of constructive proof, have interest; just which part of our intellectual experience do they help us understand? Accepting them as relevant where they are not, is almost as much of an obstacle to progress in philosophy as rejecting them (and thus not using them at all).

Before it was realized that Gentzen's argument established not only the normal-form theorem (\*) but the stronger normalization theorem, it was sometimes said that his argument was needed for a *constructive* proof of (\*) itself. In other words, the quite obvious interest of Gentzen's argument was to be analyzed in terms of constructivity. This is not adequate, because a constructive proof of (\*) can be obtained by a *routine refinement* of the model-theoretic proof as follows:

First we modify Gödel's argument<sup>12</sup> slightly to show this: for any formula  $A$  there is a structure  $M_A$  with an *arithmetically definable truth definition* such that  $M_A$  is a model of  $\sim A$  if  $A$  isn't formally derivable without cut. (The formalization in arithmetic of this type of argument is familiar.<sup>13</sup>)

To finish the proof of (\*) we need soundness of the "full" system of rules only for the specific structure  $M_A$ . Since its truth definition is arithmetic, in so-called " $\Delta_2^0$ -form" (actually uniformly for all  $A$ ), the proof of soundness for  $M_A$  can be formalized in first-order

<sup>12</sup> Or, even more simply, Henkin's definition of *complete* and *consistent extensions* in: "The Completeness of the First-order Functional Calculus," *Journal of Symbolic Logic*, xiv, 3 (September 1949): 159–166.

<sup>13</sup> G. Hasenjaeger, "Eine Bemerkung zu Henkin's Beweis für die Vollständigkeit des Prädikatenkalküls," *Journal of Symbolic Logic*, xviii, 1 (March 1953): 42–48.

arithmetic with induction applied to a (logically not at all complicated)  $\Delta_2^0$ -formula.

Finally, note that (\*) is expressed by an  $\forall\exists$  formula:

To every  $d$  there is a  $|d|$  ( $|d|$  is normal, and  $d$  and  $|d|$  have the same end formula).

But since this formula can be derived by means of  $\Delta_2^0$ -induction there is a constructive proof with a constructive mapping:  $d \rightarrow |d|$ ; as an immediate application of the functional interpretations in Appendix II (the easiest "consistency proofs"). More to the point, these interpretations suggest an ad hoc analysis of the model-theoretic proof which yields the desired constructive proof of (\*). I have convinced myself that this route needs less total labor than Gentzen's proof. But, to repeat: I do not think that total labor is the principal issue! The model-theoretic proof of (\*) breaks up into memorable parts of independent interest; *unless* significance is given to the particular steps in the normalization procedure, they are, literally, unsatisfactory; "literally" because we shall not be satisfied until we know "what they have to do with" (\*).

Returning now to our main topic of a theory of proofs, I think there can be no doubt that the *details* of Gentzen's work on predicate logic and its development are satisfactory only if derivations are thought of as expressing proofs and if proofs are treated as principal objects of study. Specifically, if one confines oneself to results that refer only to the *consequence relation*, that is, the relation between sets of formulas (the "hypotheses") and formulas (their "conclusions"), it is best to suppress derivations altogether.<sup>14</sup> At the present time there *is* doubt about the extent to which a theory of proofs can be developed, since many natural and indeed quite basic questions are open; for example,

In what sense is Gentzen's analysis of logical proofs *complete*?

Can every logical proof be expressed by a (normal) derivation?

Obviously some restriction is needed, since a proof that refers explicitly to the soundness of the rules (which are defined in "arithmetical," not "logical" terms) will not even be expressible in the logical language, let alone representable by a deduction built up

<sup>14</sup> This view is, I think convincingly established by R. M. Smullyan's operation on sets (of formulas) which have the "consistency property"; cf. *First-order Logic* (New York: Springer, 1968). (This work *eliminates* a good deal of the details in Gentzen; it does *not* give them meaning.) The superficial impression that details about derivations are needed for *infinitary* predicate logic is corrected by M. Makkai, *Journal of Symbolic Logic*, xxxiv, 3 (September 1969): 437-459.

according to the rules considered.<sup>15</sup> Or, to take an example from everyday (mathematical) reasoning: first-order assertions, say  $A$ , about ordered fields are *expressed* by purely logical formulas of the form  $OF \rightarrow A$ , where  $OF$  is the (first-order) formula expressing the property of being an ordered field. But a (modern!) proof of  $OF \rightarrow A$  often refers to the construction of the real closure (of any ordered field) and to set-theoretic operations on it which are certainly not expressed in the logical language. Of course this still leaves the possibility of a theory of those proofs that can be expressed in the logical language: but the value of such a theory for the analysis of (mathematical) reasoning may be limited.

Let me remark that something *like* a study of derivations is of course needed for so-called *mechanical proof procedures* because practical considerations involving the length of procedure and probability of error, are central. (It is certainly not enough to consider the consequence relation, which suppresses the procedure and confines itself to the end result.) But it cannot be assumed that the ideas implicit in Gentzen's work are *also* exactly right for the analysis of practical, also called "feasible" methods.<sup>16</sup>

#### METAMATHEMATICS OF FORMAL ARITHMETIC: ORDINALS

During the last ten years of his life Gentzen published two consistency proofs (132–201, 252–286; see also Appendix II), a detailed analysis of  $\alpha$ -induction for  $\alpha < \epsilon_0$  (287–308), where  $\epsilon_0$  is the limit of  $\omega_n$  with  $\omega_1 = \omega$  and  $\omega_{n+1} = \omega^{\omega_n}$ , and wrote a note on the contraction of inductions (309–311) which appeared posthumously. Before going into the use of this work for Hilbert's program, which, as already mentioned, raises epistemological problems, let me introduce a twist, roughly speaking under the slogan:

Reductions *to* arithmetic, not reductions *of* arithmetic.

<sup>15</sup> It is tempting to compare (i) completeness with respect to logical consequence (of Frege's rules) to the completeness of, say, Kleene's schemata with respect to the class of recursive functions (Church's thesis) and (ii) Gentzen's rules for the representation of *proofs* to Turing's analysis of mechanical *programs*, that is, of the process of computation, not only of the function computed (sometimes called: Church's super-thesis). But the remark in the text makes me doubtful about the comparison; the analogue of the quite familiar, and fundamental, self-reflection property seems to fail for logic: if we have a program for coding programs  $P_n$  that define number-theoretic functions  $\lambda m f_n m$ , we also have a program for defining  $\lambda n m f_n m$ .

