Tarski's Conception of Logic[1]

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In its widest scope, Tarski thought the aims of logic should be the creation of "a unified conceptual apparatus which would supply a common basis for the whole of human knowledge." Those were his very words in the Preface to the first English edition of the *Introduction to Logic* (1940). Toward that grand end, in the post-war years when the institutional and financial resources became available, with extraordinary persistence and determination Tarski campaigned vigorously on behalf of logic on several fronts from his increasingly powerful base at the University of California in Berkeley. The first order of business was to build up a school in logic bridging the university's Mathematics and Philosophy Departments, and the opening wedge in that was the hiring of Leon Henkin as Professor of Mathematics in 1953. From then on, Henkin was Tarski's right-hand man in the logic campaigns, locally, nationally and internationally, but he had other allies, both in Mathematics and in Philosophy. The first goal was to increase the proportion of logicians on the mathematics faculty to 10% of the whole; that took a number of years, eventually achieved with the appointment of Addison, Vaught, Solovay, Scott, Silver, Harrington and McKenzie. Through his influence in Philosophy, he succeeded in recruiting Myhill, Craig, Chihara and Sluga. Hans Sluga tells a story which gives a vivid picture of how Tarski operated: they met in 1966 when Tarski was in London to give the Shearman Lectures at Bedford College. Sluga, then a young faculty member interested in the philosophy of logic, was delegated to show him around. Personally impressed, at the end of his stay Tarski asked Sluga if he would like to come to Berkeley. Sluga said, "You mean permanently?" Tarski replied, "Yes." Sluga said, "You mean you can invite me just like that?" and Tarski said, "If *I* tell them to take you, they will take you."

A formal bridge between mathematics and philosophy in Berkeley was forged with the establishment in 1959 of the interdepartmental graduate degree program in Logic and Methodology. In his letter to the dean urging its creation, Tarski was already able to describe the University of California as one of the most important centers of such studies in the world. The first Ph.D. in the program was awarded in 1964, and it has been going strong ever since.

The campaign for logic on the national and the international scene was carried on in the form of major meetings either totally devoted to logic or in which logic was a

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centerpiece. Before the outbreak of the Second World War, in recognition of the growing importance of modern logic it had been natural for many of the broadly representative meetings of mathematicians and of philosophers to include lectures by logicians or even to have special sections devoted to logic. Tarski, in particular, had primarily been involved in several Unity of Science Congresses which grew out of the Vienna Circle, 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 philosophers like Carnap, Quine and others.

The big post-war meetings with their greatly increased emphasis on logic got rolling with the famous Institute in Symbolic Logic, held at Cornell University for five weeks in the summer of 1957 under the sponsorship of the American Mathematical Society. Tarski was one of the principal organizers of the Cornell institute. At the end of that same year, Tarski and Henkin organized the conference on The Axiomatic Method in Mathematics and Physics at U.C. Berkeley. In 1960, Tarski presided at Stanford University at the Third International Congress for Logic, Methodology and Philosophy of Science, which featured logic in its first three sections. And in 1963 Addison, Henkin and Tarski organized the very important Theory of Models conference at Berkeley, at the dawn of an explosion in model-theoretic methods in algebra and set theory.

By the way, the 1960 Stanford conference was organized under a new Division of Logic, Methodology and Philosophy of Science of the International Union of the History and Philosophy of Science. The story of how that Division was forged in the mid-1950s through the efforts especially of Tarski and Evert Beth is of separate interest, that I can't expand on here. In a sense the Logic and Methodology congresses are an intellectual descendant of the Unity of Science movement, but now with logic at center stage, very much in tune with Tarski's conception of logic as a common basis for the whole of human knowledge. Since 1960, they have met regularly, on the order of every three years, all over the world. I don't know if it is a record, but the one in Florence in 1995 had over 1000 participants and--in Poland--the 1999 meeting in Cracow had about 600 participants.

Though Tarski was ecumenical in his efforts to establish the importance of logic in these various ways, in his own work--even that part of it which is considered to be philosophically significant and which he made a special point of bringing to the attention of philosophers--he was first and foremost a mathematician, and--even more so--a set-theoretical one. What I want to emphasize here is how this way of approaching things affected his conception of logic, both broadly and, in particular, in his explication of the notions of *truth*, of *logical consequence* and of *what is a logical term*.

