Address at the Princeton University Bicentennial Conference on Problems of Mathematics

Hourya Benis Sinaceur

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ADDRESS AT THE PRINCETON UNIVERSITY BICENTENNIAL CONFERENCE ON PROBLEMS OF MATHEMATICS (DECEMBER 17–19, 1946), BY ALFRED TARSKI

Edited with additional material and an introduction by HOURYA SINACEUR

Abstract. This article presents Tarski's Address at the Princeton Bicentennial Conference on Problems of Mathematics, together with a separate summary. Two accounts of the discussion which followed are also included. The central topic of the Address and of the discussion is decision problems. The introductory note gives information about the Conference, about the background of the subjects discussed in the Address, and about subsequent developments to these subjects.

The Princeton University Bicentennial Conference on *Problems of Mathematics* took place during December 17–19, 1946. It was the first major international gathering of mathematicians after World War II. About eighty guests participated, among them a dozen from abroad and many of the leading American mathematicians. In 1947 the University printed a pamphlet (henceforth called the Pamphlet) containing a foreword by Solomon Lefschetz, Director of the Conference, a list of eighty-two invited participants, a program of ten sessions on different fields of mathematics, and a summary of the discussion in each of the sessions with a brief foreword by J. W. Tukey, Conference Reporter, who announced that a volume of proceedings would be published under the title: *Problems of Mathematics*. However, this project was never carried through because of lack of funds.

§1. The printed Pamphlet. From the Conference Program printed in the Pamphlet we learn that the session on Mathematical Logic took place on the first evening of the Conference, Tuesday, December 17, after dinner at 8:00 p.m.[1] Alonzo Church was chairman, Alfred Tarski discussion leader, and

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I am greatly indebted to Leon Henkin, who gave me copies of the material published here and other relevant documents, encouraging this editing project, and was always willing to listen to my questions and to answer them. I thank the Editors of BSL for their encouragement and especially Charles Parsons for profitable discussions and remarks. Last but not least, I thank warmly John W. Addison, Martin Davis, Solomon Feferman, Wilfrid Hodges, and Robert Vaught, for extensive and very precise comments and corrections on

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J. C. C. McKinsey reporter. According to the list published in the Pamphlet, other participating logicians were Kurt Gödel, Stephen C. Kleene, W. V. Quine, Raphael M. Robinson and J. Barkley Rosser. Some of these gave invited talks. A summary of the mathematical logic session was prepared by McKinsey, but what was printed in the Pamphlet was not his original report. In a letter to Tarski, dated May 18, 1947, McKinsey wrote: "I received the little booklet from Princeton describing the logic meeting there. This has been changed very much since I wrote it. As you notice, Tukey says in his introduction that he takes responsibility for the omissions — and it seems to me that in the case of the logic session he has omitted a great deal that I put in."[2] Moreover, from McKinsey's letter dated May 5, 1947, we learn that "the «popular» account of the logic session" did not please Tarski. Nevertheless, it is interesting to republish this account here along with the transcript of Tarski's talk (B1) and the general discussion (B2). This account, consisting of McKinsey's report as modified by Tukey, will be named (C) in what follows. The document (C) differs from the transcript (B1) on one significant point, apparently quoting Tarski's own words (see Note 28 to (B1)).

The account (C) contains no listing of the individual talks made in the session. It just mentions Church, Tarski, Gödel, Kleene, McKinsey, Quine, and Rosser as speakers in the session, without specifying in each case whether the person in question gave an invited talk or only spoke during the general discussion, after the invited talks. However, two living logicians have given

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my drafts. Thanks to Mary Ann Addison, who is Alonzo Church's daughter, I became aware that the Alonzo Church *Nachlass* contains a folder entitled "Bicentennial Conference Correspondence". Addison sent me some material from this folder that I could use before completing my work. A particular debt of gratitude is owed to Addison, who made new suggestions on the entire manuscript in its next to last draft. I thank also Jan Tarski for comments and suggestions on a late version. I remain, however, solely responsible for any mistakes encompassed in the text. I also wish to thank the Dibner Institute for the History of Science and Technology, at the Massachusetts Institute of Technology for a fellowship during the fall 1997, during which time much of the preliminary work on this introduction was done.

(Note by the editors of the ***Bulletin***) Of the documents published below, the first three, "A summary of the address to be given by Alfred Tarski at the conference on The Problems of Mathematics in Princeton, December 17, 1946" (A), "Talk by Prof. Tarski at the Logic Conference, December 1946, Princeton" (B1), "Discussion in the conference on logic held at Princeton Dec. 17 1946" (B2), are in the Alfred Tarski papers (BANC MSS 84/69 c, carton 12, folder 4) in the Bancroft Library of the University of California, Berkeley. They are published here by permission of the Bancroft Library, the first two also by permission of Dr. Jan Tarski. The remaining document, "Mathematical Logic" (C), is excerpted from the booklet ***Problems of Mathematics***, issued by Princeton University in 1947. It is reprinted here by permission of Princeton University.

The editors wish to thank Leon Henkin and Jan Tarski for bringing the documents (A), (B1), and (B2) to light and encouraging their publication, and the staff of the Bancroft Library and Solomon and Anita Feferman for assistance.

evidence concerning the logic session. First Quine, in his *Autobiography* (pp. 195–196) published in 1985, wrote that he "was invited to participate along with Gödel, Kleene, Rosser, and others". He writes there about Gödel's talk, which was likely preceded by Rosser's. Indeed, Quine describes Gödel pacing back and forth and glancing "at a pair of symbols that Rosser had happened to leave on the blackboard". Second, Leon Henkin was working on a Ph.D. dissertation at Princeton University under Church at that time and attended the session on mathematical logic. He remembers being asked by A. W. Tucker, one of the Conference organizers, to take notes on "three of the talks to be given there: Church, Gödel, and Tarski". He remembers that Quine was another invited speaker, and possibly also Kleene. Since Church was chairman and McKinsey was the reporter for the session, the invited speakers were, as I presume, Tarski, chosen as "discussion leader", Gödel, Kleene, Quine, and Rosser. This is corroborated by a letter from Church to Tarski dated June 20, 1946, kept in the Church *Nachlass* and sent to me by John W. Addison Jr. when my work was nearly complete. Let me quote large passages of this letter:

> "The forthcoming conference on mathematics to be held at Princeton is intended to be a conference on *problems* in mathematics. The speakers at each session will be asked to devote their time, not to setting forth new results of their own or to explaining the results of others, but to a survey of important outstanding problems in the field of that session, with discussion where appropriate of the significance of the problem, and possibly suggestions towards an attack on it. Such speeches should not be expository, for the benefit of persons ill acquainted with the field, but should be addressed to those actively interested in the field. (Of course it is not meant by this to exclude discussion of the relevance of the particular field to other fields; on the contrary, this question of interrelationship of different fields will often arise in connection with particular problems.)
>
> In special connection with the session on mathematical logic, besides yourself, Kleene, Quine, and Rosser have received invitations and have accepted. No special invitations were issued to persons who will be at Princeton University or at the Institute but of course we are counting also on the presence of Gödel and McKinsey.
>
> My plan for the session is that you should give the leading address, devoting thirty-five minutes to a survey of problems in the field of mathematical logic, and that this would be followed by ten-minute addresses by Gödel, Kleene, Quine, Rosser. You would be asked to prepare your address long in advance and to send copies to the other four speakers and to me. The function of the other speakers would be to discuss further the problems proposed by you, and to introduce additional problems, especially such as they were themselves actively

> interested in. As chairman I would reserve the right to include an address myself at any point and of any length — or not to speak myself at all — the decision to be made after seeing whether I had anything to add to the remarks of others.
>
> After the prepared addresses the meeting would be thrown open for general discussion, in which, of course, further remarks by the original speakers would not be excluded. McKinsey would be asked to serve as reporter, to prepare the material for publication. This would give him a heavy responsibility especially in connection with the impromptu discussion, as of course he would be provided with copies of the prepared addresses.
>
> I am writing to ask whether you approve the plan, and especially whether you will undertake the principal address as proposed."

Clearly Tarski accepted the proposal. From the Church correspondence folder we also learn that Tarski did indeed prepare his summary in advance. Kleene, in a letter to Church dated November 29, 1946, says, "I got Tarski's summary of his talk a little over a week ago, and I did want to orient my brief one in relation to that". Tarski himself alludes to the prepared summary he sent to Gödel too before the Princeton Conference. In a letter to Gödel dated December 10, 1946, recently edited by Jan Tarski,[3] he says: "As you see from the summary of my talk, my program is rather ambitious and probably too comprehensive; and you can easily realize that within a 30-minute talk I shall merely be able to touch upon most of the problems involved."

The account (C) begins by stating that "the discussion revolved about a single broad topic — decision problems". Actually, that was the topic of Tarski's talk, which was the "leading address" of the session. Taking his point of departure from Tarski's lecture, Gödel wrote some remarks on possible absolute notions of definability and demonstrability. The four-page paper was sent to McKinsey for the planned proceedings that never appeared. It was first published in [Davis, 1965, pp. 84–88] and has since become very well known.[4] By way of contrast, Tarski's talk remained unpublished. What happened to the other invited talks? In a private meeting arranged by Charles Parsons on January 20, 1998, I asked Quine whether he had kept a draft of his Princeton talk. He said that he "had no submitted paper and cannot remember what he talked about". I wrote to the Archives at the University of Wisconsin, where S. C. Kleene taught mathematical logic. I learned from the Director of the Archives that the box that contains Kleene's papers from 1946 contains only correspondence and manifestly no draft of the Princeton talk. As for Rosser's talk nothing could be found that would appear to be of reference to the Princeton Conference, at least at the Mathematics Research Center in Madison, Wisconsin, where Rosser spent the last years of his career in the 1960's and 1970's.

§2. The material published here for the first time. In Fall 1994, Jan Tarski brought Leon Henkin a copy of the material that had been found earlier by Jan Woleński in one of the boxes of his father's papers that had been deposited in the Bancroft Library after Tarski's death in October 1983. This material consisted of three parts. Part (A) is "A summary of the address to be given by Alfred Tarski at the Conference on The Problems of Mathematics in Princeton, December 17, 1946"; part (B1) is a ten-page typed document entitled "Talk by Prof. Tarski at the Logic Conference, December 1946, Princeton"; part (B2) is a six-page account of the discussion following the invited talks.

Church's letter to Tarski dated June 20, 1946, Kleene's letter to Church dated November 29, 1946, and the recently published letter from Tarski to Gödel quoted above confirm that the summary — part (A) — was written by Tarski himself in advance. Typed neatly, (A) contains no errors while (B1) and (B2) have some typographical errors and some typed words crossed out and replaced by hand-written ones. According to Henkin,[5] Tarski was not accustomed to write out, in advance, the text of a talk to be given and (B1) "projects very much the sense and the sound of Tarski's *speaking*".[6] On the other hand, Solomon Feferman, who attended Tarski's classroom lectures during 1948–1953, remembers that "Tarski often prepared notes, some quite systematic, in advance, on folded sheets of paper, so that these read like a little book. He did not necessarily refer to these notes directly in his lectures since, having prepared them, he had clearly in mind what he was going to say." The prepared notes for the Princeton talk are the summary (A). As for the text of the talk, Tarski *did not* write it down, in advance, even though he was asked by Church to "prepare [it] long in advance and to send copies to the other four speakers and to me". In his letter to Gödel dated December 10, 1946 mentioned above, Tarski says indeed: "Unfortunately, I cannot send you a copy of my talk since I have none and I doubt whether the text will be written down before my talk."

Thus, (B1) could be a transcript made from a tape-recording of Tarski's talk, as Henkin supposes, or else an account prepared from notes taken by someone who attended the talk. The latter possibility is supported by McKinsey's letter to Tarski, dated February 10, 1947. McKinsey indeed began by writing: "I enclose copies of a text that has been prepared of your address at Princeton, and also of the discussion which followed the prepared addresses. These accounts were prepared from notes taken by Mr. L. Henkin and Kleene. I'm sorry to say that I felt so worried and depressed the night of the meeeting that I wasn't able to take any notes myself. I hope you'll be able to find the time soon to go over these manuscripts and offer suggestions or corrections."