<sup>16</sup> Contrary to an almost universal misconception, constructivity requirements may actually *conflict* with feasibility. Suppose we want to compute a function  $f$  with the action:  $f(x, y, z, n) = 0$  if  $x^n + y^n = z^n$ , and  $= 1$  otherwise. Even for relatively small arguments the direct computation is not feasible (since exponentials grow too fast). *Any* proof of Fermat's conjecture, constructive or not, makes for feasibility (and *any* restriction on the kind of proof allowed may exclude a practical solution).

More explicitly, Hilbert considered universal sentences  $A$  with a “finitist” meaning derived in classical arithmetic and wanted to eliminate detours in their proofs via classical nonfinitist methods. Not the truth of  $A$  was problematic, only the methods of proof needed to establish  $A$ . I first want to consider applications of Gentzen’s work to “genuinely” problematic *extensions* of arithmetic and “reduce” them to arithmetic.—The reader familiar with the theory of constructible sets knows a parallel: Gödel’s model of set theory ZF by use of constructible sets ( $\epsilon L$ ) provides *no* reduction of ZF since  $L$  is defined by use of set-theoretic notions. But it provides a striking reduction to ZF of such problematic extensions as the continuum hypothesis.—The formal problem is to establish conservative extension results, that is, to show that a formula, of suitable syntactic structure, that is derivable in the extension is also derivable in arithmetic itself. Evidently, the practical interest of such a conservative extension result depends on whether or not the extension is used in practice.

Before Gentzen, Gödel’s work on incompleteness provided (the means for establishing) such reductions for *metamathematically* interesting extensions, specifically the extension by the *false* (and hence problematic!) formula  $\sim \text{Con } S$  where  $\text{Con } S$  expresses consistency.<sup>17</sup> Gödel establishes, for a general class of  $S$ ,

$$\text{Con } S \rightarrow \text{Con}(S \cup \{\sim \text{Con } S\})$$

since  $\text{Con } S$  cannot be derived in  $S$  if  $S$  is consistent. Quite generally, if a universal formula  $A$  is derived in  $S'$  then

$$\text{Con } S' \rightarrow A$$

is a theorem of (primitive recursive) formal arithmetic; this is indeed the interest of consistency, which ensures that derivable universal formulas are true. For  $S' = S \cup \{\sim \text{Con } S\}$ , if  $A$  is a theorem of  $S'$ ,

$$\sim \text{Con } S \rightarrow A \quad (\text{in } S)$$

but also

$$\text{Con } S \rightarrow A \quad \text{since } \text{Con } S \rightarrow \text{Con } S'$$

So  $S'$  is conservative over  $S$  for universal formulas. Whatever else may be in doubt, Gentzen provides a similar reduction for extensions of more *familiar mathematical* meaning, namely, extensions by the *negation of the principle of  $\epsilon_0$ -induction*, that is, the negation of a particular instance of the rule (of  $\epsilon_0$ -induction)

$$\text{Derive } A \text{ from } (\forall y \rightarrow x)A[x/y] \rightarrow A$$

<sup>17</sup> Concerning the relation between formulas and the propositions they express, see e.g., §1 (d) of SPT II, cited in fn 9.

where  $A$  contains the free variable  $x$  and  $\rightarrow$  defines the familiar ordering of the natural numbers of order type  $\epsilon_0$ . Gentzen's reduction has had some neat applications, albeit to technical mathematical logic, and not to "ordinary" mathematical practice.<sup>18</sup> Of course the mathematical interest of Gentzen's analysis of formal arithmetic was never in doubt; but now we can exhibit the evidence more concretely.

The foundational interest of all this work is less easy to analyze. Above all—as mentioned already—it is imperative not to be satisfied with facile talk of paradoxes and unreliability, which gives a *false* sense of achievement and stops one from even attempting a searching analysis. But it also helps to remember the *distinction* between a theory of proofs, treated as objects of study, and (Hilbert's) preoccupation with the elimination of abstract methods, aptly described by Prawitz (*op. cit.*) as the distinction between *general* and *reductive proof theory*. Though *some* technical work is relevant to both theories, we shall see in footnote 19 that, occasionally, there is a conflict of aims.

If then we consider a general theory of *arithmetic* proofs—and not only of logical ones treated in Gentzen's analysis of predicate logic (68–131)—probably the principal *strategic* question is this:

Should finite or infinite proof figures be used for the representation of proofs?

Evidently, if the *exact* details of the formal rules of predicate logic make an essential difference to such basic results as normalization theorems, the choice between finite and infinite representations may well be crucial. At any rate, *if* finite figures are to be used, the ideas of Gentzen's second consistency proof (252–286) are probably relevant, when "suitably" extended by its proper, if sophisticated, notion of *normal form* (again, see Prawitz).

For applications to reductive proof theory, the details of the formalization seem largely irrelevant. The crucial issue, the need for an *analysis of  $\epsilon_0$ -induction*, is best expressed by the popular joke:

<sup>18</sup> Particularly for formal systems in the *language* of analysis, but with elementary existential axioms; for example by H. Friedman, particularly p. 437 of his paper in J. Myhill, eds., *Intuitionism and Proof Theory* (Amsterdam: North-Holland, 1970), or the intrinsic role of the ordinal  $\epsilon_0$  (independently of the particular ordering  $\rightarrow$ ) on p. 341 of SPT in fn 10. By [SPT, 332(ii) or 342(2)] Gentzen's work provides also exact formal relations between "mathematical" extensions by means of principles of transfinite induction and "metamathematical" extensions by means of so-called *reflection principles*. (Research has shown that the latter are better for *this* purpose than the notion of consistency, which is equivalent to the reflection principle restricted to universal formulas.)

Gentzen is the fellow who proved the consistency of  $\omega$ -induction, that is, of ordinary mathematical induction, by means of  $\epsilon_0$ -induction.

Certainly a *first* step is to observe that, for the consistency proof, Gentzen needs only *logic-free*  $\epsilon_0$ -induction (though, I think significantly, Gentzen's writings do not even contain an explicit *formulation* of this principle), namely this: instead of inferring  $A$  from  $(\forall y \rightarrow x)A[x/y] \rightarrow A$  as above, we require an explicit term  $\tau$  and a proof of

$$\{\tau(x) \rightarrow x \rightarrow A[\tau(x)]\} \rightarrow A(x)$$

which is *prima facie* a distinctly “weaker” principle since the new premise is “stronger” than  $(\forall y \rightarrow x)A[x/y] \rightarrow A$ . (Of course, the expression  $A$  to which the principle is applied must also be logic-free.<sup>19</sup>) Corresponding to this *inference rule* there is also a *definition principle* of the form

$$\begin{aligned} F(x) &= G(x, F[\tau(x)]) && \text{if } \tau(x) \rightarrow x \\ &= H(x) && \text{if } \sim \tau(x) \rightarrow x \end{aligned}$$

where  $G$  and  $H$  are given terms and  $F$  is defined. But all this is only a first step: one has to make sure that we can convince ourselves of the weaker principle without even hidden use of the functions involved in the meaning of the (classical or intuitionistic) logical operations.

