Alfred Tarski and a watershed meeting in logic: Cornell, 1957

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For Jan Wolenski, on the occasion of his 60th birthday[2]

In the summer of 1957 at Cornell University the first of a cavalcade of large-scale meetings partially or completely devoted to logic took place--the five-week long Summer Institute for Symbolic Logic. That meeting turned out to be a watershed event in the development of logic: it was unique in bringing together for such an extended period researchers at every level in all parts of the subject, and the synergetic connections established there would thenceforth change the face of mathematical logic both qualitatively and quantitatively.

Prior to the Cornell meeting there had been nothing remotely like it for logicians. Previously, with the growing importance in the twentieth century of their subject both in mathematics and philosophy, it had been natural for many of the broadly representative meetings of mathematicians and of philosophers to include lectures by logicians or even have special sections devoted to logic. Only with the establishment of the Association for Symbolic Logic in 1936 did logicians begin to meet regularly by themselves, but until the 1950s these occasions were usually relatively short in duration, never more than a day or two.

Alfred Tarski was one of the principal organizers of the Cornell institute and of some of the major meetings to follow on its heels. Before the outbreak of World War II, outside of Poland Tarski had primarily been involved in several Unity of Science Congresses, including the first, in Paris in 1935, and the fifth, at Harvard in September, 1939. (It was the latter which brought him to the United States and fortuitously left him stranded there following the Nazi invasion of Poland.) Much attention had been given to logic at these congresses and to Tarski's own work, in particular, through the deep interest in it of Carnap, Quine and others.

Following the end of the war, Tarski forged new alliances, especially in the United States logical and mathematical communities. To begin with, as part of the year-long celebration of the two-hundredth anniversary of the founding of Princeton University, a high-level conference on the Problems of Mathematics was held there in December 1946.

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[1] The material for this article is to be part of a chapter for a biography of Alfred Tarski, under preparation with Anita Burdman Feferman.

[2] It is a pleasure on this occasion to express our thanks to Jan Wolenski both for his personal assistance and for his extensive historical and analytical work on logic and philosophy in the Lwow-Warsaw school, which has been invaluable to our work on the Tarski biography.

Amost one hundred participants attended, many from abroad, though few from the heart of war-ravaged Europe. In the words of the general report on the conference, "it became the first international gathering of mathematicians in a long and terrible decade." Tarski gave the leading address in the session on mathematical logic, followed by shorter contributions and a group discussion by Alonzo Church, Kurt Gödel, Stephen C. Kleene, Willard Van Orman Quine and J. Barkley Rosser.[3] Four years later, Tarski was one of the invited speakers at the first meeting of the International Congress of Mathematicians (ICM) to take place since 1936.[4] There, his fellow invitees for a special session on logic were Kleene, the venerable Thoralf Skolem, and the up-and-coming model-theorist, Abraham Robinson. Tarski's and Robinson's papers there resonated with each other, the former bearing the title "Some notions and methods on the borderline of algebra and meta-mathematics" and the latter, "On the application of symbolic logic to algebra."

Though Tarski was one of the prime movers of the 1957 Cornell conference, in fact the idea for it originated not with him but with the University of Chicago mathematician Paul Halmos. Noted for his work in functional analysis and ergodic theory, as well as for his lively and outgoing personality and vivid expository talents, Halmos had taken a strong interest in algebraic logic in the 1950s. Pursuing that, he was led to develop the subject of polyadic algebra, which stands to first-order predicate logic without equality as Boolean algebra stands to propositional logic. Quite independently, Tarski had for some years been vigrously promoting the development, in collaboration with his colleagues and students at U.C. Berkeley, of the subject of cylindric algebra, by means of which one could algebraicize the first-order predicate logic *with* equality. A strong personal connection between the two men was established when Halmos visited Berkeley during 1953, and that gave him an impetus to continue his own direction of work on algebraic logic when he returned to Chicago. In 1955 on his own initiative he got the ball rolling for what was to become the Cornell conference when he contacted Edwin Hewitt, the chair of the Summer Institutes Committee of the American Mathematical Society (AMS) about the possibility of organizing an institute devoted to logic. In his memoir, *I Want to be a Mathematician*, thirty years later Halmos wrote of this proposal:

> There weren't many conferences, jamborees, colloquia, and workshops in those days, and the few that existed were treasured. The AMS Summer Institutes were especially effective and prestigious, and I decided that it would be nice to have one

[3] See Hourya Sinaceur, "Address at the Princeton University Bicentennial Conference on Problems of Mathematics (December 17-19, 1946), by Alfred Tarski", *The Bulletin of Symbolic Logic 6* (2000), 1-44.
[4] The scheduled 1940 ICM meeting, to which Tarski had first been invited, was cancelled due to the war.

> in logic, especially if it were at least partly algebraic. It was a brash decision. I had no stature as a logician, I had no clout, I wasn't a member of the in-group; all I had was the brass (willingness to stick my neck out) and the drive (willingness to do the spade work).[5]

In his 1955 letter to Hewitt, Halmos had written:

> You may recall that in our conversation ... you mentioned two very reasonable necessary conditions that a subject must satisfy in order to be eligible for consideration by the committee. The first was that the subject be a live one, with something happening in it that would make an extended conference worth while, and the second was that it be not an obvious recipient of support from the many industrial and governmental sources that other sources (such as statistics and partial differential equations) can tap nowadays. The various disciplines usually grouped together under the name of symbolic logic...certainly satisfy both of these conditions.
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> In regard to the non-availability of other support, I think little need be said. Although logic is one of the oldest subjects of mathematical interest and although I am convinced that its continued study is of tremendous mathematical value, the subject is not such as to capture the imagination of an admiral of the navy or a tycoon of industry.[6]

A few weeks after Halmos' letter, Tarski and Leon Henkin also wrote Hewitt in favor of the idea for the Institute; Henkin had joined the faculty of mathematics at U.C. Berkeley in 1953, and from that point on was actively engaged in helping Tarski develop his research programs in algebraic logic and applications of logic to algebra. After pointing to the requisite evidence of the vigorous progress of mathematical logic, they wrote:

> There is one further point which perhaps deserves particular mention. In part because much of the work in logic is published in special journals there are some mathematicians who are not familiar with the many directions in which this field has recently developed. These mathematicians have the feeling that logic is

[5] Paul Halmos, *I Want to be a Mathematician. An Automathography*, New-York: Springer-Verlag (1985), p. 215.

concerned exclusively with those foundation problems which originally gave impetus to the subject; they feel that logic is isolated from the main body of mathematics, perhaps even classify it as principally philosophical in character.

Actually such judgments are quite mistaken. Mathematical logic has evolved quite far, and in many ways, from its original form. There is an increasing tendency for the subject to make contact with the other branches of mathematics, both as to subject and method. In fact we would go so far as to venture a prediction that through logical research there may emerge important unifying principles which will help to give coherence to a mathematics which sometimes seems in danger of becoming infinitely divisible.[7]

The AMS committee approved the proposed institute in the spring of 1956; it was to be sponsored by the American Mathematical Society and to be funded under a grant from the National Science Foundation (NSF). (Curiously, the Association for Symbolic Logic was not invited to join in as a co-sponsor, as it would be for subsequent logic institutes.) A committee consisting of Halmos, Kleene, Quine, Rosser (chair), and Tarski was formed to decide on venue, length of meeting, and, most importantly, topics to be covered and participants to be invited; they set to work immediately.[8] A controlling factor was the budget provided by the NSF; though close to $30,000 in gross, only about $23,000 would be available after fixed costs were deducted. Several locations were suggested, but before long the choice came down to two: Cornell University vs. U.C. Berkeley, with Rosser (who was at Cornell) pushing hard for the former, and Tarski for the latter. One argument made against California--even though there would be considerable representation from logicians in the west and especially in the San Francisco Bay Area--was that most participants would be coming from the eastern part of the U.S. and thus travel expenses would be greater as a whole for the U.C. choice than if the meeting were held at Cornell. The argument on budgetary grounds eventually won the day with a majority of the committee members; this was one of the few times in his post-war career that Tarski did not have his way.