Obviously, Tarski found no time to go over the copies he received from McKinsey, that is the parts (B1) and (B2) of the material published here for the first time. Actually, on April 19, 1947, McKinsey made a new attempt, writing: "I wish you would please look over the manuscript of your Princeton address and send it back to me." In a third letter (May 5, 1947), McKinsey noted: "It's too bad you won't be able to give any time to the account of your talk and of the discussion." In a fourth letter (May 18, 1947), McKinsey complained that his own report had been much shortened by Tukey and urged Tarski to go over his talk "as soon as possible". Tarski very probably never went over the manuscripts of his talk and the general discussion at the logic session of the Princeton conference, as the Proceedings volume did not materialize. It is really a great pity, but (B1) and (B2) are not in the final form Tarski would have given them. Indeed, it is fairly certain that the handwritten corrections on some words of (B1) are not Tarski's. I publish (B1) and (B2) as they were, just correcting some few typographic errors and adding only some obviously missing words, in square brackets, and the numbering of paragraphs, in order to make easier references to the different parts. The footnotes to (B1) are mine. They aim to explicate the many allusions and results implicitly referred to in this 30-minute talk, which was required to be not an expository speech, "for the benefit of persons ill acquainted with the field, but . . . [an address] to those actively interested in the field" (letter from Church to Tarski quoted above).

§3. Comments on (B1).

3.1. Choice of the decision problem. Let us begin by giving a summary description of the structure of (B1). The twenty three paragraphs may be grouped in the following way:

⟨1–4⟩ Introduction.
⟨5–7⟩ Decidability, recursiveness, and various notions of definability.
⟨8–10⟩ Generalities on the decision problem.
⟨11–14⟩ The decision problem for various logics.
⟨15–17⟩ Undecidable problems in number theory.
⟨18–19⟩ Undecidable problems in analysis and set theory.
⟨20–22⟩ The decision problem for algebraic systems and the notion of elementary equivalence.
⟨23⟩ A final word: relations between mathematics and logic.

This scheme roughly coincides with the plan given in the summary (A). However, there is one single dissimilarity: the Point 10 of the summary (A) mentions Gödel's and Gentzen's work on consistency and the notion of finitary proof, two themes totally missing in (B1). One reason for this discrepancy may be the lack of time for dealing with the last point listed in (A) before concluding.

I shall not comment one after the other on all the problems listed in (B1): my footnotes in the present publication of (B1) may afford some information on the historical context or the content of the solutions given to those problems, at least in case of either positively or negatively solved problems. I shall rather try to focus here on some points which seem to me significant with regard to Tarski's work *before* and *after* 1946.

(B1) has some of the flavor of Hilbert's famous address at the International Congress of Mathematicians which took place in Paris in 1900, since it aims to survey unsolved problems in the domain of logic. But it deals with one single theme, "the decision problem", albeit in its many aspects or relations to different mathematical disciplines. According to [Hilbert and Ackermann, 1928], the general decision problem for first order-logic was "the most important problem of symbolic logic" and, thus, was "the *raison d'être* of metamathematics", as Tarski comments in (B1).[7] The negative answer to the *general* decision problem was given in 1936 by Church,[8] who proved that there is no decision procedure that can determine for any sentence of first-order logic whether or not it is provable. Church found indeed a finitely axiomatizable undecidable fragment of arithmetic; it follows that pure first-order logic in the language of the fragment is undecidable. But some *local* decision methods were known for certain classes of sentences within a logical or a mathematical theory. For instance, Löwenheim gave a decision method for the monadic first-order predicate calculus with equality in 1915. Langford showed the decidability of the elementary theory of dense linear order in 1927. But it must be noted that using here the expression "decision method" follows Tarski's retrospective account — as it appeared in print in his Introduction to [Tarski, 1948] — rather than that of Löwenheim or Langford themselves.[9] By 1946 Tarski and his students had assembled some good results on "the decision problem", as listed in Paragraph ⟨21⟩ of the Princeton talk, and some important questions related to this problem were still unsolved, e.g., Hilbert's tenth problem. That might explain Tarski's choice of decidability/undecidability for the Princeton address which should survey "some outstanding problems in the field [of logic] ... and possibly [give] suggestions towards an attack on [them]", as requested by the organizers of the Conference and notified by Church to Tarski (letter dated June 20, 1946, quoted above).

In his introduction (⟨4⟩), Tarski justified his choice historically (referring to Hilbert), materially (asserting in a Hilbertian spirit that "the task of logic is to mechanize thinking"), and heuristically (diverse questions being possibly "couched in terms of ... this problem"). This latter justification may give a hint about the failure to distinguish explicitly between unsolvable decision problems for classes of sentences and undecidable, i.e., formally independent, individual propositions. This failure may be puzzling, especially in ⟨15–19⟩. But, the aim to gather "many diverse questions" under the single label "the decision problem" may have led Tarski to disregard this distinction. Thus Tarski's statement at the end of ⟨3⟩ that he is taking "the decision problem" as his main topic may be misleading, if one understands "the decision problem" as it is currently understood and does not take into account the whole context and the fact that Tarski tried to have the most general view possible and to give "a survey" of diverse outstanding problems.

In *some sense*, decidability and undecidability results were one of Tarski's main concerns since the years 1926–1928. More exactly, in a first period whose end is to be set *grosso modo* around 1940, Tarski was chiefly interested in deductive completeness problems. But he used for solving those problems the method of effective elimination of quantifiers (EEQ),[10] which affords also a positive solution to the decidability of the theory under examination. For instance, in the introduction to the first version of [Tarski, 1948], which was submitted for publication in 1939 under the title *The completeness of elementary algebra and geometry*, and published for the first time in 1967 ([Tarski, 1939/1967]), Tarski stressed the following:

> "It should be emphasized that the proofs sketched below have (like all proofs of completeness hitherto published) an «effective» character in the following sense: it is not merely shown that every statement of a given theory is, so to speak, in principle provable or disprovable, but at the same time a procedure is given which permits every such statement actually to be proved or disproved by the means of proof of the theory. By the aid of such a proof not only the problem of completeness but also the *decision problem* is solved for the given system in a positive sense. In other words, our results show that it is possible to construct a machine which would provide the solution of every problem in elementary algebra and geometry (to the extent described above)."

One can say that, at a first stage of his career and given the method used (namely EEQ), completeness and decidability were often inseparable in Tarski's mind and work. The quoted passage shows also that the link between decision procedures and mechanization of thought was made too. In the *revised text* of [Tarski, 1931] we find a similar remark after the statement of Theorem 2: " ... by analyzing the proof of this result, we see that there is a mechanical method which enables us to decide in each particular case whether a given sentence (of order 1) is provable or disprovable" ([Tarski, 1956, p. 134]). In both passages Tarski did not say any more than the bare facts. Since recursion theory was not yet established at that date, it is hard to see what else he could have done to pursue the idea. With the benefit of hindsight, one can say that an algorithm for computing a recursive function may be too "complex" and, therefore, could not be actually implemented on a machine in a "reasonable" length of time. That means that not every effective decision procedure is feasible. In particular, according to a result proved by M. J. Fischer and M. O. Rabin in 1974, the complexity of the elementary theory of the field of real numbers is at least exponential and therefore implementing the decision procedure for that theory is (at least for the present) out of the reach of human beings.

I would like to cite some further historical information as evidence of Tarski's early interest in completeness *and* decidability. But let me point out, first, that his interest in undecidability results came later, at any rate after Church's Thesis and Turing's analysis of computability in 1936. And the final shift from completeness to decidability came explicitly to light with the modified version of [Tarski, 1939/1967], prepared by McKinsey as a Rand Corporation report and published in 1948 under the new title *A decision method for elementary algebra and geometry* ([Tarski, 1948][11]). In the foreword of [Tarski, 1939/1967], dated November 1966, Tarski wrote: "A comparison of the titles of the two monographs reveals that the center of scientific interest has been shifted to the decision problem from that of completeness." The shift happened when Tarski became aware that "the precise instrument for treating the decision problem is Gödel-Herbrand notion of a general recursive function" ((B1),⟨5⟩), i.e., I assume, already in the late 1930s. (I shall try to confirm this impression below.)

3.1.1. *Some historical facts.*

(a) In the years 1926–1928, Tarski conducted a seminar at Warsaw University on the method of effective quantifier elimination (EEQ).[12] This method was used sketchily by Löwenheim in 1915 and in a developed form by Skolem in 1919 and by Langford in 1927. Skolem had in this way obtained a decision method for monadic second-order logic. Langford showed the completeness of the elementary theory of dense linear orders without first or last elements and also of the elementary theory of discrete orders with a first but no last element (for a closer analysis see [Doner and Hodges, 1988]). As stated by Robert Vaught in [Vaught, 1974, pp. 159–160], Tarski's work on EEQ led to various extensions: for instance, Langford's result for the theory of discrete orders with a first but no last element was extended to the whole class of discrete orders. This work led also to new results, such as Presburger's completeness result for the elementary theory of $(\mathbf{N}, +, =)$ in [Presburger, 1930] and Tarski's famous completeness proof and decision method for elementary algebra — that is for $(\mathbf{R}, +, \cdot, =, \geq)$ — and geometry. The latter result was already announced in Tarski's paper on definable sets of real numbers [Tarski, 1931]; its original proof was given in [Tarski, 1939/1967]. As mentioned above, Tarski emphasized there that his proof actually gives a decision procedure and in his Footnote 11 he argued that "it is possible to defend the standpoint that in all cases in which a theory is tested with respect to its completeness the essence of the problem is not in the mere proof of completeness but in giving a decision procedure (or in the demonstration that it is impossible to give such a procedure)".[13]

(b) Tarski attended the Königsberg meeting in 1930, in which Gödel spoke on his incompleteness result for the first time. Since then, his attention very likely remained focused on decidability/undecidability problems, even if he was led on his own part, and independently, to decision procedures through the method of effective quantifier elimination he used since 1926–1928. For instance, Tarski was developing his concept of an essentially undecidable theory in the late 1930s, which was to be defined precisely in [Tarski, Mostowski, and Robinson, 1953]. Indeed, in the preface of the latter, Tarski wrote: "The work contains results obtained over a long period of time, 1938–1952." And in the first paper written by Tarski alone, Footnote 1, p. 3, stresses that "The observations contained in this paper were made in 1938–1939; they were presented by the author to a meeting of the Association for Symbolic Logic in 1948, and were summarized in [Tarski, 1949a]".

(c) In the fall of 1939, Tarski used the opportunity of an invitation from Columbia University, to talk on the decision problem and its relation to Gödel's incompleteness result. Leon Henkin, who was then an undergraduate student at Columbia, listened to this lecture. Moreover, Tarski published in 1939 a paper "On undecidable statements in enlarged systems of logic and the concept of truth" ([Tarski, 1939]), giving "an example of a result obtained by a fruitful combination of the method of constructing undecidable statements (due to Gödel) with the results obtained in the theory of truth".

(d) Tarski obtained a permanent position at Berkeley in 1942. In the summer of 1943, he gave a seminar on Gödel's results there, attended, among others, by Julia Robinson.[14] Tarski continued to deal with the decision problem applied to various special theories, either himself or through his students, especially in relation to algebraic systems. In the schedule of courses and seminars of the Department of Mathematics at Berkeley,[15] from 1943–44 through 1947–48, Tarski was listed as holding a seminar largely based on unpublished work dealing with "Topics in algebra and metamathematics". Besides his own work, Tarski referred there to results obtained by some of his students, such as the undecidability of the elementary theory of the field of rational numbers proved by Julia Robinson in her dissertation at Berkeley (1947) and the decidability of the elementary theory of Abelian groups proved by Wanda Szmielew in the 1940s. Tarski very likely suggested the decision problem for Abelian groups to Szmielew before leaving Poland in 1939 (see Note 19 below); this fact and Footnote 1 to the first paper of [Tarski, Mostowski, and Robinson, 1953] lead to the conclusion that the shift from completeness to decidability was beginning *before 1940*, even though it came to a crucial point only in the beginning of 1948, when the Rand Corporation offered to publish Tarski's results on the decision method for elementary algebra and geometry [Tarski, 1948]. In his Preface to the 1951 reprint of the Rand Corporation report, Tarski makes clear that he has brought to the fore "the possibility of constructing an actual decision machine" in response to the interests of the Rand Corporation.

3.1.2. *Unpublished results in 1946.* The antepenultimate paragraph of (B1) — no 21 according to my numbering — mentions some results, still unpublished in 1946; the introduction and Footnotes 2 and 3 of [Tarski, 1948][16] allow us to attribute and partly to date these results. Tarski proved that there is neither a decision procedure for the elementary theory of groups nor for the elementary theory of lattices. According to Footnote 1, page 77 of [Tarski, Mostowski, and Robinson, 1953], Tarski stated the undecidability for the elementary theory of groups in his Princeton talk, which likely means that he had found the result before this talk and stated it there for the first time (for a bigger audience than at his seminar at Berkeley). Later on, the result appeared in print in an abstract of *The Journal of Symbolic Logic* [Tarski, 1949b]. As for the undecidability of the elementary theory of lattices, it certainly had been found before the Princeton talk, but the first publication mentioning it is an abstract published in 1949 [Tarski, 1949c] and referred to in [Tarski, Mostowski, and Robinson, 1953, p. 33, l. -4 and Footnote 24]. Other interesting examples of undecidable elementary theories are given in [Tarski, Mostowski, and Robinson, 1953, pp. 33 and 67–71 (Corollary 13)]. Among other *positive* results, Tarski found a decision method for the elementary theory of Boolean algebras in 1940. In collaboration with Andrzej Mostowski, he developed the outline of a proof of the decidability of the elementary theory of well-orderings in the late 1930s;[17] the proof itself came in 1941.[18] As noted above, his student Wanda Szmielew proved the decidability of the elementary theory of Abelian groups,[19] and McKinsey did likewise for the class of true universal sentences of elementary lattice theory in 1943.