I shall not go into current ideas on further “reductive” analyses of  $\epsilon_0$ -induction, partly because they are not implicit in Gentzen's own writings and partly because enough harm has been done already to progress in foundations by facile claims.

To conclude on a more positive note, let me restate Gentzen's own doubts concerning the significance of his consistency proof, which *can* be settled fairly easily by means of his own work. In Gentzen's own words:

What is the “finitist sense” of a logically complex theorem? that is, one which is not purely universal (and thus finitistically meaningful)?

<sup>19</sup> On the present view of reductive proof theory, iteration of the problematic logical quantifiers in classical logic (but also of implication and negation in intuitionistic logic) measures the *complexity* of an application of induction. Thus Gentzen's contraction of inductions (309–311), which *reduces their number, but increases the complexity*, is to be expected; contrary to the translator's unqualified surprise (recorded on p. 12). But, as Prawitz has pointed out to me, from the point of view of general proof theory, the contraction spoils in general the *normal* character of a derivation.

He returns to this question repeatedly, for instance, at the end of his first consistency proof for arithmetic (201). He has reservations about his own proposal of expressing this sense in terms of the reductions used in his proof because the proposed sense is only “loosely connected” with the form of the theorem considered (and, it might be added, the connection is so tortuous that one couldn’t possibly remember it).

Before giving the answer, it should be stressed, though he himself does not say so—and perhaps did not even realize—that his interest involves an *important departure* from Hilbert’s program. Whatever ambiguities there may have been in Hilbert’s formulations, it is clear that, for foundational purposes, he wanted to treat logically complicated expressions as mere gadgets, “ideal” elements that make the formal theory smoother. The very essence of a consistency proof was to establish the theoretical possibility of eliminating the use of these formulas: for logic-free  $A$  we can conclude  $A$  in a quite elementary way given a formal derivation of  $A$  in a formal system  $F$  and the consistency of  $F$  (for systems satisfying familiar conditions). I said “theoretical possibility” because, as is well known, Hilbert wanted us to do the same mathematics as before after having carried out his program as a kind of *cleansing ritual*.

All this business of eliminating problematic notions (cutting them out because they offend us) may *sound* good to an outsider, but it’s really quite contrary to our intellectual experience. As already mentioned on page 245 (in connection with normalization procedures), our usual way of making arguments in mathematics *intelligible* is different: either we eliminate concepts in practice and not only in theory or else we try to *give* an independent meaning to concepts or steps which, originally, occur as mere technical auxiliaries. A closely related point is this: to the outsider the consistency program sounds attractive because consistency is such a “reasonable,” indeed minimal requirement; but just because it is satisfied by *any* sensible interpretation, the requirement itself gives us no hint of how to satisfy it! (And when we actually try to *discover* a consistency proof, with the ultimate aim of *eliminating* logically complicated formulas, we generally proceed by giving some analysis of the latter (either in terms of Gentzen’s own ideas on the meaning of logical operations which led Prawitz to normalization procedures or of the interpretations in Appendix III).

A perhaps less obvious but, at least to me, even more disturbing defect of the consistency requirement is its *lack of contact with the realities of mathematical experience*. Of course Hilbert gave a meaning to the idea of (finitist) *justification* or *reduction* which has turned

out to be fruitful. But if we are, so to speak, genuinely interested in understanding existing proofs from a finitist view, we know that we are rarely confronted with nonconstructive proofs of logic-free (universal) assertions at all<sup>20</sup>; the *typical* cases are proofs of logically complicated assertions, and the genuine question to ask is Gentzen's: What do *these* proof establish? (from the present point of view).

The first point to notice is that even the "small" step from Hilbert's elementary, that is, purely universal, statements to  $\forall\exists$  formulas greatly increases the practical interest of a proof-theoretic analysis. And Gentzen's work shows that if  $\forall x\exists yA(x,y)$ , for decidable  $A$ , is formally derived in (classical) number theory, there is a term  $\tau$ , defined by  $\alpha$ -recursion (for some  $\alpha < \epsilon_0$ ) described above such that  $\forall xA[x,\tau(x)]$  has an elementary proof (from the defining equations of  $\tau$ ). The scope of this observation is greatly enlarged if we remember that *implications* of the form

$$\forall zC(z) \rightarrow \forall x\exists yA(x,y)$$

with decidable  $C$ , are reducible to (that is, classically equivalent to and intuitionistically implied by) the  $\forall\exists$  form

$$\forall x\exists y[C(y) \rightarrow A(x,y)]$$

Inspection shows that a good deal of practical mathematics is formulated by implications of the form above provided only we carry enough information "along." For example, the property of being a uniformly continuous function (on the rationals) is universal provided we consider such a function  $f$  *together* with a modulus of continuity  $\mu$ , viz.,

$$\forall x\forall x'\forall n(|x - x'| < \mu(n) \rightarrow |f(x) - f(x')| < n^{-1})$$

(Many concrete examples are to be found in the papers cited in footnote 10.)

Evidently  $\exists\forall$  (and logically more complicated) theorems of *classical* arithmetic do not admit "analogous" explicit bounds; for example if

$$(+) \quad \exists x\forall y[P(x) \vee \sim P(y)]$$

a consequence of the law of the excluded middle:  $\exists xP(x) \vee \sim \exists yP(y)$ , admitted a bound  $x_p$  for  $x$ , we could decide, for all elementary  $P$ , whether  $\exists x P(x)$  or whether  $\sim \exists x P(x)$ , by testing  $P(x_p)$ . One way

<sup>20</sup> Sometimes the proof of the irrationality of  $\sqrt{2}$  (expressed by the quantifier-free formula:  $n = 0 \vee m = 0 \vee n^2 \neq 2m^2$ ) is set out as a proof by reductio ad absurdum. But it does not need any logical sophistication to replace: "suppose  $n/m$  is in its lowest terms and  $n^2 = 2m^2 \dots$ " by " $n^2 - 2m^2 \neq 0$  (and hence  $|n^2 - 2m^2| \geq 1$ ) since  $n^2$  is divisible by an even power of 2 and  $2m^2$  by an odd power of 2."

to *express* a “finitist sense” of (+) is to introduce function *variables*, say  $f$ , and consider the  $\forall\exists$  formula

$$\forall f \exists x [P(x) \vee \sim P(fx)]$$

which is classically equivalent to (+); “ $\forall\exists$ ” with the difference that the universal quantifier ranges over functions or “choice sequences.”<sup>21</sup> Explicit bounds are now provided by *functionals*, defined by terms containing function variables, instead of functions; for instance

$$P[\Xi(f)] \vee \sim P(f[\Xi(f)])$$

where  $\Xi(f) = c$  (some constant) if  $P(c)$  holds and  $= f(c)$  otherwise. Provided one is interested in this type of metamathematical result at all, it is read off from any easy extension of Gentzen’s own work. Whether or not these explicit bounds provide a “finitist sense,” they certainly provide an answer to a question which patently concerned Gentzen:

- (Q) What more do we know of a theorem when we have proved it by restricted methods than when we only know that it is true?