Budgetary constraints also affected the decision as to how many senior and junior logicians could be invited with financial support. Only those from North America, or who

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[6] Letter from Halmos to Hewitt dated 13 September 1955 in the Tarski archives of the Bancroft Library, University of California, Berkeley. The letter is quoted at greater length in Joseph W. Dauben, *Abraham Robinson*, Princeton: Princeton University Press (1995), pp. 232-233.

[7] Letter from Tarski and Henkin to Hewitt dated 26 September 1955 in the Tarski archives. The letter is quoted at greater length in Dauben (*ibid.*), p. 233.

[8] The complete committee correspondence is to be found in folder 13.5 of the Tarski archives.

happened to be visiting the U.S., were to be asked, and--after the obvious senior choices, including the committee members and, of course, such luminaries as Gödel and Church--there was much controversy about the remaining choices.. Naturally, each committee member promoted as his own candidates those colleagues and students whose work they knew best, either directly or indirectly through research in their specialties. A number of those under consideration were recent Ph.D.s, and few could evaluate their accomplishments, so it was hard to make comparative rankings. Arguments about whose protegés should be put in first place kept the committee struggling almost to the end; it took them through December 1956 to arrive at the final list of some thirty-plus invitees.

Besides those offered support, the institute was also advertised widely in the logic community, and people were invited to attend on their own funds with the prospect that they could even be considered for participation in the program. Though this was a more or less open invitation, just who was allowed to come was a delicate matter, since the committee was concerned to exclude those Rosser labeled “crackpots”. In the end, eighty-five people attended; noteworthy among these was a group of twenty from the nascent computer industry, most of whom were sent by the IBM Corporation.

There were a number of spouses and even children in addition to the participants. Dormitory rooms were made available at $10 a week for singles and $16 a week for doubles, and dining was provided in the dormitories. Some families rented houses together. Cornell University is situated in upstate New York near the city of Ithaca, on a promontory overlooking Cayuga Lake, the longest of the Finger Lakes. The region is marked by numerous streams that have cut deep ravines and gorges, with waterfalls at every turn; the campus itself is bounded by two gorges, and affords many walks along trails leading to falls or fine views. Longer excursions could be made to Buttermilk Falls State Park to the south. Incidentally, at the turn of the century Ithaca was a center for the incipient movie industry, and still to be found on Cayuga Heights are mansions built by stars of the day. So, there was a feeling of vacation about the meeting, rather like being at an intellectual spa.

Of the eighty-five participants at the Institute, fifty-four gave presentations, many of them giving two or even three, some jointly. Spread over five weeks, the talks could proceed at a rate of four a day, leaving ample time for questions and informal discussion. The proceedings of the Institute were distributed in 1957 only as summaries of the talks, reproduced from typed manuscripts; the bibliographic information for that volume and (categorized) list of speakers is to be found in the Appendix. On the academic side, the list of speakers shows how widely representative an assemblage this was through the presence of so many stars, rising stars, and up-and-coming younger workers who would soon stand out in the field. Moreover, the gathering was very representative in terms of subject matter being dealt with at the leading edge of research in mathematical logic; besides talks falling squarely under the standard subdivisions into model theory, recursion theory, set theory, proof theory and constructivity and their applications, there were lectures on algebraic logic, many-valued logics, automata and logical aspects of computation. In particular, as a sign of the emerging times, Alonzo Church gave a series of lectures, "Application of recursive arithmetic to the problem of circuit synthesis". [9]

Though Tarski had been thwarted by not having the meeting take place in Berkeley on his home turf, he nevertheless made it abundantly clear that he was to be considered the number one man of the occasion. There was no direct challenge to this, since the reclusive Kurt Gödel--whose name had stood first on the invitation list--did not attend. Moreover, Tarski exerted power through the large constituency that he succeeded in having invited: among the speakers were his colleagues Leon Henkin and Raphael Robinson, and his students Jean Butler, Chen-Chung Chang, Solomon Feferman, Richard Montague, Dana Scott and Robert Vaught.[10] His close friend from the Netherlands, Evert Beth (who happened to be visiting Johns Hopkins University in 1957), was another speaker. Others attending the institute connected with Tarski were his former students Bjarni Jónsson and Julia Robinson. There was an almost palpable atmosphere of competition for status and visibility with other groups, such as those of Church, Kleene and Rosser with their students, and with rising stars such as Georg Kreisel and Abraham Robinson, both of whom were of a younger generation than the fifty-six year old Tarski. At the time, Robinson was thirty-nine and Kreisel thirty-three. Both, like Tarski, were emigrés from pre-war Europe, but via quite different paths.[11] [12] Each had worked on applied mathematics during the war and, perhaps as a result, their styles were much more experimental and make-do than Tarski's.