3.2. Unsolved cases of the decision problem. In fact, dealing precisely with decidability and especially with undecidability requires the Herbrand-Gödel concept of general recursive functions — our recursive functions —, as Tarski stressed in the beginning of ⟨5⟩ in (B1). Indeed, a theory T is decidable if the characteristic function of the set Σ of theorems of T is recursive, undecidable if the characteristic function of Σ is not recursive. Tarski recalls that in the last sentence of (B1), ⟨8⟩. In [Tarski, Mostowski, and Robinson, 1953, p. 14] Tarski will state the following definition, which is now completely standard: "A theory T is *decidable* if the set of all its valid sentences is recursive, and otherwise *undecidable*."

3.2.1. *Hilbert's tenth problem.* Among several unsolved problems the first to be mentioned in (B1) is Hilbert's tenth problem: the problem of deciding for an arbitrary Diophantine equation whether or not it has a solution in integers. Tarski mentions this problem twice in (B1), first in ⟨4⟩, then in ⟨16⟩, where the following is stated about that problem: "instead of single statements perhaps simple classes of theorems [statements] can be shown undecidable". Actually, it was one of the most famous problems Tarski spoke about to his colleagues and students. Julia Robinson wrote her Ph.D.

thesis giving a negative solution of the decision problem for the elementary theory of rational numbers in 1947. The problem was suggested to her by Tarski, through her husband, Raphael M. Robinson, who was a professor at Berkeley. (Raphael M. Robinson spent the academic year 1946–1947 at Princeton, attended the Bicentennial Conference with Julia and was in close contact with Tarski at Berkeley.) Following Tarski's guidelines, Julia Robinson showed the undecidability of the elementary theory of rational numbers and, more generally, that of fields, by showing the definability of integers within the field of rationals [Robinson, 1949a], [Robinson, 1949b]. This is a method to which Tarski would give a general and formal setting in [Tarski, Mostowski, and Robinson, 1953]. In 1948, Julia Robinson began to struggle with Hilbert's tenth problem itself. The first decisive step towards the — negative — solution was to be made only in 1961 in a joint paper by Julia Robinson, Martin Davis, and Hilary Putnam [Davis, Putnam, and Robinson, 1961]. In January 1970, stimulated by a new paper of Julia Robinson [Robinson, 1969], Yuri Matijasevich proved that there is no algorithm that can determine of a given Diophantine equation whether it has a solution in natural numbers.[20]

3.2.2. *Formally independent propositions involving integers.* Another task (B1) proposed to mathematicians has a different nature, although it focuses too on the elementary theory of arithmetic: constructing an undecidable proposition involving integers which one could come across "in the course of any ordinary mathematical work" (⟨15⟩). First of all, let us stress that up to now "undecidable" was used, in Tarski's Princeton talk, with reference to theories with an unsolvable decision problem; the task proposed now implies a different meaning of the word, referring to individual statements that are neither provable nor refutable in some given formalism, i.e., to *independent* statements of the given formalism. If there is one statement F of some formalism Φ so that F is neither provable nor refutable in Φ, then Φ is incomplete. The formalism Φ is "essentially incomplete if it is impossible to add a finite or infinite recursive set of axioms which completes it" (⟨9⟩). It must be noted here that recursivity enabled Tarski to define the notion of essential incompleteness. Such a definition was already sketched in [Tarski, 1939/1967, Footnote 37] referring to Gödel's monograph of 1931 and to [Rosser, 1936].

Now, the elementary theory of arithmetic is essentially incomplete, but Gödel's independent statement is not exactly of the kind pleasing to "the general mathematician". If one could reach Tarski's proposed goal of constructing an "ordinary"[21] independent arithmetical proposition, then "ordinary" mathematicians could no longer believe that logicians construct undecidable propositions merely for the sake of their undecidability. This is a recurrent theme of Tarski's, in that he always kept in mind daily mathematical practice, and made a great many efforts to give his reasoning and his results a mathematical form rather than a metamathematical one. Despite what is conceded on the differentiation between logic and mathematics at the beginning of (B1), ⟨2⟩, Tarski was very eager to destroy the border Hilbert had put up between mathematics and metamathematics[22] and wanted to afford evidence that mathematical logic could help solve ordinary mathematical problems. It was in this spirit that Tarski first published the paper on definable sets of real numbers [Tarski, 1931] containing results of interest to "the general mathematician".[23] And only later he published his paper on the *decision method* for real algebra, that led to the results of the earlier paper. However this method, based on effective elimination of quantifiers (EEQ) for the elementary theory of the real numbers, is a generalization of a mathematical theorem, Sturm's algorithm for counting the real roots of polynomial equations.[24] Thus, it is difficult to separate the (logical) method and its (mathematical) content and consequences, and we can agree with Henkin[25] who thinks that Tarski "considered his most important contribution to *mathematics* up to 1946", to have been *this* decision method for real algebra. In accord with the last sentences of (B1), in the account of the final session of the Princeton Conference, the reporter has noted the "insistent pressing [of mathematical logic] on toward the problems of the general mathematician". Certainly, this "insistent pressing" was Tarski's hallmark more than that of any other logician present at the Princeton logic session.[26] As is known, Paris and Harrington [Paris and Harrington, 1977] have constructed a first-order arithmetic proposition, more "natural" than Gödel's proposition, which they proved neither provable nor refutable in Peano Arithmetic (PA). However, it still seems today that propositions like this do not occur often in mathematical practice.

3.2.3. *Absolutely unsolvable arithmetic propositions.* A third question asked by (B1) about arithmetic propositions was whether there are arithmetic propositions which are absolutely undecidable (⟨17⟩, Question 2) or, let us better say, absolutely unsolvable, the search being for propositions that "are neither provable nor disprovable in any member of a hierarchy of systems constructed on the original systems in which the [given] proposition is undecidable". In his letter to Gödel dated December 10, 1946 (published in [Woleński and Köhler, 1999, pp. 271–272]), Tarski says:

> "As regards the question in which you are interested [absolute provability, definability, etc.], I don't think that I can do anything else [in the Princeton Bicentennial talk] but to emphasize the fundamental difference between all the undecidable statements known at present in elementary number theory on the one hand and some undecidable statements (like continuum hypothesis) in analysis and set theory; the statements of the first kind being clearly undecidable in a relative sense while those of the second seem to be undecidable in some absolute sense. And in this connection I shall raise the problems

> (1) whether and how the notions of relative and absolute undecidability can be made precise and (2) whether, on the basis of some adequate definition of these notions, it will be possible to show that a number-theoretical problem can be undecidable only in a relative sense. Perhaps I shall make some additional remarks emphasizing the difficulty of these problems; and, of course, since now I know what you are planning to discuss,[27] I shall refer to your subsequent remarks."

The answer to the Question 2 is positive in some cases, negative in others, depending not only on the nature of the extension principle adopted but also on which paths through the Church-Kleene constructive ordinals are allowed.

3.3. Undecidable problems in analysis and set theory. The largest part of (B1) is devoted to the decision problems for number-theoretical propositions, even if account is taken of their geometric interpretation and the link with recursive functions is stressed. Indeed, it is only with Paragraph ⟨18⟩ that Tarski turns to questions in other mathematical domains. The first problem proposed in ⟨18⟩ is whether the elementary theory $\mathbf{R}_{\text{exp}}$ of the real numbers with addition, multiplication, and exponentiation is decidable. First, let us note that Tarski puts the question of the decidability of $\mathbf{R}_{\text{exp}}$ in *analysis and set theory*, whereas the completeness and decidability of the elementary theory of the ordered field $(\mathbf{R}, 0, 1, +, \cdot, =, \geq)$ was proved within a framework excluding "the sophisticated machinery of set theory" and the general concept of whole number. The method of proof was Sturm's theorem whose mathematical content is purely algebraic as Tarski stressed (see [Tarski, 1939/1967, pp. 315, 332]). The "elementary algebra" — as Tarski wrote[28] — of $\mathbf{R}$ and the elementary theory of complete ordered fields as well are decidable. The answer to the question asked here for $\mathbf{R}_{\text{exp}}$ remains open today, but important progress has been made. First, making essential use of results from analysis, Alex Wilkie proved that $\mathbf{R}_{\text{exp}}$ does not admit elimination of quantifiers but that it is model complete, i.e., that for every formula of $\mathbf{R}_{\text{exp}}$ one can find an equivalent existential formula.[29] Then, in [Macintyre and Wilkie, 1996] it is proved that this theory is decidable under a strong hypothesis, namely that Schanuel's conjecture for the real numbers is true — a conjecture that is generally thought to be plausible but out of reach at present.

The second outstanding problem briefly mentioned in ⟨18⟩ is the well-known continuum hypothesis (CH). Gödel proved in 1937 the relative consistency of CH with Zermelo-Fraenkel set theory with the axiom of choice (ZFC). The passage of his letter to Gödel, quoted above in 3.2.3, gives evidence that Tarski conjectured the independence of CH from ZFC. As it is well-known, Paul Cohen confirmed this conjecture in 1963. In ⟨19⟩ Tarski conjectured also the "absolute unsolvability" of Suslin's problem and this conjecture was confirmed too twenty-five years later in a joint work by Robert Solovay and S. Tennenbaum [Solovay and Tennenbaum, 1971]. Suslin's problem was the following: Is an order complete, order dense linearly ordered set without first or last element and in which every family of disjoint open intervals is countable necessarily isomorphic to the real line? Assuming the consistency of ZFC, this problem was proved independent of ZFC.

3.4. The notion of elementary equivalence. As stated by [Vaught, 1974], Tarski's work on EEQ was going in another direction than finding decision procedures. It led indeed to the general study of the notions of elementary equivalence, elementary classes, and elementary extensions. From the beginning to the end of his career, Tarski was deeply interested in these notions and especially in finding the complete elementary extensions of a theory. The notion of elementary equivalence — or, equivalently "arithmetical equivalence", which is the expression used in (B1) — appeared in print for the first time in the appendix to [Tarski, 1936]. There, Tarski classified dense orders up to elementary equivalence and stated that only four such elementary nonequivalent order types exist. He concluded that "it seems that a new, wide realm of investigation is here opened up. It is perhaps of interest that these investigations can be carried out within the framework of mathematics itself (e.g., set theory) and that the concepts and methods of metamathematics are essentially superfluous." In the conclusion of (B1) further study of the notion of arithmetical equivalence is suggested, and the construction of a theory of elementary equivalence of algebras "as deep as the notions of isomorphism, etc. now in use" is proposed (⟨22⟩). Maybe the warmest tribute paid to this suggestion is Nathan Jacobson's treatise on *Basic Algebra* (published in the 1970s), in which a large space is devoted to model-theoretic algebra, i.e., essentially to the application of the logical relation of elementary equivalence to algebraic structures.

It is, of course, a corollary to Löwenheim's theorem (see [Löwenheim, 1915]) that two nonisomorphic structures can be elementarily equivalent, i.e., indistinguishable from the point of view of first-order logic. For example, the structure consisting of just any countably infinite *set* (without any relations, operations, or distinguished individuals) is a model of the (complete) elementary theory of the structure consisting of just the *set* of the real numbers and is thus elementarily equivalent (but not of course isomorphic) to that structure. Tarski found (and stated in [Tarski, 1939/1967, comments on Corollary 2.13, pp. 324–325] a deeper and more interesting example of nonisomorphic, elementary equivalent structures, namely the *field* of real numbers and the (countable) *field* of real agebraic numbers. In the appendix to [Tarski, 1936], Tarski defined the notions of "arithmetical class" and "elementary equivalence". In [Tarski, 1952] he gave an outline of the general theory of arithmetical classes. A few years later he elaborated, in collaboration with Robert Vaught, the fundamental notion of elementary extension [Tarski and Vaught, 1957].

Conclusion. I have started these comments with a rough comparison with Hilbert's 1900 address. I must conclude now by stressing some differences between Hilbert's address and Tarski's one. First, Tarski develops very briefly his views on what is a problem and how to define mathematical logic (Paragraphs $\langle 1 \rangle$ and $\langle 2 \rangle$). In contrast, Hilbert gives a much more substantive account of the nature and the growth of mathematics, of the significance of some problems for various mathematical domains, of the necessity of (logical) rigor in mathematical proofs (understood as finite procedures), of the simplicity introduced by more general axiomatic foundations, of the fruitful interplay between problems or methods arising from different areas of mathematics which, then, show profound analogies with each other, etc. Secondly, I have to recall that Tarski's talk is not a paper written and revised by its author; hence some formulations are not as precise as they could and should have been.