There is indeed a genuine question whether “finitist” reasoning (as the term was understood) includes the use of function *variables*. Gentzen himself did not feel at ease about this question, despite his insistence, mentioned in footnote 4, that constructive mathematics should not exclude the mention, but limit the use of concepts of higher type. It is best to discuss the matter by reference to some technical material in Appendixes II and III. But one general point is clear and, I believe, absolutely essential for progress in foundations.

When we start with an epistemological program like Hilbert’s, we must stick to the original meaning of the words. If *finitist* reasoning is not intended to include function variables, so be it: classical theorems may not have an adequate or “manageable” finitist sense. In fact, we may come to think of the (mathematical) problem of finding a finitist consistency proof not only as an end in itself, but also as a prop that helps us reflect on the (epistemological) notion of “finitist,” on its sense and significance. As a corollary, a good answer to question (Q) in a different epistemological scheme indicates an inadequacy of Hilbert’s finitist conception (which, like any other

<sup>21</sup> The general scheme (no-counterexample interpretation) is this:

$$\exists x_1 \forall y_1 \exists x_2 \forall y_2 \cdots \exists x_n \forall y_n A(x_1, \dots, x_n, y_1, \dots, y_n)$$

is replaced by

$$\forall f_1 \forall f_2 \cdots \forall f_n \exists x_1 \cdots \exists x_n A[x_1, \dots, x_n, f_1(x_1), f_2(x_1, x_2), \dots, f_n(x_1 \cdots x_n)]$$

traditional conception, was of course formed at an early stage of foundational research).

As I read Gentzen, he—perhaps more than anybody working on Hilbert's program—was free from dogma and impatience, always prepared to question the significance of his own results, to return to the same problem repeatedly, and to improve his solutions and formulations; not frightened to give them a second thought. So, presumably, he had a deep trust in his ideas and his work.

#### GENTZEN'S DEATH

His serene courage in his work co-existed with—or, perhaps, depended on—an almost staggering lack of contact with reality, that is, with the political realities of the Third Reich and its immediate aftermath (which coincided more or less with his adult life). Insofar as we think of ourselves as controlling our fate, this lack of understanding led to his starving to death in a Czech internment camp before he was thirty-six years old. Specifically he refused<sup>22</sup> the chance of leaving Prague (where he had been *Dozent* at the German University during the last years of the war) before the Soviet troops swept into the city. And even after beatings in the camp (where Germans, who had stayed on, were interned) he said that this was a suitable place for work on the consistency of analysis.<sup>23</sup> Obviously, politically speaking, his actions were plainly wrong. But this is quite a different thing from their being incomprehensible or mad, as they probably appear to people without experience of a society where there is harsh and, above all, centrally organized oppression. One place to look for some understanding is in Solzhenitsyn's writings; perhaps not in the *First Circle* though it deals specifically with scientists and mathematicians in a prison camp, simply because the Zeks in Mavrino Prison are after all conscious and at least mildly critical of the political conditions outside. But there are fine portraits of a political innocent in the *Cancer Ward* (Vadim Zatsyrko) and of the fears and frustrations and the futile scheming and suspicion which eat up the others. Or, at the other end of the scale,

<sup>22</sup> According to a (medical) Dr. F. Kramer, in a letter to Dr. H. Pinl, dated Nov. 23, 1946. The writer attributes the refusal to unworldly "idealism" which, he thinks, is very common among mathematicians. I see no reason to question the overt facts described in that letter; but I see no evidence of the kind of sensibility needed for their interpretation. In particular, the writer simply splutters with indignation at the atrocities in the camp, so much so that he probably really had no thought left for the war-time atrocities by Germans in near-by Lidice and Theresienstadt (or, for that matter, their antecedents), which made some violent reaction inevitable.

<sup>23</sup> According to the eye-witness, Dr. F. Kraus, in a letter to Prof. P. Bernays, dated May 9, 1948. Passages from this letter are quoted, anonymously, in the translator's biographical sketch.

we should perhaps look at Solzhenitsyn himself to see what it takes, by way of moral and intellectual stature, to live under real oppression, face the facts, and still preserve one's humanity (which is, of course, the real issue here). Where there is real oppression, established and severe, opposition requires more than "grand" gestures (which, all too often, are subjectively ambiguous, viz., whenever they are *unconsciously* used to make political or psychological capital).

I never met Gentzen; but I have talked to his fellow logicians Bernays and Schütte who were unsympathetic to his politics, and his friend Witt who was not. From all I heard I get the impression that Gentzen lived within his moral and emotional means and never harmed a fly.

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#### APPENDIX I. On the Relation between Intuitionistic and Classical Arithmetic (53–67)

Gentzen withdrew this paper at proof stage when Gödel's note containing related results, also about propositional and predicate logic, appeared.<sup>24</sup> Both authors considered *fragments* of the classical systems, with the following properties:

(i) The fragments are *classically* equivalent to the full system, in the sense that each formula of the full system is equivalent by classical *logic* to one in the fragment (the fragments are "representative").

(ii) A formula of the fragment is classically derivable if and only if it is intuitionistically derivable.

(iii) (Important for applications, though not stated) The property (ii) extends to *deduction* if the formulas  $A$  and  $B$ , but not necessarily  $A \rightarrow B$ , belong to the fragment.

Gentzen took the fragment built up by  $\{\sim, \wedge, \rightarrow, \forall\}$  from *negated* atomic formulas (if they are not decidable). Gödel omitted  $\rightarrow$ , an inessential difference in that the two fragments are *intuitionistically* equivalent. Both have the property that, for each formula  $A$ ,  $A \leftrightarrow \sim \sim A$  (again of course intuitionistically).