Robinson would soon be recognized as a leader in the applications of model theory to algebra, and would make his mark internationally in 1965 with the creation of non-

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[9] Incidentally, this was the only text reproduced in full in the summaries of talks.

[10] Though identified as Tarski's students, neither Butler nor Scott ended up obtaining their Ph.D. degrees with him; Scott had, by the time of the Cornell meeting, completed his doctoral work under the direction of Church in Princeton, while Butler obtained her Ph. D. somewhat later, working with Victor Klee of the University of Washington.

[11] Robinson left Germany for Palestine with his family in the early 30s, came to France to study in 1939, fled to England in 1940, and ended up at the University of Toronto in 1951where he stayed until 1957. See Dauben (*ibid.*) for a full biography of Abraham Robinson.

[12]Kreisel was sent from Austria to England by his parents in the late 30s, studied at Cambridge, and was a Lecturer at the University of Reading for most of the 1950s. See Piergiorgio Odifreddi (ed.), *Kreiseliana: About and Around Georg Kreisel*, Wellesley: A.K. Peters (1996), p. xiii, for Kreisel's vita, as well as for other articles in that volume containing biographical information.

standard analysis. Kreisel devoted his energies to proof theory and constructive mathematics, fields that Tarski had no real feeling for; in that respect, to Tarski's annoyance, Kreisel became something of a guru to a number of younger logicians, including Tarski's students Feferman and Scott and Kleene's student Spector. Fueling the game of one-upmanship, Kreisel made a not so subtle show of the fact that he was one of the rare logicians to be on intimate personal and intellectual terms with Gödel; they had become close during Kreisel's visit to the Institute for Advanced Study in Princeton during the two years leading up to the Cornell meeting. One way he made that relationship clear in the course of the conference was to communicate a previously unpublished result of Gödel's, giving a constructive functional interpretation of the system of intuitionistic arithmetic. This was surprising, since it was generally thought that Gödel had ceased to work actively in mathematical logic after his stunning consistency results concerning the axiom of choice and the continuum hypothesis at the end of the 1930s.[13]

A methodologically interesting four-way connection between Henkin, Kreisel, Robinson and Tarski was brought out at Cornell in one of the reports by Henkin and Kreisel, respectively. This concerned Hilbert's 17th problem from his famous list of twenty-three problems at the Paris meeting of the ICM in 1900. Hilbert had conjectured that every positive semi-definite polynomial with real coefficients could be written as a sum of squares of rational functions. This conjecture was settled affirmatively in 1927 by the algebraist Emil Artin, who introduced for that purpose the notion of real closed field, which generalizes the properties of the real numbers as that of algebraically closed field generalizes the properties of the complex numbers. Tarski's elimination of quantifiers procedure for the first-order theory of real numbers, established a few years later, showed that the complete theory of the field of real numbers is given by the axioms for real closed fields. In 1955, Robinson proved as a consequence of Tarski's work that one could place uniform bounds on the number and degrees of the rational functions used in the represention of a given polynomial as a sum of squares, in terms of the number of variables and degree of that polynomial; however, these bounds were not effective. A year later, Kreisel had shown how, using proof-theoretical ideas, one could extract recursive bounds from Artin's original proof. At Cornell, Henkin and Kreisel both gave talks entitled "Sums of squares", each showing that primitive recursive bounds could be obtained for the representation; Henkin did this by a more careful reworking of Robinson's model-theoretic proof using facts about Tarski's elimination of quantifiers procedure, while Kreisel