Nevertheless, as it is, the transcript of Tarski's talk (B1), along with the summary (A) and the transcript of the general discussion (B2), is instructive, because it seems to have been done in the middle of Tarski's work on undecidability. Decidability/undecidability was indeed a chief theme for a long period whose crucial point seems to have been around 1946–1948 and which was crowned by the booklet *Undecidable Theories* published in 1953. Actually, it is natural to think that (B1) is certainly the remaining record of an intermediate stage of Tarski's work on elaborating the notion of essential undecidability. Let us note that, in fact, (B1) used and defined "essential incompleteness" and not "essential undecidability", but naturally the latter notion is present through Rosser's result on the essential undecidability of PA which is referred to. According to Tarski's preface of [Tarski, Mostowski, and Robinson, 1953], his two contributed papers to this book cover work during the period 1938–1952, the results of the first paper being summarized in [Tarski, 1949a] and referred to in [Robinson, 1949b].[30]

The document (B1) is also interesting because it explicitly proposes the task of constructing a theory of elementary equivalence, which certainly was the pivot of the by-then already flourishing but still unnamed model theory. According to Footnote 18 of [Tarski, 1952], Tarski mentioned in his Princeton talk some applications of the theory of elementarily equivalent classes of algebraic systems. Tarski wrote in his Footnote 18 that he had, in 1946, explicitly pointed out the general method of proving the existence of algebraic structures with some properties prescribed in advance, for instance the existence of non-Archimedean ordered fields. It must be noted that (B1) kept no record of that mention.

Finally, (B1) alludes to some epistemological views on mathematics and logic. Everybody who knew Tarski knows that he always tried to move attention to metamathematical problems that he could solve without invoking any philosophical position. That does not mean that he had no views at all on how to understand "doing mathematics or logic" or "solving a problem". Let us close by some remarks on this issue.

First, Tarski stresses in Paragraph ⟨1⟩ of his talk that he will not consider that solving a problem affords a clue as to the question of *being* such-and-such for some mathematical *object*. More pragmatically, by solving a problem he means the *task* of *constructing* "something with such-and-such properties" or, more generally, constructing a mathematical theory which makes clear such-and-such *properties* of some mathematical entity (object or process). Tarski's "constructionism" — if I may use a neologism — is asserted here from a pragmatic point of view and not for philosophical or epistemological reasons, "pragmatic" being understood by reference to (mathematical vs. logical) practice. It is in such a pragmatic way that Tarski used the term "logic" as denoting *by definition* the work of people who regard themselves as logicians or are considered logicians by mathematicians. Thus, Paragraphs ⟨1⟩ and ⟨2⟩ of this talk confirm Tarski's pragmatic point of view, referring to none of the three great trends in philosophy of mathematics and logic dominant at the beginning of our century: logicism of Russell, formalism of Hilbert's program, and intuitionism of Brouwer and being quite different from Gödel's "platonism". Tarski once characterized his attitude towards the foundations of mathematics as an "intuitionistic formalism", referring to Lesniewski. It was in 1930, in the last paragraph of the introduction to his *Fundamentale Begriffe der Methodologie der deduktiven Wissenschaften*.[31] But when the paper was translated by J. H. Woodger into English in 1956, Tarski added a footnote saying that this characterization no longer reflected his present attittude. However, he did not characterize his new attitude positively in this footnote. Later, in a lecture given in 1966 and edited twenty years later by John Corcoran [Tarski, 1986b, p. 145], addressing the question "What are logical notions?", Tarski stressed how much his approach stood away from attempts to "catch the proper, true meaning of a notion, something independent of actual usage, and independent of any normative proposals, something like the platonic idea behind the notion". He added that, for his part, he would make "a suggestion or proposal about a possible use of the term 'logical notion' ". Although Tarski did not himself use the term "pragmatism", this term seems to me the most appropriate in the case of what is said in the quoted passage and in (B1) as well. Actually, this may be in conflict with the belief Tarski expressed another time, when he said that "he, perhaps in a «future incarnation», would be able to accept a sort of moderate platonism" (quoted in [Woleński, 1995, p. 336], with reference to the typescript of Tarski's contribution to a meeting, in 1965, on the significance of Gödel's theorem). But this quotation itself is in sharp conflict with a passage quoted by Solomon Feferman in one of his recently published papers ([Feferman, 1999, p. 61]). This passage is an excerpt from "taped and transcribed (but unpublished) extemporaneous remarks from an Association of Symbolic Logic symposium in 1965" (the same year as for the typescript referred to by Woleński). In this excerpt Tarski described himself as an «extreme anti-Platonist» and says "However, I represent this very crude, naive kind of anti-Platonism, one thing I could describe as materialism, or nominalism with some materialistic taint". This description does not contradict the pragmatistic attitude of (B1).

Secondly and as a consequence of his pragmatistic attitude, Tarski stressed here, as in some other writings, the practical (mathematical) *efficiency* of logic and expressed the hope that "the general mathematician" (⟨15⟩) will "appreciate the work of logicians more than they do at present" (⟨23⟩). Thanks to a great deal of effort expanded not only by Tarski himself but also by many other outstanding logicians, the prestige of logicians among mathematicians has much increased since 1946. Nevertheless, the hope that such a prestige would keep on increasing is still justified.

NOTES

[1]See the Program printed on pages 30 and 31 of the Pamphlet, reprinted in [Duren, 1989, pp. 330–331], and below pp. 38–39. On page 39 below are also the names of the members of the Conference Committee, among them E. Artin, S. Bochner, C. Chevalley, A. Church, S. Lefschetz, A. W. Tucker, J. W. Tukey, E. P. Wigner, etc.

[2]The Tarski Archives in the Bancroft Library at the University of California, at Berkeley, contain four letters from McKinsey to Tarski dated February 10, April 19, May 5, and May 18, 1947. I am indebted to Leon Henkin for providing me with copies; his attention was called to them by Francisco Rodriguez-Consuegra.

[3]See [Woleński and Köhler, 1999, pp. 271–272].

[4]It was reprinted with revisions in 1968 in Klibansky's *Contemporary Philosophy*, then in 1990 (as Gödel 1946) in [Gödel, 1986–, Volume II], with an illuminating introductory note by Charles Parsons (op. cit. pp. 144–153).

[5]See also [Givant, 1991].

[6]My emphasis.

[7]Let me recall that in 1953 Tarski still described the decision problem as "one of the central problems of contemporary metamathematics" ([Tarski, Mostowski, and Robinson, 1953, p. 3]).

[8]See [Church, 1936b].

[9]As noted by [Doner and Hodges, 1988, p. 29], Tarski was one of the first logicians to refer in print to the idea of a decision method for determining the consequences of a nonlogical (i.e., *mathematical*) system of axioms and that as early as 1931; see [Tarski, 1931, p. 542]. For other similar references at the same time, see [Presburger, 1930, Paragraph 1] and [Skolem, 1930, Paragraph 3]. Note that Tarski's papers reprinted in [Tarski, 1986a] are and will be cited according to the reprints.

[10]A theory T admits EEQ if there exists a mechanical procedure which, given any formula F of the language of T, produces a quantifier-free formula G which is logically equivalent to F in the theory T.

[11]The 2nd revised edition in 1951 was published by the University of California Press.

[12]See [Tarski, 1939/1967, Footnotes 6 and 21, pp. 335 and 338]. See also [Tarski, 1948, Footnotes 4 and 11, pp. 351 and 354]: "In Tarski's university lectures for the years 1926–1928 this method was developed in a general and systematic way."

[13]See [Tarski, 1939/1967, p. 336, Footnote 11]. Later on, Tarski gave emphasis again to the fact that using EEQ for solving a completeness problem actually gives a decision method.

[14]See [Reid, 1996, p. 47].

[15]I wish to thank Silvan Schweber for obtaining this listing and sending it to me in January 1998.

[16]Pp. 305–306 and 351.

[17][Doner and Hodges, 1988, p. 21].

[18]See [Mostowski and Tarski, 1949a, l. -1].

[19]According to Robert Vaught, Wanda Szmielew may have started to work with Tarski before he left Poland in 1939. Szmielew's result on the decidability of the elementary theory of Abelian groups was established in 1950 — in her doctoral dissertation, University of California. However, Solomon Feferman remembers that Szmielew had already worked out the decision procedure for Abelian groups by the method of EEQ prior to her arrival in Berkeley, in 1949, for writing her Ph.D. under Tarski's supervision. We can even put a lower bound on the date of Szmielew's work, thanks to a letter from Tarski to Mostowski dated January 8, 1941, that was sent via Heinrich Scholz. In this letter, kept in the Archives of H. Scholz in Münster and mentioned to me by Solomon Feferman, Tarski writes: "What do you think about the decision problem for Abelian groups in general? Is Frau Szmielew near to a definitive solution to this problem? Is it not too hard to her?" Tarski may have suggested the decision problem for Abelian groups to "Frau Szmielew" before leaving Poland. "Frau Szmielew" announced her result at the 10th International Congress of Philosophy in Amsterdam in 1948; her paper in the Proceedings, published in 1949, did not give full proofs (see [Szmielew, 1949]). In 1949 she published also an Abstract in the *Bulletin of the American Mathematical Society* 55, p. 65, and the same year Tarski mentions in [Tarski, 1949b, p. 77] that Szmielew *has* shown the decidability of Abelian groups. (B1) gives evidence that this result was found *before December 1946*. Finally, the solution with full proofs was published in [Szmielew, 1955].

[20]For more details see [Davis, Matijasevich, and Robinson, 1976]; see also Davis M., The collaboration in the United States, in [Reid, 1996, pp. 91–97], and Matijasevich Y., My collaboration with Julia Robinson, in [Reid, 1996, pp. 99–116].

[21]"Ordinary" or even "normal" are the two terms used by Tarski to denote the mathematical practice as contrasted with the more specific logical practice. In introducing [Tarski, 1931], Tarski explains that he will reconstruct the metamathematical concept of definability in the "normal" framework of mathematics. In [Tarski, 1952] Tarski asserts the following: "The notion of an arithmetical class is of a metamathematical origin ... However, it has proved to be possible to characterize this notion in purely mathematical terms and to discuss it by means of *normal* mathematical methods" (my emphasis). Thus, Tarski wanted to mathematize or to *normalize* (!) metamathematical results.

[22][Hilbert, 1922, p. 165].

[23]Tarski wrote indeed in his introduction to [Tarski, 1931] the following: "The notion of definability as usually conceived is of a metamathematical nature. I believe, however, that I have found a general method which ... allows us to reconstruct this notion in the domain of mathematics. This method is also applicable to certain other notions of metamathematical nature. The reconstructed concepts do not differ at all from other mathematical notions

and need arouse neither fears nor doubts; their study remains entirely within the domain of normal mathematical reasoning. ... A description of the method in question which is quite general and abstract would involve certain technical difficulties, and, if given at the outset, would lack that clarity I should like it to have. For this reason, I prefer in this article to restrict consideration to a special case, one which is particularly important from the point of view of the questions which interest mathematicians at the present time."

[24]For more details on Sturm's theorem and how Tarski generalized it, see [Sinaceur, 1991]. According to Solomon Feferman, "Tarski used to stress that the heart of a proof of decidability for a theory T must always be something specific to the kind of mathematics that T is concerned with" (quoted by [Doner and Hodges, 1988, p. 24]).

[25]In the late 1970s, J. W. Addison once asked Tarski what he considered to be his most important work, the one of which he was most proud. Tarski said that there were two: his work on truth, and his decision method for elementary algebra and geometry (Addison told that to Henkin, thanks to whom I know it). On the other hand, Hodges remembers that C. C. Chang told him that Tarski thought that he would be remembered in a hundred years' time for the work on finite algebras which he did with Bjarni Jónsson.

[26]However, Tarski was not the only one who stressed how the concepts and methods of mathematical logic could be fruitful within the domain of "ordinary" mathematics. See, for instance, Post's introduction to his paper [Post, 1944].

[27]See [Gödel, 1986–, vol. II, pp. 144–153].

[28]See my comments in [Sinaceur, 1991].

[29]I refer here to Wilkie's talk at the XIXth International Congress of History of Science held at Zaragoza (Spain) in August 1993. A revised version of this paper appeared in [Wilkie, 1996].

[30]This is the paper submitted by J. Robinson as her Doctoral Dissertation in 1947; it was received by *The Journal of Symbolic Logic* in September 1948.

[31][Tarski, 1930b, p. 349].