Both authors showed how a classical derivation  $d$  of  $A$  in the fragment considered can be transformed into an intuitionistic derivation  $d^i$  of  $A$ . The proofs are so clear and direct that, at first sight, it seems a waste of time to look for alternatives. What more can we want?

Here we remember the refrain which goes throughout the main review: The *results* refer only to the *sets of* (classical and intuitionistic) *theorems*

<sup>24</sup> "Zur intuitionistischen Arithmetik und Zahlentheorie," *Ergebnisse eines mathematischen Kolloquiums*, iv (1933): 34–38.

in the fragments. So *either* we want to eliminate transformations of derivations altogether *or* we want to give an independent interest to the mapping:  $d \rightarrow d^i$ ; for example, in terms of a theory of proofs, we ask whether  $d$  and  $d^i$  express the *same* proof (and see that, in general, they do not, since, e.g., for  $A$  in the fragment, an intuitionistic derivation  $d^i$  of  $\sim [(\forall x \sim \sim A) \wedge \sim (\forall x A)]$  does and  $d$  does not necessarily depend on the syntactic form of  $A$ ). In fact I do not know any such interest. The first alternative has been pursued by the group of proof theorists at Leningrad who searched for formal systems that are both classically complete *and* intuitionistically valid for suitable fragments; condition (ii) is satisfied automatically (as long as the intuitionistic rules are, formally subsystems of the classical ones); cf. the model-theoretic proof of normal-form theorems on page 246. Their search has led directly to the discovery of more recondite fragments satisfying at least conditions (i) and (ii) and indirectly to striking undecidability results.<sup>25</sup>

Both Gentzen and Gödel have a number of marginal remarks and discussions which seem to me of interest to the modern reader.

Naturally, in connection with his later and longer consistency proof for arithmetic Gentzen formulates questions left open by the present paper, mainly in terms of the abstract character of intuitionistic implication or negation (167–169). Here it should be added that the fragments above do *not* include  $\forall\exists$ ; so we only know that the corresponding  $\sim\sim\forall\sim\sim\exists$  or, equivalently,  $\forall\sim\forall\sim$  formula is an intuitionistic theorem, if  $\forall\exists$  is a classical one; whereas his later consistency proof applies to the  $\forall\exists$  formula itself which, as mentioned on page 253, is important for practice. Incidentally, Gentzen himself was interested in what principles of reasoning are actually used in practice, and refers to the matter at least twice in the same paper! (p. 136, 1.7–10, and p. 170, §11.4).

Gentzen and Gödel stress different aspects of the relation between classical and intuitionistic systems. Gentzen seems impressed by what one may call the *richness* of intuitionistic logic. On page 53, line 5, after noting that the latter is, formally, a part of classical logic, he stresses that this has *purely external* (formal) significance. And on page 66, end of §5.8, he points out that, from his translation, the “decision problem” for classical logic would be solved if it were solved for intuitionistic logic, but that the converse does not follow (easily from his argument.<sup>26</sup>) Gödel concluded from his work that the “only” difference between classical and intuitionistic logic was: eine etwas abweichende Interpretation, that is, a slight change in meaning.

<sup>25</sup> E.g., Maslov, Minc, Orevkov, *Doklady Akademii Nauk*, CLXIII (1965): 295–297, translated in *Soviet Mathematics*, VI (1965): 918–920.

<sup>26</sup> The idea is sound but not the formulation, since, at least in terms of the *current* reducibility notions, the converse does hold, the decision problem being of degree  $0'$ . Perhaps it is more satisfactory to consider condition (i) for the intuitionistic system: although (i) holds for the classical calculus, not only is the intuitionistic fragment not equivalent to the full system, but the latter cannot even be faithfully interpreted so as to preserve consequence in the fragment.

Incidentally, Gödel's speculation (at the end of his paper) on the limitations of the relation can now be analyzed precisely. Gödel thought that the difference between predicative and impredicative principles was crucial, but now one would emphasize, more correctly, the difference between axioms for species and functions, added to predicate logic. Specifically, in the case of arithmetic the principle of induction *can* be added (since it intuitionistically implies its image, by (i), in the fragment); and the same holds also for the impredicative comprehension principle. On the other hand *function* (existence) schemata in general, do not imply their "translations"; e.g.,

$$\forall x \exists ! y A(x, y) \rightarrow \exists f \forall x A(x, fx)$$

for number variables  $x, y$  and function variables  $f$ . In the grand language of the thirties one would express our present knowledge as follows. Whatever doubts there may be about the constructive validity of some (impredicative) principles studied in current literature on intuitionism, the *addition* of the law of the excluded middle is not problematic for any theory of species; but the addition is problematic even for quite elementary and obviously predicative theories of functions. Evidently without close attention to the exact meaning of "constructive validity," the grand language adds little to the formal results quoted. Equally evidently, at the time Gödel's principal interest was in the formal results of the paper cited in fn 24, and not in (slight) differences of meaning.

*Digression on the relation between the full intuitionistic systems and the corresponding fragments.* So-called "economy of presentation" is usually associated with similarities or "unifying principles"; getting things cheaply. But there is also "economy" in getting a lot more for little extra effort, in particular, by looking for more detailed or exceptionally simple treatments of special cases. As an example of the former, quite weak conditions on the concept of *normal* derivation are sufficient for the full language, in the perfectly practical sense that (under such conditions) many useful *derived rules* are apparent which cannot be read off from the usual formalizations. (A rule is called "derived" if, for  $A$  and  $B$  of suitable syntactic structure,  $B$  is derivable whenever  $A$  is derivable, but the implication  $A \rightarrow B$  is not; often with  $B$  of the form  $\exists x(C \rightarrow D)$  when  $A$  has the form  $C \rightarrow \exists xD$ .) These normal derivations need not possess the familiar *subformula property* (87), which seems necessary for useful applications in the case of the fragment (or of classical logic.) As an example of the latter we may compare the expositions of "Beth's tableaux" (for classical logic)<sup>27</sup> and Gentzen's own "cut-free" rules (for the corresponding fragment). Certainly the resulting rules are almost identical, but the *intentions* are quite different. Gentzen's aim was to analyze classical reasoning, in terms of his ideas on interpreting the logical operations (which is certainly not much easier than analyzing intuitionistic reasoning); and the classical

<sup>27</sup> See, for instance, the presentation by Smullyan, cited in fn 14.