We can trace Tarski's set-theoretic approach to conceptual analysis back to his mathematical studies at the University of Warsaw during the years 1919-1924, alongside his logical studies with Stanislaw Lesniewski and Jan Lukasiewicz. Tarski's choice of concentration on mathematics and logic in this period was fortuitous due to the phenomenal intellectual explosion in these subjects in Poland following its independence in 1918. On the side of logic this has been richly detailed by Jan Wolenski in his indispensable book about the Lvov-Warsaw school (1989). A valuable account on the mathematical side is given in the little volume of "remembrances and reflections" by Kazimierz Kuratowski, *A Half Century of Polish Mathematics* (1980). The grounds for the post-war explosion in Polish mathematics were laid by a young professor, Zygmunt Janiszewski. He had obtained a doctor's degree in the then newly developing subject of topology in Paris in 1912, and was appointed, along with the topologist, Stefan Mazurkiewicz, to the faculty of mathematics at the University of Warsaw in 1915. It was Janiszewski's brilliant idea to establish a distinctive Polish school of mathematics and to make an impact on the international scene by founding a new journal called *Fundamenta Mathematicae* devoted entirely to a few subjects undergoing active development. Namely, it was to concentrate on the modern directions of set theory, topology, mathematical logic and the foundations of mathematics that had begun to flourish in Western Europe early in the Twentieth Century. Moreover, the articles in that journal were to be written in *international* languages--which at the time meant French, German or English--and to break a tradition of publication in Polish, a language practically no one but Poles could read. Unfortunately, Janiszewski died in the flu epidemic of 1920 and did not live to see the appearance of the first volume of *Fundamenta* that same year.

Tarski's teachers in mathematics at the University of Warsaw were the young and vital Waclaw Sierpinski, Stefan Mazurkiewicz and Kazimierz Kuratowski; Sierpinski and Mazurkiewicz were professors and Kuratowski was a docent. In 1919, the year that Tarski began his studies, the old man of the group was Sierpinski, aged thirty-seven; Mazurkiewicz was thirty-one, while Kuratowski at twenty-three was the "baby". During the earlier inter-war years at Warsaw University, Mazurkiewicz was the chair and central figure in the mathematics department, noted for the brilliance and intelligence of his lectures. He was an extremely active researcher, especially but not exclusively in the field of topology, and he influenced many young people to do research in the modern fields of mathematics. Mazurkiewicz and Sierpinski were the initial co-editors-in-chief of

Fundamenta Mathematicae. Besides his editorial work, Mazurkiewicz chaired the meetings of the Warsaw division of the Polish Mathematical Society and--according to Kuratowski--was the life and soul of the unofficial "coffee house" meetings that would continue afterward.

The senior member in the Warsaw mathematics department, Waclaw Sierpinski, was especially noted for his work in set theory, a subject that Tarski took up with a vengeance directly following his doctoral work on Lesniewski's system of protothetic. Though Cantorian set theory was still greeted in some quarters with much suspicion and hostility, it was due to such people as Hausdorff in Germany and Sierpinski in Poland that it was transformed into a systematic field that could be pursued with as much confidence as more traditional parts of mathematics.

One of the things that I want to emphasize in Tarski's background is that in the 1920s, the period of his intellectual maturation in mathematics, topology was dominated by the set-theoretical approach, and its great progress lay as much in conceptual analysis as in new results. We take the definitions of the concepts of limit point, closed set, open set, connected set, compact set, continuous function, and homeomorphism--to name only some of the most basic ones--so much for granted that it takes some effort to put ourselves back in the frame of mind of that fast-evolving era in which such definitions were formulated and came to be accepted. Of course, some of the ideas of general topology go back to Cantor and Weierstrass, but it was not until the 1910s that general topology emerged as a subject in its own right. Tarski couldn't have missed being impressed by the evident success of that work in its use of set theory in turning vague informal concepts into precise definitions, in terms of which definite and often remarkable theorems could be proved.

The overt impact of topology on Tarski's work includes his seminal paper with Kuratowski on the connection of definability in the real numbers with the projective hierarchy, then his work on the sentential calculus and topology in 1936, and its transformation with J.C.C. McKinsey via closure algebras in the papers of the 1940s. Less directly, I think his axiomatization of the consequence operation in the early work on the methodology of deductive systems is modeled on the closure operation in topology; and, finally, it was Tarski who stressed that the finiteness property of satisfiability of arbitrary sets of sentences in the first-order predicate calculus is to be described as a compactness property of a suitable topological space. But what I want to emphasize instead of the *overt* appearance of topology in his work is *its paradigmatic use of set theory for the purposes of conceptual analysis*. It is that to which I now turn.