(A)
A summary of the address to be given by Alfred Tarski at the conference on the problems of mathematics in Princeton, December 17, 1946.[1]

1. The discussion of some outstanding problems in mathematical logic as the main task of the talk. Some explanations regarding the way in which the terms "problem" and "mathematical logic" are used. Among problems discussed are both definite questions of the form "Is it so and so?" as well as problems of a more general and less determined nature, which could be more properly characterized as tasks. The term "mathematical logic" is used rather in a pragmatic sense intented to cover the work actually done by mathematical logicians. In consequence, most of the problems discussed belong to the domain of metalogic and metamathematics, and not to that of logic in a stricter sense.

2. The impossibility of discussing all important problems of mathematical logic in the present talk. The decision to disregard problems whose main importance seems to lie in their implications for such domains as epistemology or the methodology of empirical sciences. The choice of the decision

problem, with its various ramifications and connections, as the focal point of the address. A justification of this choice from historical, material, and heuristic points of view.

3. The notion of general recursiveness as a precise instrument for the study of the decision problem; its intrinsic significance. The importance of a more penetrating study of this notion. The task of constructing a "mathematical" theory of general recursive sets of integers (and general resursive relations between integers). Some suggestions for attacking this problem. Analogies between known facts about general recursive sets of integers on the one hand, and Borelian and projective sets of real numbers on the other. A classification of sets of integers analogous to that of projective sets; the problem of the place of general recursive sets in this classification.

4. The formulation of the logical decision problem in terms of general recursiveness: Is the class S of all provable statements in a given formalized system general recursive? The connection between this problem and that of completeness of a given formalized system; the notion of the essential incompleteness of a system. Decision problem as applied to subclasses of S or to other classes of formulas. The question of intuitive adequacy of the notion of general recursiveness for the formulation of the decision problem.

5. The decision problems applying to elementary parts of logic — sentential calculus and predicate calculus. Examples of some problems in this domain which remain open; is there a procedure to decide whether a given (finite) system of formulas of sentential calculus constitutes an adequate axiom system for this calculus?

6. The non-classical systems of mathematical logic and their intuitive connection with the decision problem. "Many valued" systems of sentential calculus; specific decision problems regarding such systems. The problem of extending non-classical systems of logic beyond sentential calculus.

7. Decision problem in number theory. The insufficiency of the results so far obtained from the viewpoint of mathematicians. The question of solvability of individual number-theoretical problems. Open decision problems for special important classes of number-theoretical theorems;[2] is the class of solvable Diophantine equations with constant coefficients general recursive? The "relative" character of insolvability of number-theoretical problems; the question of the existence of problems which are insolvable in an "absolute" sense.

8. Decision problems in analysis and general set theory. Emphasis on the application of the problem to individual statements (in view of the negative character of general results). Examples of unsolved set-theoretical problems where help on the part of mathematical logic can reasonably be expected. Problems of the foundations of set theory connected with the decision problem.

9. Decision problems for various abstract algebraic systems. A short survey of known results and open decision problems. Problem of the actual construction of "decision machines" for these systems for which the decision problem has been affirmatively solved. Methods of attacking open decision problems. Problems and notions which result from the study of the decision problem for algebraic systems. The notions of an arithmetical property (an elementary definable property) of an algebraic system and of the arithmetical equivalence of two such systems; related problems.

10. Consistency problem and its connection with the decision problem. The results of Gödel and Gentzen and their intuitive value; the question of a precise characterization of the notion of a finitary proof. Possible extensions of Gentzen's result.

11. Concluding remarks on the present stage of logical research and its importance for mathematics.

(B1)
TALK BY PROF. TARSKI AT THE LOGIC CONFERENCE, DECEMBER 1946, PRINCETON.[3]

⟨1⟩ The title and general character of the Princeton mathematical bicentennial conference suggests that the session devoted to mathematical logic should address itself to some outstanding problem or problems in mathematical logic. Now the word "problem" has two distinct senses: in one sense, a problem is a definite question like "Is such-and-such the case[?]"; in another sense, we mean by a problem something of a less determined nature — which could perhaps more properly be characterized as a task — such as "Construct something with such-and-such properties". It is in this second — more general, if you prefer — sense that I shall call the attention of this assembly to some important unsolved problems in mathematical logic.

⟨2⟩ I give the term "logic" here no precise definition. I use the word pragmatically to denote the work of people who regard themselves as logicians — or those who are considered logicians by mathematicians generally. We agree to differentiate between logic and mathematics on the one hand and metalogic and metamathematics on the other. And we immediately note in connection with our pragmatic definition of logic that the chief concern of logicians today is with metalogic and metamathematics rather than with specific deductive principles.[4]

⟨3⟩ Now it would of course be impossible to speak of all the current problems of mathematical logic in the present talk. Therefore we are going to omit problems, such as those concerning confirmation and modality, whose main importance seems to lie in their implications for such domains as epistemology or the methodology of the empirical sciences. Furthermore, limitation of time determines that the conference shall concern itself with one important problem: I have taken the decision problem.

⟨4⟩ The choice of this important topic can be justified on several grounds. From a historical point of view we note that Hilbert considered the main task of logic to be the construction of a symbolism for use in solving the general decision problem. This was the *raison d'être* of metamathematics. In 1900 in his Paris address Hilbert formulated the problem of deciding for an arbitrary Diophantine equation whether it has a solution in integers — a problem which still remains unsolved.[5] Materially, we can justify the choice of the decision problem on the grounds that the task of logic is to mechanize thinking.[6] Heuristically, the choice can be justified by the fact that many diverse questions can be couched in terms of, and brought to a focus by, this problem.

⟨5⟩ Now it appears that the precise instrument for treating the decision problem is the Gödel-Herbrand notion of a general recursive function. The notion of general recursive function, as well as of general recursive set,[7] is a worthwhile idea in itself and, apart from its purely logical applications, certainly deserves to be studied more closely from a strictly mathematical point of view. Without defining "general recursive set" precisely, we may say that a set is general recursive if one can always determine in a finite number of steps whether an arbitrary given element belongs to it. Such sets were considered by Kronecker.[8]

⟨6⟩ With regard to the task of constructing a "mathematical" theory of general recursive sets of integers and general recursive relations between integers we may compare general recursive sets of integers on the one hand and the Borelian and projective sets of real numbers studied by Luzin on the other. There are analogies between these two theories, in methods as well [as] in results.[9] We note that performing certain operations on general recursive sets, such as taking [finite] unions, [finite] intersections, or complements, will yield new recursive sets. This is not true, however, of all operations which one may perform on general recursive sets — a simple example being given by the operation of "projection", which corresponds to the logical operation of quantification.[10] The situation becomes geometrically clear if we regard a general recursive set of integers as represented by a set in n-dimensional space. If the propositional function $f(x_1, x_2, \dots, x_n)$ defines such a recursive set, the propositional function $(\mathrm{E}x_1) f(x_1, x_2, \dots, x_n)$ defines merely a recursively enumerable set, the complement of which need not be recursively enumerable, and which may be regarded as the projection of the original set onto an $(n-1)$-dimensional subspace. We are confronted with a similar situation in the case of Borelian sets.[11]

⟨7⟩ We find other interesting problems arising out of this analogy. We know, for instance, that for any two disjoint analytic sets there exist disjoint Borelian sets containing the given sets. We should like to know if, similarly, one can be certain there exist disjoint recursive sets containing any given pair of disjoint recursively enumerable sets.[12] Again, in connection with the comparison of Borelian sets with recursive sets,[13] we note that Borelian sets are classified by rank as they are built up from simple sets by complementation and kindred operations. In the classification of recursive sets what should be taken as simple sets of integers? For this second problem I have a definite suggestion as to a process that should be attempted. It seems advisable to me to start with sets of integers satisfying certain Diophantine equations: $f(x_1, x_2, \ldots, x_n) = 0$.[14] It seems obvious that several operations on this class will yield general recursive sets. It further seems plausible that all general recursive sets can be obtained in this way.[15] Moreover I believe, this way of working with recursive sets is the procedure best adapted to the solution of special problems. One possibility of procedure would be to construct an abstract theory of projective sets to include Luzin-theory and the theory of general recursive sets as two cases.[16]

⟨8⟩ We now apply the notion of general recursive sets to a formulation of the logical decision problem. In an intuitive way the solution of the decision problem means determining whether there exists a mechanical means of deciding whether any given statement of a formal system is a theorem. More precisely it means determining whether the set of provable statements of a formal system is general recursive.

⟨9⟩ The term "formal system" as I am using it here is, of course, defined in the ordinary way: we take for granted a list of symbols, a selection of certain sequences of symbols from the set of all finite sequences to be called "formulas", the selection of a subset of the formulas called "axioms", and of rules whereby formulas can be deduced from the axioms, such formulas being called "theorems".[17] The term "completeness", as I shall use it, will also have the usual definition. With negation either a primitive or defined symbol of the formal system, by completeness we mean simply that, given any formula [without free variables], either that formula or its negation is a theorem. Since the theorems of any formal system are recursively enumerable we note that any system which is complete is recursive — but of course not conversely.[18] With respect to any incomplete system the question immediately arises whether we can add axioms to complete the system. We shall call a system "essentially incomplete" if it is impossible to add a finite or infinite recursive set of axioms which completes it. Gödel's work shows the existence of essentially incomplete systems.

⟨10⟩ I should mention the fact that some logicians have felt that the notion of general recursiveness may not in actuality be adequate to handle the intuitive content of the decision problem. While I do not deny these men the right to doubt such adequacy I feel that they have the obligation to offer a substitute for general recursiveness if they expect to halt the work along lines based on the notion of general recursiveness. But regarding such adequacy of general recursiveness more will be said by another speaker[19] later this evening, and therefore I shall refrain from further remarks on the subject.

⟨11⟩ Now let us examine the decision problem in some elementary forms of logic. First, the sentential calculus: for this there is the positive result based on the two-valued truth-table method. I do not know who actually is the author of this procedure — whether it was Frege or Peirce — but what is important is that we do have this now classical result.[20] For the monadic functional calculus it is well known that the result is positive.[21] It is negative, however, for the general case of the predicate calculus.[22]

⟨12⟩ With respect to these elementary systems there are still many unsolved problems. There is, for example, the question of finding a procedure to tell whether a given set of formulas is adequate as a set of axioms for the sentential calculus. This is a decision question of a new sort, which does not concern itself merely with the formal theorems of a system.[23]

⟨13⟩ Historically the decision problem has had a direct bearing on the origin of many-valued systems of logic. At one time it seems that logicians in general felt that the solution of the decision problem for the classical two-valued logic was too difficult to attack directly and that the problem should be attempted piecemeal, that is by first solving the decision problem for various subsystems of the classical calculus. It was in this way that the multi-valued systems were created:[24] for they are in most cases just that — subsystems of the classical calculus — though this remark does not apply to the Lewis systems.[25] Some of the decision problems associated with these systems have been solved.[26]

⟨14⟩ In passing from this topic — and I hope that no creators of many-valued logics are present, so that I may speak freely — I should say that the only one of these systems for which there is any hope of survival is that of Birkhoff and von Neumann.[27] This system will survive because it does fulfil a real need.[28] Here we note that the decision problem is still open.[29]

⟨15⟩ Let us now consider the decision problem in relation to number theory. Number theory is incomplete — and indeed, essentially incomplete. I wish to say a word, however, about the type of undecidable propositions which have been constructed by logicians in number theory. None of these problems is exactly pleasing to the general mathematician; it would be a great stride forward in placating the mathematician if we could actually construct such an undecidable proposition involving integers. After all, the mathematician believes that these propositions have been constructed for the sake of their undecidability, and he has great difficulty convincing himself that he will ever address himself to such a sentence in the course of any ordinary mathematical work. For proving a "simple" arithmetic proposition undecidable, the methods used heretofore certainly seem inadequate. It would also prove a great advance — and one which again I might say would greatly increase the prestige of logicians among mathematicians — if we could prove certain questions not general recursive. And as Dr. Gödel has given me permission to refer to his unpublished works[30] I may say that we are extremely close to the culmination of such an enterprise.

⟨16⟩ As another plan of attack I wish to suggest that instead of single statements perhaps simple classes of theorems[31] can be shown undecidable — such [a] procedure being suggested by the problem which Hilbert advanced in 1900 on Diophantine equations.