rules, especially for  $\rightarrow$  and  $\sim$ , presented difficulties. In contrast, the idea behind Beth's tableaux is to list "simply" *truth conditions* for a counter-model to  $\Gamma \vdash \Delta$  where  $\Gamma$  and  $\Delta$  are sets of formulas  $G, D$  respectively; that is, for given  $\Gamma$  and  $\Delta$ , he finds sets  $\Gamma_i \vdash \Delta_i$  consisting of formulas of *lower logical complexity* with the property: for a counter model (in which all  $G \in \Gamma$  are true and all  $D \in \Delta$  are false) it is sufficient to have a counter model to some  $\Gamma_i \vdash \Delta_i$ . As far as atomic formulas are concerned the only obstacle to a counter model is:  $\Gamma \wedge \Delta \neq \phi$ . Here, in contrast to Gentzen's intentions, the very meaning of  $A \rightarrow B$  is  $B \vee \sim A$  and nothing could be simpler than rules for  $\sim$ ; let  $\Delta = \{\sim D, \Delta'\}$ ; then for  $i = 1$ ,  $\Gamma_1 = \Gamma \cup \{D\}$  and  $\Delta_1 = \Delta'$ . Looked at in this way, by use of a few preliminary lemmas, Beth's analysis can be so presented that we see instantaneously why the rules are going to be *complete*. But this procedure does not remotely look like giving an analysis of the structure of (classical) *proofs*; for example, treating  $A \rightarrow B$  as  $B \vee \sim A$  makes this unlikely.

#### APPENDIX II. Gentzen's First Version of His Consistency Proof for Number Theory (201–213)

He withdrew this treatment (of the last step in the proof) when objections were made to an alleged use of the fan theorem.<sup>28</sup> Though, as we shall see, this specific objection is totally unfounded, Gentzen's more complicated published version presents a quite *significant improvement*.

Roughly speaking, any mathematical result requires a certain amount of *proof power* measured, as one says, by the "product"

$$(\text{combinatorial complexity}) \times (\text{logical complexity})$$

For consistency proofs the second factor is of principal interest, whereas mathematical practice (and reliability in the literal sense of the word!) is mainly concerned with the first; and the published version reduces, or can be used to reduce, logical complexity, that is, the level of abstraction. It would be romantic to suppose that we can always combine greatest simplicity and lowest level of abstraction; as romantic as to suppose that the greatest good will also be the greatest good for the greatest number.—As already indicated on page 254 (and developed in Appendix III), there is a second likely conflict between two requirements: low level of abstraction of the concepts used and *intelligibility* of some (more or less) "finitist" sense for logically complex propositions.

The issues involved in comparing the two versions of Gentzen's first consistency proof are quite general, and so it probably pays to linger over them. First of all, if simplicity were to be used as a criterion, one could not beat the "trivial" argument: the axioms are true (for the natural numbers), the rules preserve truth, and so only true formulas are formal theorems. Incidentally, realistically speaking, such a verification is not

<sup>28</sup> Cf. Bernays's contribution on pp. 409–418 of Myhill, *Intuitionism and Proof Theory*, *op. cit.* The analysis in this Appendix does not agree in all essential points with Bernays's.

trivial: one way of finding out whether we have actually put down the formal rules we have in mind is simply to *read* them in the terms above. If this simplest consistency proof is disregarded because it is not constructive, we have the one in Appendix I which reduced classical arithmetic to the intuitionistic principles formulated by Heyting, principles involving essentially just the two logical operations  $\forall$  and  $\rightarrow$ . (And closer analysis is needed to make explicit the reduction achieved by *limiting the iteration* of these operations; cf. top of p. 212). So the improvement achieved by Gentzen's later consistency proofs will be *convincing* only if it is formulated without use of these operations; in contrast to Bernays's analysis,<sup>28</sup> last paragraph but one on page 417. It is worth observing in passing that various doctrinaire objections to (the "unreliability" of classical or the "unintelligibility" of intuitionistic) principles give us the illusion that such an explicit formulation is "minor"; by presenting those other principles as simply false. Perhaps this is the "method" in the apparent madness of doctrinaire objections.

It seems quite clear that the proper *language* for formulating the first version of Gentzen's proof contains *variables* for natural numbers and choice sequences of natural numbers and constants for operations on these objects (that is, number-theoretic functions and functionals of lowest type). The need for this language is not in doubt, since *free choices* of numbers (in "reductions" performed on universal formulas and conjunctions) are explicitly mentioned by Gentzen, e.g., on p. 203, 1.10 or 1.15; p. 205, 1.8; p. 207, 1.-2. A little more care is required to show that the language is sufficient. Thus one uses the familiar contraction of several arguments of a functional to a single one by means of *pairing* function(al)s and the representation of functionals  $\Phi$  with sequences as values by functionals  $\Phi_1$  with an additional argument,  $\Phi_1(f, n) = (\Phi(f))(n)$ . The critical concept is described by Gentzen's (new) word *Reduziervorschrift*, translated as "reduction rule," in particular in the context (p. 212, 1.6):

A reduction rule is storable.

The intention is patently clear. At an earlier stage Gentzen described, in primitive recursive terms, nondeterministic reduction steps on formal derivations (in fancy language, an *immediate* reduction relation between derivations). The nondeterministic element is, roughly, speaking, that a derivation  $d$  of a universal formula or of a conjunction has, respectively, infinitely many or two immediate "predecessors." Also he describes, primitive recursively, the notion of a minimal element in his reduction relation. Then consistency follows in an elementary way from the *termination of arbitrary sequences of reduction steps*; "arbitrary" for *free choices* (p. 207, 1.-2) of immediate predecessors. As we should say now, consistency follows if Gentzen's reduction relation is *well founded*. Now well-foundedness is *expressed* by giving a reduction rule that tells us the termination of an arbitrary sequence of reduction steps. Specifically, writing  $\alpha$  for choice sequence of derivations (coded by natural numbers) and  $\bar{\alpha}$  for

the immediate reduction relation, we have to express

$$\forall \alpha \exists n \sim [\alpha(n+1) \rightarrow \alpha(n)]$$

that is, an arbitrary sequence  $\alpha$  terminates. This is certainly done if we have a functional constant  $\Phi$  such that, for freely chosen  $\alpha$ ,

$$(*) \quad \sim \alpha[\Phi(\alpha) + 1] \rightarrow [\Phi(\alpha)]$$

So far we have only considered the *language* for formulating the meta-mathematical argument. As Gentzen recognizes quite clearly (p. 221, 1.7—1.13), the *logical* inferences needed are absolutely elementary; as we should say now, purely truth-functional inferences and substitution applied to free-variable expressions (with variables for natural numbers and choice sequences). Consequently, the critical elements are *definition principles* or “defining equations” for the functional constants from which such assertions as (\*) are derivable (by the elementary methods mentioned).