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[13] It turned out much later that Gödel had already lectured on the functional interpretation in 1941; his own publication of these results did not take place until 1958. See Solomon Feferman, *In the Light of Logic*, New York: Oxford University Press (1998), Ch. 11, for further information as to its development.

sketched how this could be done by applying proof-theoretical methods to the latter. Few were able to follow Kreisel's argument, both because of the relative unfamiliarity of the methods he used and due to the vagueness of some of the steps involved; it took some thirty years for the details to be worked out in full.[14] By contrast, Henkin's presentation was "clean", to Tarski's satisfaction, and sufficiently detailed to not require further elaboration. Still, the unexpected connection between proof theory and model theory aroused considerable interest.

As it turned out, Tarski was at the Cornell institute for the first three weeks only, since he had an invitation to lecture at the University of Mexico in the latter part of July. But while he was there he was always very much in evidence, almost always being the first to rise during question periods, exercising his usual critical attitude as to sloppy statements of results or proofs, raising points of priority or proper attribution, and even making value judgments about the work in question. Sometimes the criticism seemed unduly harsh or undeserved. For example, during a discussion by Tarski of the notion of rank in the cumulative hierarchy of sets for Zermelo-Fraenkel set theory, when someone brought up Quine's "New Foundations" system. Still not known to be consistent relative to ZF, the Quine system NF has a number of curious properties and does not admit the notion of rank, but it had attracted serious attention from some logicians including--among those present--both Rosser and Wang; nevertheless, Tarski snapped, "I wasn't talking about such futuristic systems."[15] More severely, after a lecture by George Dekker on the recursion-theoretic notion of isols, an interesting recursive analogue of Dedekind's definition of finiteness,[16] Tarski rose to say that he did not find *that* direction of work at all worthwhile. He then went on at length to stress his own early work on Dedekind's and other notions of finiteness which require the axiom of choice for their equivalence, even though that was not directly relevant. Visibly annoyed, the then young logician and philosopher Hilary Putnam rose to say that he thought such critical remarks inappropriate and that they should be reserved for Tarski's autobiography.[17] Putnam was not alone in his distaste for Tarski's autocratic persona.

By contrast, others had a much more positive reaction to Tarski, which accorded with the experience of those who had personally been swept up by his enthusiasm for the subject of logic, his wide range of interests and problems within it to excite and engage

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[14] Cf. Charles N. Delzell, "Kreisel's unwinding of Artin's proof" in Odifreddi (1996), 113-246.

[15] This incident was recently recalled to me by Martin Davis (in an e-mail message, 17 September 2000).

[16] An *isol* is a recursive equivalence type of sets which are either finite or have no recursively enumerable subset.

[17] Interview with Hilary Putnam in Berkeley 25 April 1995. Putnam added that none of Tarski's students came to his defense on that occasion.

them, and who were challenged by his exceptionally high standards. For example, William Tait, then a graduate student at Yale and uncertain of his interest in logic, reported that "most of the more senior people were rather inaccessible to students, or so it seemed to me. The two exceptions, to whom I have always felt grateful, were Paul Halmos ... and Tarski. Both seemed to welcome interaction with students and I spent a number of evenings in their company. ...speaking with them helped me lose my sense of being an alien and gave me confidence about my work."[18]

There is one more aspect of the Cornell Institute, only partly specific to Tarski, that needs to be highlighted, namely the many talks connecting up with the emerging field of computer science. The theoretical foundation of that subject had been laid in the 1930s via the analysis of effective computability according to the independently developed approaches of Herbrand-Gödel, Church, Turing and Post.[19] Its first applications were to proofs of the algorithmic unsolvability of various problems in logic and mathematics. A major branch of the subject that came to be called recursion theory (from the Herbrand-Gödel notion of general recursive functions), was subsequently devoted to the properties of recursively enumerable (r.e.) sets, and within that, after Post (in a famous 1944 article) to questions about degrees of unsolvability of such sets. Richard Friedberg, a then undergraduate at Harvard, created a sensation in 1956 when he solved the main problem concerning such degrees that had been raised by Post, by means of a novel "priority" method that was to become a fundamental technique in the field.[20] Friedberg had learned of Post's problem in a course by Hartley Rogers of M.I.T. After graduation from Harvard, he was to enter medical school and make a career in medicine, and it was thus touch and go whether he would attend the Cornell institute. In the end, not only did he do so, but he gave three talks, two about his remarkable results concerning r.e. sets and one about ideas for designing a learning machine.