⟨17⟩ Perhaps the question of number-theoretic undecidable propositions will be better understood if we appreciate the relative character of this unsolvability. With respect to the known examples of unsolvable propositions the proof of unsolvability gives an intuitive proof of the truth (or the falsity) of the theorem, and leads to a natural extension of the original system in which the theorem becomes decidable. But we are faced here with two very important problems: (1) how can we make precise this notion of natural extension;[32] and (2) are there propositions of a number-theoretic nature which are undecidable in an absolute sense, that is, are neither provable nor disprovable in any member of a hierarchy of systems constructed on the original systems in which the proposition is undecidable?[33]

⟨18⟩ Questions regarding the decision problem for analysis and set theory are far more complicated than the corresponding questions for number theory, since the former contain arithmetic as a subdomain. Of course it is well known that the general decision problem for analysis and set theory has been answered in the negative.[34] But important problems [which are] not yet worked out exist for special classes of statements; for example, to decide whether a given statement is provable from or independent of certain axioms. The class of statements about the domain of real numbers which can be stated using only addition and multiplication has a general decision procedure; the existence of such a general decision procedure is equivalent to the solution of the decision problem for (elementary) analytic geometry.[35] As soon as we generalize slightly, however, and ask ourselves what is the situation when we allow also the operation of exponentiation, for instance, little or nothing is known.[36] And lastly, as a source of a wealth of labor for future logicians interested in decision-problem questions for analysis, we should mention the results of Gödel on the continuum hypothesis — too well-known to require elaboration in the brief time we have at our disposal.

⟨19⟩ In discussing the relations between set theory itself and logic I shall begin by saying that I believe that the set-theorist may expect much from the formal logician. I believe that certain problems of set theory may actually be independent of the axioms of set theory and may be shown to be so independent by formal logical means. Suslin's problem on ordered sets is a good example of a problem which mathematicians have not been able to solve either positively or negatively, and for which this situation of independence of the axioms of set theory probably holds.[37] I should remark here that with regard to set theory the question of a definition of "natural extension" again arises.[38] Here too, such a definition must be agreed upon in order to define the notion of absolute unsolvability.

⟨20⟩ Although the question of the decision problem for algebraic systems is exceedingly interesting to me, because of lack of time, I shall confine myself to a few — perhaps superficial — observations. The reason why the decision problem for algebraic systems is so interesting is precisely this: within any algebraic system the sets of statements for which we may expect positive results on the decision problem have intrinsic algebraic significance.

⟨21⟩ In any modern algebraic treatment of groups, rings, etc. we can distinguish two complementary classes of statements. The first class deals with the elements and fundamental operations of some particular algebra; this class we shall call the arithmetic statements[39] of the algebra. The second class, requiring much of the sophisticated machinery of set theory, deals with relations between algebras, etc. Now it may be possible to solve the decision problem for the first of these classes. After all, we do already possess most important results: we know that the decision problem for general elementary groups has been answered in the negative[40] and for elementary Abelian groups in the positive;[41] moreover we know that for general lattices the answer is negative,[42] but that it is positive for certain special lattices (for example, for Boolean algebras[43]).

⟨22⟩ In closing I should like to suggest further study of arithmetical properties and arithmetical equivalence of algebras. (These words themselves, of course, need precise definition.[44]) In particular I should like to offer the challenge of two problems: (1) In how many ways can arithmetic axioms for Abelian group theory be added to make a complete system?[45] (2) Algebras exist which are not isomorphic, but which cannot be distinguished by their arithmetic properties; it would be desirable to construct a theory of arithmetic equivalence of algebras as deep as the notions of isomorphism, etc. now in use.[46]

⟨23⟩ A final word: I might say that one of my aims in this talk was to show the relations between logic and mathematics. I consider logic a branch of mathematics, although I freely admit that the part of logic which is mathematics does not perhaps exhaust logic. Certainly logicians do owe mathematicians a great debt of gratitude for the wealth of problems they [the mathematicians] have offered. Conversely, however, it is hoped that under the plan I have suggested mathematicians generally will appreciate the work of logicians more than they do at present.

NOTES

[1]This summary was prepared by Tarski before the Conference (cf. letter from Kleene to Church of November 29, 1946, quoted in my introduction §1, p. 4).

[2]Tarski evidently means "statements" (and not "theorems").

[3]As I made clear above (my introduction, §1 and §2, pp. 4–6), the text of this talk was not written down by Tarski. It was based on notes of L. Henkin and S. C. Kleene, or on a tape-recording. The title was therefore not given by Tarski.

[4]Tarski used the Hilbertian word "metamathematics" in a new sense. As he stated in his *Grundzüge des Systemenkalküls* ([Tarski, 1935, p. 27]), the task of metamathematics is "to define the meaning of general metamathematical concepts which appear in the discussion of the most diverse deductive theories and to establish the basic properties of these concepts". And quoting from [Tarski, 1930a]: "Formalized deductive disciplines form the field of research of metamathematics roughly in the same sense in which spatial entities form the field of research in geometry" (p. 313). Examples of general metamathematical concepts include consequence, model, satisfiability, completeness, decidability, etc. Let us recall that Tarski's papers reprinted in [Tarski, 1986a] are and will be cited according to the reprints.

[5]Hilbert's tenth problem has been negatively solved in 1970 by Yuri Matijasevich, using earlier work of Martin Davis, Hilary Putnam, and Julia Robinson; more is said about the history of the solution in my introduction, 3.2.1.

[6]Should we understand that Tarski had in mind something like Leibniz's "*calculus ratiocinator*"? In paragraph ⟨8⟩ below of his talk, Tarski says more explicitly: "The solution of the decision problem means determining whether there exists a *mechanical means* [my emphasis] of deciding whether any given statement of a formal system is a theorem." This is just the way of speaking when one takes Hilbert's view on formalization and on what one should be looking for by constructing formal systems.

[7]The class of (general) recursive functions (from some finite power of the set $\mathbf{N}$ of natural numbers into $\mathbf{N}$) is the smallest set which contains all the constant functions, the successor and the projections, and which is closed under composition, a form of simple definition by induction (primitive recursion), and minimalization (see, for example, [Moschovakis, 1980, pp. 5–6]). A set is recursive if its characteristic function is recursive.

[8]Tarski is probably alluding to Kronecker's famous paper *Über den Zahlbegriff* ([Kronecker, 1887]).

[9]In the early 1930s Tarski and Kuratowski had emphazised analogies between Luzin's projective sets and Tarski's definable sets of real numbers. In particular, performing the operation of projection on definable sets of real numbers yields definable sets of real numbers. Cf. [Tarski, 1931, pp. 547–548 and 551–559]. The theory of projective sets was created (according to [Sierpiński, 1950]) by N. Luzin in 1924 in Moscow (see [Luzin, 1925]). A subset of a finite Cartesian product of $\mathbf{R}$ is *Borelian*, or *Borel*, iff it is in the least class containing all open sets and closed under the operations of complementation and countable union. A set is *projective* iff it is in the least class containing all Borel sets and closed under the operations of complementation and projection.

[10]More specifically, existential quantification, as Tarski makes clear by the example he gives immediately after.

[11]The projection of a Borel set is not necessarily Borel. Suslin introduced the analytic sets in 1917. He showed that there were analytic non-Borel sets and proved that a set is *analytic* iff it is the projection of a Borel set. See [Suslin, 1917].

[12]A negative answer to this question was given in [Kleene, 1950], where two disjoint recursively enumerable sets of natural numbers were found which could not be separated by any recursive set. The theory of recursive-function-theoretic hierarchies of sets of natural numbers had had its origins in [Kleene, 1943], where what is known today as the *arithmetical hierarchy* was introduced. Subsequently, but independently, Mostowski, who was Tarski's student in the 1930s, developed a version of the arithmetical hierarchy, basing his work at least partially on analogies with the hierarchy of projective sets. He makes at least six references to these analogies in his formulation of the theory, which appears in [Mostowski,

1947]. But after learning of the result of [Kleene, 1950] Mostowski pointed out to Kleene that the inseparability theorem of [Kleene, 1950] created a major breakdown in the analogy.

At Kleene's suggestion, in the spring of 1952 his student John Addison investigated this breakdown and discovered that a far closer analogy existed between the arithmetical hierarchy and the finite Borel hierarchy. One of the keys to understanding and perfecting the analogy was to consider the finite Borel hierarchy on the Baire space $\mathbf{N}^{\mathbf{N}}$ (which is isomorphic to the set of functions from $\mathbf{N}$ to $\mathbf{N}$ under the product topology) rather than on the set of real numbers. Using this analogy Kleene's inseparability theorem corresponded exactly to the inseparability theorem for the open subsets of the Baire space. Using the new analogy Addison defined, in his dissertation [Addison, 1954], an "effective Borel hierarchy" on both $\mathbf{N}$ and on $\mathbf{N}^{\mathbf{N}}$ and found that a unified treatment could be given for these two hierarchies and the classical Borel hierarchy on $\mathbf{N}^{\mathbf{N}}$. Moreover, it turned out that this effective Borel hierarchy on $\mathbf{N}$ was substantially the same as Kleene's extension of his arithmetical hierarchy known as the *hyperarithmetical hierarchy* (versions of which were also produced independently by Davis and by Mostowski).

Addison isolated as "the fundamental principle of the analogies" (see also [Addison, 1959]) the fact that a function f from $\mathbf{N}^{\mathbf{N}}$ into $\mathbf{N}^{\mathbf{N}}$ is continuous iff there exists an element α of $\mathbf{N}^{\mathbf{N}}$ such that f is recursive in α. He also defined an "effective Luzin hierarchy" on $\mathbf{N}$ and on $\mathbf{N}^{\mathbf{N}}$ and gave a unified treatment of these with the classical projective hierarchy. These effective hierarchies were equivalent to the "function-quantifier-hierarchies" of second-order number theory which Kleene had been investigating independently.

To emphasize and clarify the close analogies between the classical Borel and projective hierarchies and their "effective" counterparts Addison coined the term "effective descriptive set theory" and introduced the uniform Σ^i_n, Π^i_n and $\boldsymbol{\Sigma}^i_n$, $\boldsymbol{\Pi}^i_n$ notation for them (see [Addison, 1959]). Here the superscript i represents the order of the defining logical formulas, the subscript n the number of alternating blocks of existential and universal quantifiers, and the boldface represents that the defining formulas can include constant names for any elements of $\mathbf{N}^{\mathbf{N}}$. Thus we have, for example,

$\mathbf{N}$	$\mathbf{N}^{\mathbf{N}}$	$\mathbf{N}^{\mathbf{N}}$
recursive ($=\Delta^0_1$)	recursive (effectively clopen $=\Delta^0_1$)	clopen ($=\boldsymbol{\Delta}^0_1$)
recursively enumerable ($=\Sigma^0_1$)	effectively open ($=\Sigma^0_1$)	open ($=\boldsymbol{\Sigma}^0_1$)
arithmetical ($=\cup\{\Sigma^0_n : n\in\omega\}$)	arithmetical ($=\cup\{\Sigma^0_n : n\in\omega\}$)	finite Borel ($=\cup\{\boldsymbol{\Sigma}^0_n : n\in\omega\}$)
hyperarithmetical ($=\cup\{\Sigma^0_\alpha : \alpha\in\omega_1^{CK}\}$)	effectively Borel ($=\cup\{\Sigma^0_\alpha : \alpha\in\omega_1^{CK}\}$)	Borel ($=\cup\{\boldsymbol{\Sigma}^0_\alpha : \alpha\in\omega_1\}$)
Σ^1_1	effectively analytic ($=\Sigma^1_1$)	analytic ($=\boldsymbol{\Sigma}^1_1$)
Σ^1_2	effectively PCA ($=\Sigma^1_2$)	PCA ($=\boldsymbol{\Sigma}^1_2$)
analytical ($=\cup\{\Sigma^1_n : n\in\omega\}$)	analytical ($=\cup\{\Sigma^1_n : n\in\omega\}$)	projective ($=\cup\{\boldsymbol{\Sigma}^1_n : n\in\omega\}$)

(Here, Δ^0_1 is short for $\Sigma^0_1\cap\Pi^0_1$ (and similarly in the boldface case). The symbol ω_1^{CK} names the Church-Kleene first nonconstructive ordinal. The class PCA is the class of *projections* of *complements* of *analytic* sets. Finally, note that the terms "analytic" and "analytical", which arose in different theories, are quite distinct and name nonanalogous classes.)

[13]Tarski may have been led to consider this approach by one known similarity between the Borel sets and the recursive sets. By a famous theorem of Suslin [Suslin, 1917] a set of real numbers is Borel iff both it and its complement are analytic. Analogously a set of natural numbers is recursive iff both it and its complement are recursively enumerable. But, as was

noted in Note 12, a closer analogy exists between the Borel and the the hyperarithmetical sets.

[14]Today, we have the following definitions:

(1) A subset X of $\mathbf{N}^n$ is a *polynomial set* iff its elements are the solutions of some Diophantine equation $f(x_1, x_2, \ldots, x_n) = 0$.
(2) A subset X of $\mathbf{N}^p$ is *Diophantine* iff it is the projection of a polynomial set $Y \subseteq \mathbf{N}^{n+p}$.

Matijasevich proved in 1970 the following theorem: a subset X of $\mathbf{N}^p$ is recursively enumerable iff X is Diophantine. It follows that there is a Diophantine set which is not recursive and hence there can be no effective general procedure for testing Diophantine equations to determine whether they have integer solutions.

