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An assessment of the situation in research on proof theory

(1950)

Zur Beurteilung der Situation in der beweistheoretischen Forschung

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In speaking here briefly about the situation in proof theoretic research, it seems appropriate to recall what is characteristic of this research: it is the systematic investigation of the various applications and consequences of logical reasoning in the mathematical disciplines, in which the concept formations and the assumptions are fixed in such a way that a strict formalization of the proofs is possible with the help of symbolic logic as a means of expression.

As you know, Hilbert initiated this kind of investigation mainly through questions of consistency. But he also envisaged from the start the treatment of questions regarding the completeness and decidability in the context of these investigations, for example already in the lecture "Axiomatic thinking" (1917, *vide* [**?**]). He formulated questions regarding completeness in more detail in the lecture "Problems of the founding of mathematics" in Bologna (1928, *vide* [**?**]).

To be sure, Hilbert assumed many things to be simpler than they eventually turned out to be, regarding both the results to be obtained and the method. The knowledge of these major difficulties awakened in many the idea that proof theoretic research has experienced a definitive failure. But a glance at the actual state of affairs shows that there is no question of this: the methods of proof theoretic consideration find themselves in a state of rich development and respectable results have been obtained in various directions. Let me list some noteworthy successes regarding the problems Hilbert formulated:

1. Gödel's Completeness Theorem (proof of the completeness of the first order predicate calculus) together with its related extensions.

2. One succeeded in making the concept of decidability precise in such a way that systematic results could be obtained on the basis of this definition, in particular the proof of the unsolvability of the decision problem for predicate calculus by Church and, in a second way, by Turing.

3. While the aforementioned methods lead only to conclusions concerning undecidability, Tarski succeeded, on the other hand, in specifying decision procedures for certain mathematically non-trivial domains. In connection with these results, as well as through results supplementing Gödel's completeness theorem, there have been applications in mathematics which are also of interest to mathematicians not concerned with foundations.

4. Regarding the questions of consistency, a consistency proof for full analysis has not been achieved from the finitist standpoint, but one has been obtained for restricted analysis (for example in Weyl's sense or in the sense of ramified type theory) from a constructive standpoint. Gentzen first supplied such a proof for the number theoretic formalism; but Gentzen already had in mind the extension of his method to ramified analysis. This has been carried through by Lorenzen, Schütte, and Ackermann, whereby the method of proof also became more transparent. Also to be mentioned is a new transparent consistency proof for number theory by Stenius. Furthermore, it is remarkable that the extension of the finitist standpoint to the constructive standpoint in a broader sense makes it possible to consider proofs that do not have to be formalized in the full sense, but can contain parts in which metamathematical derivations can be specified which sometimes depend on a syntactical numerical parameter. In this way, one transcends the domain of those systems to which Gödel's incompleteness theorem applies.

Incedentally, this important theorem is by no means to be judged only as a negative result; rather it plays a role for proof theory similar to that of the discovery of the irrational numbers for arithmetic.

5. Finally, efforts have been made to supplement the statement of consistency with a more general form of question: what can be extracted from the formal provability of a theorem, from the constructive standpoint? Kreisel's investigations move in this direction.

Given all this it would obviously be totally inappropriate to speak of a general crisis in proof theory. On the other hand it must be acknowledged that not only has the most essential work in this domain still to be done, but also that, regarding the methodology, there is no clear resolution and no unanimity. I would like to raise a few points in this connection.

One speaks today a bit condescendingly about "naive set theory." We must, however, remind ourselves that it is, in any case, naive to think that, by a retreat to the axiomatic standpoint, without any contentual approach supporting it, we have at our disposal anything like what we started with. The retreat to the axiomatic in the case of non-Euclidean geometry is less problematic, because there we take arithmetic and set theory, as given knowledge, as a foundation. The discussions about possible geometries, in particular the model theoretic considerations, take place within the framework of arithmetic (analysis). By challenging this framework and assigning to set theory itself the role of an axiomatic theory, it becomes necessary to determine a different underlying framework which has to act as arithmetic proper. Different views are possible with regard to the choice of this methodological framework.

The minimal requirement for a sharpened axiomatization is that the objects not be taken from a domain that is regarded as being antecedent, but that they be constituted by generating processes. But one could take the meaning of this to be that these generating processes determine the extensions of the objects; this point of view motivates the law of *tertium non datur*. In fact, the openness of a domain can be understood in two senses: on the one hand, that the processes of construction lead beyond any single element, and on the other hand, that the resulting domain does not represent a mathematically determined manifold at all. Depending on whether the number sequence is understood in the first sense or in the second, one obtains the acknowledgment of *tertium non datur* with respect to the numbers, or the intuitionistic standpoint. For the finitist standpoint, the requirement is added that the considerations have to be made by means of investigating finite configurations, thus, in particular, assumptions in the form of general statements are excluded.