The work of Alan Turing, in his theoretical analysis of the potentialities of computing machines, became especially important when large scale electronic digital computers came into existence towards the end of World War II. In 1945 the mathematician John von Neumann made a number of proposals which were crucial for their practical development. In particular, the "von Neumann architecture" for the use of stored programs incorporated Turing's concept of a universal computing machine, which could duplicate the work of any particular (Turing) machine by taking its program as part of

[18] Personal communication (e-mail message of 7 October 2000).
[19] Cf. Robin Gandy, "The confluence of ideas in 1936", in Rolf Herken (ed.), *The Universal Turing Machine: A Half-Century Survey,* Oxford: Oxford University Press (1988), 55-111.
[20] The problem was solved independently by the Russian mathematician A. A. Muchnik.

the input data. Though the first such computers were constructed in academic settings (including the Institute for Advanced Study), commercial development followed soon enough through the work of such companies as IBM and Remington Rand. The context for the interest in actual computation of the attendees at Cornell was a sea-change in the computer industry: the first generation of commercial electronic digital computers which had been brought into existence in the late 1940s was coming to a close. Around 1956-57, both the technology and the software began to change in a significant way, with the introduction of such computers as the IBM 704 and the scientific programming language FORTRAN, which made possible the relatively ready translation of high level algorithms given by formulas, into programs that could be compiled automatically so as to be machine readable.[21]

At last a genuine connection could be made between the kind of high-level theoretical work pursued by the recursion-theorists and that of researchers in the computer industry. The Cornell meeting was the first time a large number of computer scientists came together with logicians. On the academic side, one had, besides the lectures of Church on switching circuits and of Friedberg on a learning machine mentioned above, talks by Rosser on the relation between Turing machines and actual computers, by Rabin and Scott on finite automata, by A. Robinson on theorem proving as done by man and machine, and by Davis on his implementation (on the I.A.S. "johnniac" computer) of Presburger's decision procedure for the arithmetic of the integers under addition. On the industry side, there were fifteen talks given by researchers from IBM (see the Appendix), a number of them demonstrating the utility of FORTRAN-like programs for solving problems of potential interest to logicians. One in particular should have caught Tarski's attention, namely that of George Collins on the implementation on an IBM 704 of parts of Tarski's decision procedure for elementary algebra in order to deal with a variety of problems that could be expressed in that language. Collins had completed a Ph. D. thesis on the relation of the NF system to axiomatic set theory under Rosser's direction at Cornell in 1955, but already before that had begun to think about the decision procedure for algebra. Recently, in response to a question as to Tarski's reaction to his work, Collins reported: "He didn't show any appreciation, either then or later. I was somewhat surprised and disappointed."[22] As it turned out in his subsequent pursuit of these problems, Collins found that Tarski's own procedure was not best suited for actual computational purposes, and in 1973 he developed an alternative procedure called cylindric algebraic decomposition, which through its further improvements is now incorporated in some programs for

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[21] Cf. Paul N. Ceruzzi, *A History of Modern Computing,* Cambridge: MIT Press (1999).

symbolic algebraic computation.[23] This is one way in which Tarski's work had a significant impact on computer science; it is a pity that despite Tarski's own recognition of the importance and systematic pursuit of the decision problem for various algebraic theories beginning with his own work and that of his students (such as Presburger) in Warsaw in the late 1920s, Tarski did not evince the least bit of interest in its practical applications. It must be admitted though that it took a long time for many of the other theoretical logicians attending the Cornell institute (the author included), to recognize the interest and value of relevant research in computer science.