[15]It is not clear what operations Tarski may have had in mind here. The class of recursive subsets of finite products of $\mathbf{N}$ is closed, for example, under complementation, finite union, finite intersection and bounded quantification, but closing out the class of polynomial sets under these operations falls far short of exhausting the class of recursive sets. Indeed, no very natural and useful hierarchy of all recursive sets of natural numbers has ever been found. On the other hand there is a natural hierarchy of ω_1^{CK} of all recursive subsets of $\mathbf{N}^{\mathbf{N}}$ which is an effective analog of the so-called Kalmar hierarchy ω_1 of all clopen subsets of $\mathbf{N}^{\mathbf{N}}$. See [Kleene, 1958, Footnote 36] and [Barnes, 1965].

[16]It is not certain what Tarski had in mind here. If he is suggesting (as would appear to be the case in view of the preceding sentences) that a hierarchy of all recursive sets might be found by using an analogy with the projective hierarchy, then the idea has not materialized. But note that computer scientists today consider a potentially interesting (although conceivably degenerate) classification of a proper subclass of the class of recursive sets — the so-called *polynomial time hierarchy* — which has been compared by some with the projective hierarchy. Under this analogy the class NP (of sets of finite sequences of 0's and 1's decidable in polynomial time by a nondeterministic Turing machine) corresponds to the class of analytic ($\mathbf{\Sigma}_1^1$) sets of infinite sequences of 0's and 1's.

[17]Tarski clearly has in mind that a "formal system" must be an effectively given one, so that, for example, the set of axioms is effectively decidable, the rules of inference are effectively calculable, etc.

[18]For example, the elementary theory of identity and the elementary theory of dense linear orders are incomplete but decidable.

[19]The "adequacy of general recursiveness" refers to what is known today as "Church's thesis", the explicit statement of which is in [Church, 1936a] and consists of identifying the formal notion of "general recursiveness" and the vague intuitive notion of "effective calculability". Among the logicians present, Church, Kleene, and Gödel would have been obvious candidates to say something about that.

[20]Emil Post gave a proof of the validity of the truth-table method as a decision method, in [Post, 1921]. The first explicit formulation of the method of truth tables for deciding propositional validity of which we are aware is by Charles Sanders Peirce in [Peirce, 1885, p. 191].

[21]This result may be dated from [Löwenheim, 1915] and [Skolem, 1919] — see, for example, [Tarski, 1948], [Ackermann, 1954] or [Church, 1956, pp. 292–293]. Later but independently, Behmann proved the same result in [Behmann, 1922].

[22]This was first shown in [Church, 1936b]. In [Church, 1936a] Church proposed a formal definition (now, following Kleene, known as "Church's thesis") of the vague intuitive notion of "effectively calculable". In [Church, 1936b] he showed that under this definition the characteristic function of the set of valid sentences of the first-order predicate calculus is not effectively calculable. See also [Turing, 1936] for his alternative definition ("Turing's thesis") of "effectively calculable" and his proof that his class of functions and Church's class are equal.

[23]There is no decision method which, for recursively given sets of propositional tautologies, will determine whether or not all propositional tautologies can be derived from that set. The first published work dealing with this is an abstract written by Emil Post and Samuel Linial in [Linial and Post, 1949].

[24]On February 10, 1947 McKinsey wrote to Tarski: "I'm afraid that the account of your own talk could be much improved in some points. Especially in connection with the discussion of many-valued logics. I don't remember your having said that the many-valued logics originated in the attempt of logicians to solve easier decision problems than the decision problem for classical logic — and, though I'm never strong in history, it doesn't seem to me that this is the case." We do not know how (or even if) Tarski responded to McKinsey's letter. However, it should be noted that Point 6 of the summary (A) (which we definitely know to have been written by Tarski) does confirm that he intended to say something about "non-classical systems of mathematical logic and their intuitive connection with the decision problem". First, let me recall here that Tarski gave a large meaning, in this talk, to the expression "the decision problem" (see my introduction 3.1, p. 7 and 3.2.2, p. 12). In particular in paragraph ⟨15⟩ below, "an undecidable proposition" means a proposition *independent* of a given formalism. Now Łukasiewicz wrote in 1929: "Actually, it is the method of proving the *independence* [my emphasis] of propositions in the theory of deduction which has occasioned our researches into many-valued logics" (quoted by [Bocheński, 1961, p. 405]). Thus, using Tarski's way of speaking in this talk, we may say that the many-valued logics did originate in the attempt to solve a "decision problem".

[25]Tarski is referring here to the modal propositional logics introduced by Clarence I. Lewis. Readers puzzled by Tarski's inclusion of these logics under his discussion of *many-valued* systems should look, for example, at [Lewis and Langford, 1932], where the "method of matrices" (or many-valued truth tables) is employed in the development of the Lewis systems.

[26]Tarski surely had in mind here the deductive systems $\mathbf{L}_n$ discussed in [Łukasiewicz and Tarski, 1930] as well as McKinsey's positive solution of the decision problem for the Lewis systems S2 and S4: [McKinsey, 1941].

[27]See [Birkhoff and von Neumann, 1936].

[28]On this issue (C) is slightly different: "Tarski felt that «The system of von Neumann and Birkhoff seems to me to be the most interesting of these (non-classical logics), and the only one which has any chance to replace our customary two-valued logic, since it is the only one which has arisen from the needs of science»."

[29]At the present time this system is known to be undecidable. See [Roddy, 1989].

[30]Tarski was very likely alluding to a paper on undecidable diophantine propositions, which was first published in [Gödel, 1986–, vol. 3, pp. 156–175] along with an Introduction by Martin Davis. This text was probably prepared between 1938 and 1940 for an undelivered lecture. Gödel's result was reducing the form of definition of primitive recursive sets to those in universal Diophantine form. According to Feferman's remark: "if this is formalized in Peano arithmetic (PA) it implies a «mathematical» independence result from PA". As Davis stresses in his Introduction (p. 161) "It is certainly remarkable that Gödel had found a simple undecidable class of statements about Diophantine equations a decade before anyone else was giving the matter serious thought".

[31]By "theorems" Tarski evidently means "statements". Notice that the word 'theorems' is also misused in the summary (A), Point 7.

[32]According to [Tarski, Mostowski, and Robinson, 1953, p. 11] T_2 is an extension of T_1, if every sentence which is valid in T_1 is also valid in T_2. Now, what is a "natural" extension? One possible answer is: extension by consistency statements, or more generally by reflection principles.

[33]If one admits that reflection principles yield natural extensions, then this question is answered by iterating such extensions effectively into the constructive transfinite, as first

done by [Turing, 1939] and carried out by Feferman in his paper on recursive progressions of theories [Feferman, 1962] — for historical information see [Feferman, 1988]. The answer depends on the nature of the reflection principle chosen and also on which paths through the Church-Kleene constructive ordinals are allowed.

[34][Rosser, 1936] proved that no consistent extension of Peano arithmetic (PA) is decidable, that is that PA is essentially undecidable. The notion of essentially undecidable theories was *formally* introduced by Tarski's abstract with that title in 1949 [Tarski, 1949a] and elaborated in [Tarski, Mostowski, and Robinson, 1953]. PA is known to be interpretable in various systems of set theory; hence (by Theorems 7 and 10), these systems are essentially undecidable. Moreover, the subsystem Q obtained by replacing in PA the induction schema by the single axiom $(\forall x)(x \neq 0 \rightarrow \exists y(x = Sy))$ also is essentially undecidable (see [Mostowski and Tarski, 1949b]). It follows that any consistent system in which Q is relatively interpretable is essentially undecidable. This applies to quite weak systems of analysis and set theory, as was shown by Szmielew and Tarski (see [Tarski, Mostowski, and Robinson, 1953, p. 34]).

[35]This decision procedure was found by Tarski between 1926 and 1930, announced in [Tarski, 1931], proved in [Tarski, 1939/1967], and published for the first time in [Tarski, 1948] — see my introduction, 3.1.1(a).

[36]The decidability of the elementary theory of real numbers using the operations of addition, multiplication, and exponentiation still is an open problem. For information about progress on the problem see my introduction, 3.3.

[37]Tarski's conjecture was confirmed by R. Solovay and S. Tennenbaum in [Solovay and Tennenbaum, 1971] (for the statement of Suslin's problem see my introduction, 3.3). In a passage of his letter to Gödel dated December 10, 1946 (quoted in my introduction, 3.2.3), Tarski made a similar conjecture about the more famous continuum hypothesis.

[38]The question of natural extensions of set theory is a subject of even greater discussion and debate today with some logicians proposing that ZFC (Zermelo-Fraenkel set theory with the axiom of choice) enriched with various so-called "large cardinal" axioms constitute "natural extensions" of set theory. But even allowing such extensions as "natural", both Suslin's hypothesis and the continuum hypothesis still remain candidates for absolutely undecidable statements, that is ones that are independent in all "natural" extensions of ZFC.

[39]Tarski says, equivalently, "arithmetic" or "arithmetical", "elementary", and "first-order". Tarski had already used the notion of "arithmetical classes" in [Tarski, 1936, p. 241]. He would give an explicit definition of this notion in 1950; see [Tarski, 1952]. Here, according to the summary (A), Point 9, an "arithmetical property" is defined as "an elementarily definable property".

[40]By Tarski himself, probably in the year 1946 (see my introduction, 3.1.2). Let us add that the elementary theory of rings, that of commutative rings, and that of ordered rings are undecidable; see [Mostowski and Tarski, 1949b] and [Tarski, Mostowski, and Robinson, 1953, pp. 33, 69–71].

[41]By Tarski's student Wanda Szmielew in the 1940s (see my introduction, Note 19).

[42]As far as we know, Tarski's first published announcement of the undecidability of the elementary theory of lattices came a few years after this talk, in [Tarski, 1949c]. In that abstract Tarski outlines a proof using undecidability results of Julia Robinson in the arithmetic of the rational numbers but he notes that the theorem itself is "an older result originally obtained by an analogous method, but without the help of Mrs. Robinson's results".

[43]By Tarski in 1940.

[44]Tarski used the notion of "arithmetical equivalence" already in the 1930s. The definition is to be found in the appendix to [Tarski, 1936] and in [Tarski, 1952].

[45]The answer to this question is: there are continuum many complete extensions of the elementary theory of Abelian groups (see [Szmielew, 1955]). Let us note that this type of question interested Tarski from his early days; see, for example, [Tarski, 1936, Section 4],

concluding remarks on the ordered pair of cardinal numbers (α, ν), which characterizes a model of the axioms listed in Section 1, α being the power of the class of all complete axiomatizable systems and ν the power of the class of all complete non-axiomatizable systems.

[46]Let us recall Tarski's early result on the elementary equivalence between the field of real numbers and the field of real algebraic numbers (mentionned immediately after Corollary 2.13 in [Tarski, 1939/1967, pp. 324–325]). Carrying out the program proposed here, Tarski published [Tarski, 1952], which is "an outline of the *general theory of arithmetical classes* and a discussion of some of its applications". One question related to that program is, for instance, the algebraic characterization of elementary equivalence. The first characterization was the back-and-forth criterion given by Fraïssé in 1955, and independently by Ehrenfeucht in 1961 and Taimanov in 1962. Other algebraic characterizations were by ultralimits (Kochen in 1961) and by ultrapowers (Keisler in 1964 assuming the generalized continuum hypothesis and Shelah in 1971 without it). Another question may be the preservation of elementary equivalence under algebraic operations such as generalized products. There were early results by Mostowski and Fraïssé, but the definitive result is in [Feferman and Vaught, 1959].

(B2)
Discussion in the conference of logic held at Princeton, December 17, 1946.*

⟨1⟩ After the prepaired talks the chairman, Prof. Church, made several remarks on the subjects mentioned, which led into general discussion.

⟨2⟩ Church began by emphasizing the critical aspects of Rosser's talk with respect to the Birkhoff-von Neumann suggestion that a many-valued logic be used to formulate theoretical quantum mechanics. He stressed the fact that such a proposal cannot be considered seriously unless it includes a "many-valued" formulation of quantification and set theory as well as a changed sentential (propositional) calculus.

⟨3⟩ Gödel raised the question: Is there any difficulty in setting up a formal system in which ordinary logic is retained for mathematical propositions and non-Aristotelian logic applied only to propositions about physical facts? Church aggreed that a procedure might be developed whereby the classical, as well as some many-valued, propositional calculus were incorporated into one system, and the ordinary quantification-theory constructed as an extension of this. However, he stressed that unless such a program is carried through in detail the enterprise must be considered unsatisfactory. These same general criticisms apply to other non-classical systems such as those of Reichenbach, Fevrier, and Strauss.