Frankly I rather doubt whether it is of *current* interest to present Gentzen’s particular exposition in full detail; it is the *general choice of language* rather than *precision in its formulation* that seemed problematic. This is so because a precise formulation of this kind of language and a list of definition principles is already in the literature.<sup>29</sup>

Having formulated these principles it is not hard to formulate the improvement achieved by the passage from the unpublished to the published version. We have to do with a *quantitative refinement of the relation between functionals* (of lowest type) *and ordinals*. The qualitative properties of this relation were well-known before Gentzen. By (\*) above, the property of well-foundedness (and, more specifically, the use of proof by induction on  $\rightarrow$  applied to logic-free assertions, and definition by recursion on  $\rightarrow$ , cf. p. 251) is expressed by means of functionals, specifically, functionals that are determined by a finite number of values of their arguments. In the opposite direction, Brouwer asserted<sup>30</sup> that each such functional can be (constructively seen to be) generated by an inductive procedure, and so the theory of these functionals is reduced to properties of ordinals or, perhaps better, well-founded relations. Even if this assertion is plausible (and certainly interesting) it is difficult to make headway with it because the full possibilities of the general notion of functional or, equivalently, of

<sup>29</sup> Cf. W. W. Tait, “Functionals Defined by Transfinite Recursion,” *Journal of Symbolic Logic*, xxx, 2 (June 1965): 155–192, where Ackermann’s consistency proof for number theory [*Mathematische Annalen*, cxii (1940): 162–194] is reworked and presented with an explicit list of the metamathematical principles used. Tait’s exposition incorporates two novel ideas that were published in the fifties: the use of *functionals of lowest type*, in the no-counterexample interpretation which makes the argument more intelligible and the *strategy*, first made explicit by Gödel in *Dialectica*, xii (1958): 280–287, of matching formal rules of inference in the system studied to rules of definition for functional constants in the meta-mathematics. (Gödel uses functionals of all finite, not only of lowest type.)

<sup>30</sup> “Über Definitionsbereiche von Funktionen,” *Mathematische Annalen*, xcvi (1926): 60–75, where what is now called the “bar theorem” is stated.

the general notion of constructive proof of propositions of the form  $\forall\alpha\exists!xR(\alpha,x)$  are not well understood. Gentzen introduced a *mathematical twist*, which can be described as follows. Of course, if we simply read the first version of his consistency proof, we *understand* it in terms of the general (constructive but rather abstract) notion of functional; that is why the proof is so easy to follow. Listing explicit definition principles, as a kind of afterthought, does not alter this situation because we recognize their validity by thinking of them in terms of the abstract notion too. Gentzen's analysis achieves two things. First, it eliminates altogether the problematic aspect of Brouwer's assertion, whether *all* his functionals can be inductively generated; Gentzen has a specific list only, and for these it is possible to *establish Brouwer's assertion mathematically*. Secondly, by introducing a *quantitative ordinal measure* he forces us to pay attention to combinatorial complexity and thereby makes it at least more difficult for us to slip into an abstract reading.

Now, as somebody said, a difference in degree can make a difference in kind. In the present case, as already mentioned on p. 251, this involves an independent analysis of (Gentzen's uses of)  $\epsilon_0$ -induction, that is independent even of the constructive notion of ordinal.

*Remark* on the original objections to Gentzen's proof, in particular, to an alleged use of the *fan theorem*, that is, of the assertion

Constructive functionals defined on choice sequences with *uniformly bounded* values are *uniformly* continuous.

The objections are about as wrong as they can be. First of all, as noted already, the problematic element in the assertion concerns the *general* concept of constructive functional, whereas, as Gentzen insisted, he used only specific ones. Secondly, when reducing derivations of universal formulas, he has to do with choice sequences that may take *arbitrary* natural numbers as values, and so they are not uniformly bounded! In fact, not the fan theorem, but rather the *bar theorem*<sup>30</sup> is involved in Gentzen's proof; or, to be precise, if we think of the latter as an implication, Gentzen uses the corresponding *rule*. As shown in detail by Tait (*op. cit.*), this principle is capable of analysis by means of  $\epsilon_0$ -induction.

Amusingly, the fan theorem would not be enough for a consistency proof of number theory! in the following precise sense: If we add the fan theorem to elementary (intuitionistic) analysis as principal nonelementary axiom scheme, the consistency of the resulting system can be reduced primitive-recursively to first-order arithmetic.<sup>31</sup>

<sup>31</sup> Alternatively, we may add to (elementary) classical analysis, instead of the comprehension axiom, the principle: If a "finitary" tree is infinite it has an infinite path. Incidentally, a nice technical distinction is relevant to the exact *formulation* of *finitary*: the tree is supposed to be given together with an effective method which associates with any node an explicit list of all its immediate successors. It would not be enough to have (an effective description of) a tree such that the set of immediate successors of each node is, nonconstructively speaking, finite.

## APPENDIX III. Functional Interpretations

Below I shall elaborate on Gentzen's aim (201 or 212) of assigning a "finitist sense" to nonconstructively proved theorems, mentioned already at the end of the main review, in particular the relevance of Herbrand's theorem (also mentioned on page 6 of the translation's introduction). The *idea* of assigning such a sense seems to me unmistakable in Herbrand's work<sup>32</sup>; since his discussions are a bit undisciplined it is perhaps less easy to say how central the *intention* of assigning such a sense was. (We all know that, if we let our minds roam, almost every plausible idea will occur to us in a short time; the difficulty is to decide which of them should be pursued.) Be that as it may, to every formula  $A$  of predicate logic he assigns a *sequence* of elementary, that is, propositional formulas  $A_n$  such that (in terms of an *infinite* disjunction) we have

$$(*) \quad (\vdash A) \leftrightarrow \vdash (A_1 \vee A_2 \vee \dots)$$

or, if preferred, such that

$$(\vdash A) \leftrightarrow \exists n \vdash (A_1 \vee \dots \vee A_n)$$

(I purposely spoke of assigning a finitist sense to *proved* theorems since, demonstrably, in general we do not have

$$\vdash [A \rightarrow (A_1 \vee A_2 \vee \dots)]$$

though, for each  $n$ , we do have:  $A_n \vdash A$ ). And if, for proved  $A$ , we have

$$\vdash (A_1 \vee \dots \vee A_n)$$

this may be taken to express the "finitist sense" of:  $\vdash A$ .