CODA: The cascade of meetings

In general, besides its immediate and sustained excitement, the value for the participants at the Cornell institute on the academic side of mathematical logic lay in establishing contact with many individuals who would figure prominently in the years to come, and in gaining an appreciation of the varied routes that the subject had taken. For the Tarski group, which had specialized in model theory, set theory, and algebraic logic, this meant especially coming abreast of recursion theory in its various guises, as well as proof theory and constructive mathematics. It took several years for these connections to gel in unexpected yet synergistic ways. The qualitative change that would take place with their confluence would only begin to become evident at the Theory of Models conference held in Berkeley on Tarski's home ground in 1963. In the interim, a series of major conferences on logic, or in which logic had a significant representation, began to stack up, one after another. At the end of the summer of 1957, one month after Cornell, a conference on constructivity in mathematics, organized by Arend Heyting, took place in Amsterdam. Then at the end of 1957, Tarski and Henkin organized a conference on the axiomatic method in mathematics and physics at U.C. Berkeley. Two years later, Tarski and several of his students attended a conference on infinitistic methods held in Warsaw. Then in 1960, Tarski presided at Stanford University at the Third International Congress for Logic, Methodology and Philosophy of Science. This last was organized under the aegis of the Division of Logic and Methodology of Science (DLMPS) of the International Union of the History and Philosophy of Science. The story of how the DLMPS was established in 1955 through the efforts especially of Tarski and Evert Beth is of separate interest, to be told elsewhere. In a sense it is an intellectual descendant of the Unity of Science movement, but

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[22] Personal communication (e-mail message of 29 September 2000).

[23] A full survey of this development and its applications is to be found in B.F. Caviness and J. R. Johnson (eds.), *Quantifier Elimination and Cylindrical Algebraic Decomposition,* Wien: Springer (1998).

now with logic at center stage. Since 1960, the LMPS congresses have met regularly, on the order of every three years, all over the world.

As one sees from all this, Halmos' statement that "there weren't many conferences, jamborees, colloquia, and workshops in those days, and the few that existed were treasured," became instantly quaint and outdated following the Cornell meeting of 1957. Those who attended could say that they were there at the beginning.

APPENDIX

The lists of participants, speakers, and summaries of talks presented at the Cornell meeting were typescripted, dittoed and then organized into a volume entitled:
Summer Institute for Symbolic Logic, Cornell University 1957. Summaries of talks.
No editor is listed. A bound second facsimile edition was put out on 25 July 1960 by the Communications Research Division of the Institute for Defense Analyses (of which J. Barkley Rosser was then head) and distributed to select libraries. The following lists are taken from the Index of Authors of that volume (pp. xiii-xiv), here divided into three categories.

1. Individual presentations by speakers from academic institutions.

   J. W. Addison. Jr., P. Axt, E. W. Beth, J. Butler, C.C. Chang (2 x), A. Church, A. Cobham, W. Craig, H. Curry, M. Davis (2 x), G. Dekker, B. S. Dreben (2 x), S. Feferman (2 x), R. Friedberg (3 x), P. Gilmore, P. Halmos, L. Henkin, H. Hiz, S. C. Kleene (2 x), S. Kochen, G. Kreisel (3 x), R. Lyndon, E. Mendelson, R. Montague (2 x), A. Nerode (2 x), S. Orey, H. Putnam, M. Rabin (2 x), H. Ribeiro, A. Robinson (3 x), R. M. Robinson, H. Rogers, J. B. Rosser (2 x), D. Scott (2 x), J. R. Shoenfield (2 x), C. Spector (2 x), A. Tarski (2 x), R. L. Vaught, and H. Wang (2 x).

2. Joint presentations by speakers from academic institutions.

   M. Davis with H. Putnam, L. Henkin with A. Tarski, G. Kreisel with D. Lacombe and J. R. Shoenfield, R. Montague with A. Tarski, R. Montague with R. L. Vaught, M. Rabin with D. Scott, D. Scott with A. Tarski, and A. Tarski with R. L. Vaught.

3. Presentations by speakers from the computer industry.

D. M. Brender, G. W. Collins, W. L. Duda, B. Dunham, R. Fridshal, M. J. Gazalé, H. Gelernter, J. Jeenel, C. Katz, M. Kochen, J. H. North, J. P. Roth, D. Sayre, P. Sheridan, and C. C. Yehling.

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