⟨4⟩ Church next commented on Gödel's talk and examined the frequent suggestion that finitary rules of inference and formation be relaxed, and transfinite rules admitted, in order to escape the classical Gödel incompleteness results (or for other reasons). He emphasized that while such systems

*The document was apparently prepared along with (B1) from the same information (notes or tape recording). In the title, "conference" is evidently an oversight; it should be "session".

might have considerable interest of one kind or another, they could not properly be considered *logics*, insofar as logics explicate the notion of *proof*. For what we mean by a proof is something which carries finality of conviction to any one who admits the assumptions (axioms and rules) on which the proof is based; and this requires that there be an effective (finitary, recursive) syntactical test of the validity of proposed proofs.

⟨5⟩ Church asserted that while a determined sceptic could always insist on doubting, there were "reasonable" grounds for doubting in some cases but not in others. In particular he stated that in case that "proof" was characterized by transfinite conditions an observer might always ask for a proof that a proposed "proof" did indeed satisfy the transfinite conditions; whereas in the case of effective conditions the observer is always able to check directly by a mechanical procedure.

⟨6⟩ Gödel replied by first pointing out that his talk was not concerned with setting up a formal system, but with the probelm of setting up a theory of the whole transfinite series of stronger and stronger formal systems for mathematics.

⟨7⟩ With respect to the question whether a proof is required that a given sequence of sentences is a proof, Gödel said there would be no difference between the scheme he proposed and a finitary characterization of the axioms; but that the difference would rather lie in the fact that in the second (as opposed to the first) case there is always complete agreement, even among mathematicians of the most divergent philosophical views, as to whether some given sequence is a proof in the sense defined. Furthermore, he remarked that in the scheme he proposed (assuming it can be carried out) there would even exist recursive conditions characterizing, not the axioms, but the axioms and their negations, so that if this condition were satisfied for a sentence A, either A or $\neg A$ would be recognizable as true without proof, by a mere consideration of the meaning of the terms occurring.

⟨8⟩ Later Tarksi, commenting on this exchange, said that he agreed with Church's thesis but with Gödel's mode of argumentation. He thought that within the philosophical framework of idealism Gödel's position could be consistently maintained and that a philosophical position of empiricism should be used to buttress Church's contentions. Church denied that his thesis required the support of empiricism or other special philosophy.

⟨9⟩ In commenting on Tarski's discussion of the decision problem Church emphasized the desirability of obtaining unsolvability proofs for various particular problems in the different branches of mathematics. In topology, for example, such a problem as "to find a complete set of invariants for 3-dimensional closed simplicial manifolds" is actually a disguised form of a decision problem, namely to decide for an arbitrary pair of such manifolds (as given, say, by incident matrices) whether they are homeomorphic.

For this, or other problems like it, it may be possible to obtain absolute unsolvability proofs.

⟨10⟩ Church thought that an attempt should be made in some sense to draw the line between mathematical problems which are solvable and those which are not. However he conjectured that the problem, to find an effective method of deciding in all cases whether a given problem is solvable, is not solvable.

⟨11⟩ An example of a specific problem which had been seriously proposed for solution in a methematical context (by Thue, 1914) and then proved unsolvable (by Post, unpublished) was given by Church:

⟨12⟩ Consider a system consisting of a set of symbols $a_1, a_2, \dots, a_n$ and a set of equations $A_1 = A_2, A_3 = A_4, \dots, A_{m-1} = A_m$, where the A_j are "words" —i.e., finite sequences of the a_i. The problem is to find a method for deciding, for an arbitrary such system and an arbitrary pair of its words, whether the words can be transformed into each other by a finite sequence of substitutions employing the given equations as axioms. The word problem of group theory is a special case of the above where $n = 2r$ and all relations $a_i a_{n+i} = 1$ and $a_{n+i} a_i = 1$ (where "1" denotes the null word) are among the given equations. In this case the symbols are taken to be generators and the equations, defining relations for the group. Whether the decision problem for this special case is solvable is not known. Tarksi pointed out that Thue's problem may be regarded as the word problem for semi-groups.

⟨13⟩ The question was asked of Gödel whether it might not be possible to obtain an absolute definition of definability based on the constructive ordinals rather than on all ordinals, and thus avoid some of Church's objections to transfinite methods in "linguistic" operations. Gödel said that assuming classical mathematics to be a meaningful theory (not a mere play with symbols) there cannot exist any mathematical definition of definability allowing only denumerably many definable ordinals (because of the argument of the least undefinable ordinal). He went on to say that in his opinion classical mathematics has meaning (i.e., describes some reality) and that failure to assume this (at least as a working hypothesis) must psychologically work as a hindrance in research.

⟨14⟩ At Dr. F. Mautner's request Tarski expanded his description of the arithmetical properties of algebraic structures. He showed how one can make use of a general theorem of Skolem to establish the existence of certain infinite models.

⟨15⟩ Mr. P. Wachtell asked whether a proof of the unsolvability of Fermat's Theorem would not imply its truth, since its falsity would permit a calculable disproof. Kleene, answering in the affirmative, explained that this was a case in which an unsolvability proof relative to a given system led to an extension of the system by the addition of a statement true in the classical sense. He pointed out that there are examples of statements which are true classically but not realizable, so that a system in which such a statement is not decided may admit different extensions according as classical or intuitionistic truth is taken as the basis.

⟨16⟩ The occurrence in this discussion of the phrase "the strongest formal system for mathematics known at present" prompted Gödel to remark that such a system cannot exist, because, if any system is given, the system obtained by adding the proposition that the given system is consistent is stronger, and the new axiom must be acknowledged as a true proposition if the axioms of the given system are acknowledged as true.

⟨17⟩ This suggested to Tarski the question: what must be added to a system for number theory to get the strength equivalent to that obtained by imbedding the system in a logic of higher type? Mr. L. Henkin stated that any proposition of the extended logic could be expressed in the simpler system providing a predicate $P(x, y)$ was added as a primitive notion, where $P(x, y)$ holds just for pairs of numbers such that x and y are Gödel numbers (in some standard correspondence between numbers and the formulas of the logic of higher type) which denote the same entity.

(C)
Mathematical logic.†

Here the discussion revolved about a single broad topic — decision problems. The notion of a decision method is a formalization of the classical notion of an algorithm. The known equivalence of Turing's "computability", defined in terms of a very general computing machine, Herbrand and Gödel's "general recursiveness", and Church's "λ-definability" has led to general agreement on the natural formalization of the notion of an algorithm. More and more decision problems are being shown to be unsolvable, in the sense that there exists no algorithm with the required properties. While the general mathematician has to regard this as somewhat negative progress, it is real progress, not only for mathematical logic but for other fields of mathematics as well.

The nearest approach to a proof of unsolvability for a problem of importance in other fields is the recent theorem of Post that the word problem in semi-groups is insoluble. Such results have encouraged mathematical logicians to go further and try to show that problems of a more standard mathematical character are unsoluble. In his summary, Church suggested the word problem for groups and the problems of giving a complete set of topological invariants for knots and for closed simplicial manifolds of dimension n as likely possibilities. Theorems are needed to characterize or provide criteria for the distinction between solvable and unsolvable decision

†The following account and the program reprinted below, pp. 38–39, are excerpted from the Pamphlet, *Problems of Mathematics*, printed by the Princeton University in 1947.

problems (though to find a decision method is no doubt itself an insoluble decision problem).

Closely related to the decision problem for theories is the question of whether or not particular questions are decidable in a given theory. It was pointed out that in particular the Reimann Hypothesis might be undecidable in a particular theory, but that the flexible position of the general mathematician would prevent its ever becoming demonstrably undecidable for him.

The analogy between generally recursive sets of integers and Borel sets of real numbers was stressed by Tarski, who pointed out the possibilities of proving analogous theorems and of developing a single theory to include both as special cases. Tarski then surveyed the status of the decision problem in various logical fields: sentential (propositional) calculus, predicate (functional) calculus, many-valued systems of sentential calculus, number theory, analysis, general set theory, and various abstract algebraic systems. In all of these, even in two-valued sentential calculus, where we would like to be able to decide when a set of formulas is an adequate axiom system, he pointed out open, important problems.

No formal system, with the usual restrictions, which is strong enough to deal with the arithmetic of integers can be complete (Gödel 1931), it must contain undecidable propositions. This led Gödel to propose a particular tremendous enlargement of the notion of a formal system — which would allow uncountably many primitive notions and allow the notion of an axiom to depend on the notion of truth. "I do not feel sure that the set of all things of which we can think is denumerable."

This led to a spirited discussion led by Church and Gödel, which centered on the non-mathematical questions of what could reasonably be called a "proof" and when a listener could "reasonably" doubt a proof. From a psychological point of view the discussion resembled the classical debates on intuitionism to a remarkable degree; Church arguing for finiteness and security and Gödel arguing for the ability to obtain results.

Kleene discussed the limitations which general recursiveness may place on quantitative proof. McKinsey discussed the criticisms of general recursiveness as the formalization of the notion of an algorithm.

Quine proposed to evade the undecidability of arithmetic by studying a restrictive arithmetic without quantifiers, which might have a decision method. It was pointed out that, in particular, Fermat's Last Theorem could be expressed in such a system. The corresponding problem for a partial system of real numbers without quantifiers was proposed as an open problem.

The subject of non-classical logic recurred throughout the session, with relatively favorable words for the quantum-mechanical system of Birkhoff and von Neumann. Tarski felt that "The system of von Neumann and Birkhoff seems to me to be the most interesting of these (non-classical logics), and the only one which has any chance to replace our customary two-valued logic, since it is the only one which has arisen from the needs of science". Rosser discussed the problems involved in applying a many-valued logic, for example Reichenbach's, to all the steps which lead up to quantum mechanics, including truth-function theory (propositional calculus), quantification theory (functional calculus), set theory, theory of the positive integers, theory of real numbers, theory of limits and functions, theory of Hilbert space, theory of quantum mechanics. The initial steps have shown a great tendency of the theory to ramify, single notions in the classical theory corresponding to more and more distinct notions as one passes to more and more complex theories. Gödel proposed using the two kinds of logic, each in its place. Church would accept this proposal for consideration, only if a single logistic system were constructed providing syntactical criteria by which the place of each kind of logic is fixed. He felt that all non-classical logics faced great difficulties in such applications.

PROGRAM

PRINCETON UNIVERSITY BICENTENNIAL CONFERENCE

Problems of Mathematics

FIRST DAY—TUESDAY, DECEMBER 17, 1946

Opening of Conference
Chairman: L. P. Eisenhart

Session 1: ALGEBRA
Chairman: E. Artin — Reporter: G. P. Hochschild
Discussion leaders: G. Birkhoff, R. Brauer, N. Jacobson

Session 2: ALGEBRAIC GEOMETRY
Chairman: S. Lefschetz — Reporter: I. S. Cohen
Discussion leaders: W. V. D. Hodge, O. Zariski

Session 3: DIFFERENTIAL GEOMETRY
Chairman: O. Veblen — Reporter: C. B. Allendoerfer
Discussion leaders: V. Hlavatý, T. Y. Thomas

Session 4: MATHEMATICAL LOGIC
Chairman: A. Church — Reporter: J. C. C. McKinsey
Discussion leader: A. Tarski

SECOND DAY—WEDNESDAY, DECEMBER 18, 1946

Session 5: TOPOLOGY
Chairman: A. W. Tucker — Reporter: S. Eilenberg
Discussion leaders: H. Hopf, D. Montgomery, N. E. Steenrod, J. H. C. Whitehead

Session 6: NEW FIELDS
Chairman: J. von Neumann — Reporter: V. Bargmann
Discussion leaders: G. C. Evans, F. D. Murnaghan, J. L. Synge, N. Wiener

Session 7: MATHEMATICAL PROBABILITY
Chairman: S. S. Wilks — Reporter: J. W. Tukey
Discussion leaders: H. Cramér, J. L. Doob, W. Feller

THIRD DAY—THURSDAY, DECEMBER 19, 1946

Session 8: ANALYSIS
Chairman: S. Bochner — Reporter: R. P. Boas
Discussion leaders: L. V. Ahlfors, E. Hille, M. Riesz, A. Zygmund

Session 9: ANALYSIS IN THE LARGE
Chairman: M. Morse — Reporter: M. Shiffman
Discussion leaders: R. Courant, H. Hopf

Final Dinner
Chairman: H. P. Robertson
Speakers: S. MacLane, M. H. Stone, H. Weyl

CONFERENCE COMMITTEE

E. ARTIN, V. BARGMANN, S. BOCHNER, C. CHEVALLEY, A. CHURCH, R. H. FOX, S. LEFSCHETZ (Chairman), H. P. ROBERTSON, A. W. TUCKER, J. W. TUKEY, E. P. WIGNER, S. S. WILKS

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CNRS-PARIS I, IHPST,
13 RUE DU FOUR, 75006 PARIS, FRANCE
E-mail: sinaceur@canoe.ens.fr