THE ANALYSIS OF MATHEMATICAL TEXTS,
AND THEIR CALIBRATION IN TERMS OF INTRINSIC STRENGTH II

by

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This is the second in a series of reports on an ongoing mathematical program that will take me several years to complete. The first report was left undated by mistake, and should carry the date, April 3, 1975.

This report concerns the relation between our program, as outlined in the April 3 report, and Hilbert's second problem and Hilbert's program. I attach a copy of the relevant pages of the English translation of the Hilbert problem list that appeared in the July, 1902 Bulletin of the AMS. I also attach a copy of the semi-formal system Hilbert was referring to that appears in Morris Kline, Mathematical Thought from Ancient to Modern Times, Oxford University Press, 1972.

In his second problem, Hilbert asks for a demonstration of the consistency of the proper axiomatizations of branches of science. He is particularly concerned with the particular branches which he refers to as "geometry" and the "arithmetic of real numbers". He refers to his semi-formal axiomatization of "geometry", and his semi-formal axiomatization of the "arithmetic of real numbers".

In his second problem, Hilbert gives no account of the methods he would allow for a consistency proof. Historically, it is clear, at least roughly, what he would allow. In particular, see his 1925 article, On the infinite, in Philosophy of Mathematics, edited by Benacerraf and Putnam. This much can be said: Hilbert was expecting consistency proofs to use rather elementary methods, which nowadays we would regard as being formalizable in a system such as PRA (primitive recursive arithmetic), or Skolem arithmetic, as it sometimes is called.

As far as the particular semi-formal system of Hilbert is concerned, it is difficult to pin down which of the myriad ways of making it formal is most akin to Hilbert's conception. In any case, at the time he wrote the problem, the subtle distinctions involved in the different ways of formalizing his semi-formal system were not known to him. (Different formulations lead to different logical strengths.)

Gödel's results showed that no matter how Hilbert's system is made formal, as long as it is reasonably done, so as to, at least, codify directly quite elementary analysis, then the system is not provably consistent in PRA. So the answer to Hilbert's second problem is NO, constrained to Hilbert's "arithmetical axioms" and PRA.

Since Gödel's result, the logic community, including Hilbert and Gödel, have taken the position that one should not subject Hilbert's second problem to such constraints as above. In particular, Gödel and Hilbert, at least implicitly, proposed that the upper bound of PRA for the acceptable methods for proving consistency, be dropped.

The following is what is commonly regarded today as an upper bound to what is "finitary".

> free variable induction on each ordinal below ϵ_0; or $< \epsilon_0$ - induction for short.

(Gentzen showed that the consistency of each finite fragment of Peano arithmetic could be proved consistent by means of $< \epsilon_0$ - induction.)

However, Gödel's results still showed that, no matter how Hilbert's system is reasonably made formal, so long as it codifies directly farily elementary analysis, it cannot be proved consistent by means of $< \epsilon_o$ - induction.

Now Hilbert's basic concern in the second problem was proving the consistency of the proper axiomatizations of science; in particular, geometry, and the "arithmetic of the reals". Later, e.g., in <u>On the Infinite</u>, he explicitly spoke of the program of "establishing throughout mathematics the same certitude for our deductions as exist in ordinary elementary number theory, which no one doubts and where contradictions and paradoxes arise only through our own carelessness".

The following remained open, despite the negative results of Gödel.

> Can you find a new type of formalism for "geometry", "arithmetic of the reals", or "analysis", which properly codifies any of these branches, and give a consistency proof of these by means of $< \epsilon_o$ - induction?

After all, if someone found another way of looking at analysis, gave a new formalism for analysis, and then proved the consistency of that formalism by means of $< \epsilon_o$ - induction, then presumably Hilbert would have accepted this as a positive solution to his second problem and his program, as regards analysis (to the extent that he would agree it codified the whole of analysis).

The above is simply not ruled out by Gödel result, and only a careful study of actual analysis can definitely rule it out. An early consequence of our program is the following negative result.

> No consistency proof of even elementary actual analysis can be given by means of $< \epsilon_o$ - induction. That is, no formalism that codifies elementary

> actual analysis can be proved formally
> consistent by means of $< \epsilon_0$ - induction.

From our vantage point, this is the first definitive negative result on Hilbert's second problem. The extent that it is not definitive is the extent that $< \epsilon_0$ - induction is an upper bound on "acceptable methods". But we actually show:

> No consistency proof of certain nonelementary
> actual analysis can be given by means of Z_2 =
> second order arithmetic.

Z_2 is an extremely strong system, for, far beyond anything anyone has the nerve to even propose as acceptable for this problem.

In my program, I determine the following.

> Which specific methods are, or are not,
> sufficient to prove the consistency of
> which specific bodies of mathematics,
> idealized or actual?

Far ranging positive results are obtained.

Two competing approaches. Let us now look at my program, versus the classical program for attacking Hilbert's second problem and program.

The classical approach is as follows. Let us be considerable more liberal in what we allow to be used in our consistency proofs - but still insist that the methods used be remotely "finitary". In this approach, the systems to be proved consistent are fragments of set theory based on impredicative conprehension principles.

My own approach is as follows: Construct new formal systems tied directly to the mathematics under investigation. (Impredicative comprehension principles, as such, are never axioms in such systems, though impredicative principles of other kinds are). Then see precisely which of the standard specifically formalized methods suffice, or do not suffice, to prove the consistency of these newly constructed formal systems. I abandon the search for a grand extension of "finitary methods", and consider only standard specified methods.

The classical approach, beyond perhaps Gentzen, has remained unfruitful (I grant that interesting technical work, and important discoveries unrelated to the issues at hand have resulted). NO inkling of any powerful generalization or systematization of the informal concept of "finitary" has emerged. The work has often been dull, boring, tedious, overly complex, routine, unpenetrable, poor, dry, and unsatisfying, either singly or in concert. I know of no major active proof theorist who is optimistic about this classical approach.

On the ensuing reports, in which I will get down to the business at hand, instead of talking in generalities as I have done in these first two reports, I invite you to compare my approach with the classical approach. As Hilbert said in <u>On the Infinite</u>, (admittedly in a different context,)

> ... such success is in fact essential, for in mathematics as elsewhere success is the supreme court to whose decisions everyone submits.