Undoubtedly, at Herbrand's time the discovery that *something of this kind* was possible constituted real progress. But already for Gentzen *stronger requirements* became relevant since Herbrand's own formulation was complicated; the finitist sense was "unwieldy," cf. middle of page 212. And a little later it became of interest to state *explicit adequacy conditions* for a satisfactory analysis of the concept of "finitist sense," since (\*) alone can of course be satisfied by sequences  $A_n$  that patently do not provide such a sense (even on a most superficial understanding of the term finitist sense).<sup>33</sup>

I do not know whether it is profitable to put oneself back into Herbrand's position if one is primarily interested in getting new mathematical results in logic; or, for that matter, in a (philosophical) analysis of objective logical relations. It is no "slight" to Herbrand, to recognize that, despite the attraction of his ideas, research has shown other ideas, in

<sup>32</sup> See *Ecrits logiques*, J. van Heijenoort, ed. (Paris: Presses Universitaires de France, 1968).

<sup>33</sup> For such conditions see, e.g., my "On the Interpretation of Non-finitist Proofs," *Journal of Symbolic Logic*, xvi, 4 (December 1951): 241–267, where the no-counterexample interpretation, mentioned in fn 21, is developed.

particular those going back to Gentzen, to be more powerful and therefore more profitable for current research; cf. footnote 1. (Of course, one has to *know* alternative methods to judge between them!) But for the philosopher in the more popular sense of the word, who contemplates the world as it presents itself to us, in particular our *existing* knowledge of the world, it is, I think, rewarding to look at the options open at Herbrand's time, above all:

Was it an oversight, a mere accident, that Herbrand's own formulations were complicated?

I do not think so; referring to his *Ecrits*,<sup>32</sup> his description of "finitist" methods (which he calls "intuitionist") bottom of p. 210, certainly excludes the use of function *variables* altogether, incidentally in contrast to Gentzen, cf. footnote 4 above and of course Appendix II; like Hilbert, Herbrand seems firmly convinced that abstract or "transcendental" methods are eliminable from proofs of arithmetic theorems (page 152 in italics, apparently without restriction on their syntactic structure), but I cannot find any mention of the greater *intelligibility* of abstract methods which Hilbert stressed so much (after his long experience in mathematics.) It should not be too hard to show, for a natural measure of *complexity*, that there just is no simple "finitist sense" for logically complicated (proved)  $A$  if the  $A_n$ , as in Herbrand's formulation, do not contain function variables; that is, if the only function symbols in  $A_n$  also occur in  $A$  and therefore act as *parameters*. Thus the no-counter example interpretation (see fn 21) which, as has been stressed,<sup>33</sup> was suggested by Herbrand's work (or, more precisely, by the particular proof given in Hilbert-Bernays), would not have satisfied his requirements.<sup>34</sup>

Unquestionably not only "result-conscious" technicians, but also contemplative philosophers are reluctant to pursue the kind of distinctions made above in great detail. There are some evident superficial reasons; it may seem churlish to criticize pioneers (as if people like Herbrand or Gentzen needed that kind of patronage); understanding pioneer work is, subjectively, not too different from making a new discovery, and at least some people are so attached to (or exhausted by) their own productions that they think of them as "definitive." But perhaps the most powerful reason is simply the fear that the whole fabric would come to pieces if one examined it too closely, that distinctions would be followed by further distinctions, and so on. (On page 255 I alluded to Gentzen's obvious freedom from this kind of fear.) This seems a pity since, as a matter of fact, our logical ideas have stood up well to examination: distinctions *are* needed, perhaps unexpected ones, but remarkably few.

*Remark.* On p. 6, 1.-15 of his introduction, the translator finds it necessary to explain why Gentzen considered Herbrand's theorem a special case of

<sup>34</sup> Gödel's formulation of a *completeness theorem*, in terms of the nonconstructive notion of model-theoretic validity, constitutes an (incomparably more significant) advance which is even further removed from Herbrand's intentions (but, as one says, easily obtained from his formal machinery); cf. pp. 143-144 of his *Ecrits*, *op. cit.*

his own result, but makes a hopeless job of it. Gentzen's principal topic was the intuitionistic calculus which, as we know from Appendix I, has a more delicate proof-theoretic structure than the classical calculus. (There is no need to go into details of recent "Herbrand-style interpretations" of intuitionistic predicate logic by Mints,<sup>35</sup> since Herbrand himself had not touched this case at all.) In the translator's view, presumably second-hand, Gentzen overlooks Herbrand's own formulation for *non-prenex* formulas, and its extra "information" (p. 6, 1.-10). This view overlooks the fact that, in classical predicate logic, *every* formula has a prenex normal form and, more importantly, the corresponding  $A_n$  have a more graspable and hence useful content. As to the extra information, he does not ask whether we really want it, whether it improves the efficiency ratio of: interest of result to effort involved. No, to state the difference between Herbrand's and Gentzen's material it is not enough to toy with superficial formalities. As we have seen we need something like Herbrand's theorem to satisfy Gentzen's own demand for a "finitist sense." It is a separate matter whether we should use the strategy (cf. Gödel, *op. cit.*, fn 24) of establishing the interpretation directly, or whether we should first establish a "normal-form" theorem (cf. Mints, *op. cit.*). The latter or, more precisely, a "normalization theorem" is needed if we want to treat proofs as principal objects of study; or, in Prawitz's terminology, if we are also interested in general proof theory—like Gentzen and unlike Herbrand whose interests were mainly reductive. Incidentally this difference is quite consistent with the relative role of intuitionistic systems in their work which systems do lend themselves very well to Gentzen's analysis but less so to interpretations.

*Nachgelassene Schriften.* GOTTLÖB FREGE. HANS HERMES, FRIEDRICH KAMBARTEL, and FRIEDRICH KAULBACH, editors. Hamburg: Felix Meiner, 1969. xli, 322 p. DM 74.

#### I. HISTORY

When Gottlob Frege died in 1925 he left a parcel of manuscripts to his adopted son Alfred with a note attached saying:

Dear Alfred,

Do not despise these papers written by me. Even if they are not pure gold, there is gold in them. I believe that one day some of the things in them will be valued more highly than now. See to it that nothing is lost.

In love your Father.

There is a great deal of myself that I leave you in these papers.\*

<sup>35</sup> "Disjunctive Interpretation of the LJ Calculus," pp. 86–89 of: X. Slisenko, ed., *Seminars in Mathematics*, vol. VIII (1970), translated from the Russian, Consultants Bureau of New York. Mints *uses* Gentzen's normal form for LJ as a tool for his interpretation (to be thought of as analogous to Herbrand's theorem). The content of the corresponding  $A_n$  is not very easy to grasp.

\* *Nachgelassene Schriften*, xxxiv; hereafter NS; translations by reviewer.