The maximal requirement for the methodical framework goes beyond even that of the finitist standpoint. This standpoint in fact contains existence assumptions, required for the possibility of systematic considerations, which are not self-evident from the standpoint of the properly concrete. For example, the application of such existence assumptions is necessary if we want to show the eliminability of complete induction in the sense of Lorenzen. Originally, Hilbert also wanted to adopt the narrower standpoint which does not presuppose the intuitive general concept of numeral. This can be seen from his lecture in Heidelberg (1904), among others. It was already a compromise of sorts that he decided in favor of adopting the finitist standpoint in his publications. If we reflect on this, then the need for the transition from the finitist standpoint to an extended constructive standpoint does not appear so catastrophic.

To be sure, this requires a philosophical adjustment. Many think that one either has to accept only absolute evidence, or that evidence has to be generally abandoned as a feature of the sciences. Instead of this "all or nothing" attitude, it appears to be more appropriate to understand evidence as something that is acquired. A man obtains evidences the way he learns to walk, or a bird learns to fly. One comes hereby to the Socratic acknowledgment of our basic inability to know in advance. In the theoretical realm we can only try out different points of view and standpoints and possibly have intellectual success with them.

This does not mean that, with these points of view, the problem of the foundations is already solved in principle. But at least such modesty allows that we not be completely disconcerted whenever new antinomies are discovered. Such antinomies then appear rather to be instructive clues for the right choice of our approach and methods.

The problem in foundational research that has still to be overcome consists of different aspects: on the one hand, the choice of the methodological standpoint in foundational research, as well as the choice of the deductive framework, and on the other hand, the understanding of mathematics. With regard to this second point a decision is perhaps not to be expected by means of foundational research, but in respect to the first questions it is not too immodest to hope that the comparison of the results of the different directions of research will show a clear advantage to one of the ways of proceeding in the foreseeable future.

DISCUSSION

Arnold Schmidt: My introduction of degrees of consistency, which was mentioned by Mr. Bernays, was merely meant to emphasize the problem of the role that consistency plays epistemologically. [...]

With regard to the extensions that the finitist standpoint has experienced in the course of its development I would like to remark that *tertium non datur* remains excluded at all stages of this development.

With respect to the problem of evidence one can say the following, in a certain analogy to the interpretation of the Kantian *a priori*: the individual can obtain evidence through reflection, but the criteria for the evidence must be independent of such experience in order to rule out deceptive evidence which can arise by habituation. As much as I acknowledge that the matters of fact which are not evident at first sight can become evident by a thorough clarification, I want to emphasize, on the other hand, that in my opinion there can be *only one* kind of evidence, thus no relative or graduated evidence. From this point of view the task of the proof consists in reducing something that is not evident to something that is evident.

Paul Bernays: There is no disagreement with regard to the first point. With respect to the second remark I'd like to call attention to the fact that I did not intend to write history. Had this been the case, I would have distinguished five stages of metamathematics: 1. the finitist standpoint, 2. the definite standpoint ((1) with existence assumptions), 3. Intuitionism, 4. *tertium non datur*, 5. impredicative concept formation. This ordering gives more and more freedom. While it was possible to point out intimate agreements between Intuitionism (3) and the classical standpoint (4), this has not succeeded for (4) and (5) although Gentzen struggled with it. Thus the decisive point lies beyond the introduction of *tertium non datur*. Finally I would like to say that one should not construe evidence only objectively, forgetting about subjective determinations. [...]
[In response to Behmann, who adduced Helmholtz's argument for the "evidence" of non-Euclidean geometries]: Although differently constituted beings could have a different conception of evidence, it is our concern to determine what counts as evident for us. [...]

Alfred Tarski: [...] Furthermore I should like to remark that there seems to be a tendency among mathematical logicians to overemphasize the importance of consistency problems [...]. Gentzen's proof of the consistency of arithmetic is undoubtedly a very interesting metamathematical result, which may prove very stimulating and fruitful. I cannot say, however, that the consistency of arithmetic is now much more evident to me (at any rate, perhaps, to use the terminology of the differential calculus, more than by an epsilon) than it was before the proof was given.

Paul Bernays: My thought has not been rightly interpreted. I did not wish to say that Gentzen's proof made arithmetic, or truths about arithmetic, more evident. But I tried to stress that some mathematical methods simultaneously show deducibility and validity. [...]