Let's look to begin with at how the notions of sentential function and satisfaction are defined in the *Wahrheitsbegriff*.[i] Tarski there distinguishes three kinds of definitions in this paper: *inductive*, *recursive* and *normal*. Roughly speaking, according to his explanations, in an inductive definition of a class or relation S we define a sequence S_n, where S_0 is defined outright, and S_{n+1} is defined explicitly in terms of S_n, and then S is defined explicitly in terms of the sequence of S_n s In a recursive definition, we define S in terms of itself. Finally, in a normal definition, S is given by an explicit set-theoretical definition. Interestingly, Tarski says of recursive definitions that they may raise "methodological misgivings", but that they can be transformed into equivalent inductive or normal definitions in a systematic way, and are preferable to the latter because they are more perspicuous. But he does take pains to spell out the equivalent normal (set-theoretical) definition in each case. Thus, for example, x is a sentential function (or formula, as we would now say) if and only if x belongs to the smallest class which contains the atomic formulas and is closed under negation, disjunction and universal quantification. Similarly, for satisfaction in the language of classes--which is the language Tarski uses to illustrate his definition of satisfaction--a sequence f of classes satisfies a sentential function x if and only if the pair (f, x) belongs to the smallest relation S satisfying the appropriate closure conditions. These explicit set-theoretical definitions have a surface similarity, but in modern terms there is quite a difference between them that Tarski does not elicit. Namely, the property of being a sentential function is primitive recursive, hence meaningful to a finitist, but the relation of satisfaction is essentially infinitary and impredicative. While such a definition would thus not be acceptable in the restricted form of metamathematics that Hilbert dictated for his consistency program, it clearly did not faze Tarski.

Let us turn now to Tarski's definition of logical consequence in his famous short, relatively informal paper of 1936, according to which "The sentence φ *follows logically* from the sentences of the class K if and only if every model of the class K is also a model of the sentence φ." I want to set aside here the interesting historical question as to how Tarski's conception of model in the 1936 paper relates to the one commonly taken these days and that he himself used systematically in the post-war years (cf. Hodges 1985/1986). I also want to set aside the provocative examination of Tarski's definition by John Etchemendy (1990), with its interesting challenge to whether it properly explicates the informal notion of logical consequence. My focus instead is on the nature of the definition itself: on the face of it, very simply, it reduces the notion of logical consequence to the notion of truth in a model, and thence to the notion of satisfaction. But that oversimplifies matters, for as Tarski stressed:

> Underlying our whole construction is the division of all terms of the language discussed into logical and extra-logical. This division is certainly not quite arbitrary. If, for example, we were to include among the extra-logical signs the implication sign, or the universal quantifier, then our definition of the concept of consequence would lead to results which obviously contradict ordinary usage. On the other hand, no objective grounds are known to me which permit us to draw a sharp boundary between the two groups of terms. It seems to me to be possible to include among logical terms some which are usually regarded by logicians as extra-logical without running into consequences which stand in sharp contrast to ordinary usage.

This is presaged earlier in the article by Tarski's argument against the proposed explication of the notion of logical consequence in syntactic terms, according to which φ is a consequence of K in the language of a formal theory T if it follows from K by the axioms and rules of inference of T. In his "scepticism" against the formal explication, he poses consequence via the ω-rule, in which a sentence A of the form "Every natural number possess the property P", follows from the collection of instances P(n) for each natural number n. If it is accepted that consequence under the ω-rule counts as logical consequence then the quantifier 'for all natural numbers...' must also be included as a logical operation. But the issue of the division between logical and non-logical terms opens up another problem for Tarski's definition of logical consequence, for if the logical operations can go beyond those of the classical first-order predicate calculus, one will first have to explain how the semantical notions of satisfaction and truth can be extended to any choice of such. In that case his definition of these concepts in the *Wahrheitsbegriff* could no longer be considered as definitively general, but merely as a special case which is illustrative of how one would seek to proceed. It is perhaps because of this that Tarski himself stated at the end of the 1936 article that he is "not at all of the opinion that ... the problem of a materially adequate [i.e., extensionally correct] definition of the concept of consequence has been completely solved."

Tarski did not return to the issue of how, if at all, the terms of a language are to be divided between those that are logical and those that are extra-logical for another thirty years, but when he did--in his lecture for a general audience, “What are logical notions”, first given at Bedford College in London in 1966 (published posthumously as Tarski 1986)--it was with a quite definite proposal. Namely, it locates logical notions in the finite simple type structure of classes and relations over an arbitrary non-empty domain M of individuals, and singles out the logical ones among all possible entities in such a structure as exactly those which are invariant under arbitrary permutations of the underlying domain M of individuals.

In his lecture, Tarski gave several simple examples of logical notions in this sense, as follows:

(i) No individual is a logical notion, assuming there are at least two individuals.

(ii) The only classes of individuals which are logical are the empty class and the universal class.

(iii) The only binary relations between individuals which are logical are the empty relation, the universal relation, the identity relation and its complement.

(iv) At the next level, i.e. classes of classes of individuals, Tarski mentions as logical notions those given by cardinality properties of classes, and says that “the only properties of classes (of individuals) which are logical are properties concerning the number of elements in these classes. That a class consists of three elements, or four elements...that it is finite, or infinite--these are logical notions, and are essentially the only logical notions on this level.”

(v) Finally, among relations between classes (of individuals) Tarski points to several which are “well known to those of you who have studied the elements of logic” such as “inclusion between classes, disjointness of two classes, overlapping of two classes” , and so on. He continues: “all these are example of logical relations in the normal sense, and they are also logical in the sense of my suggestion.”

Tarski did not attempt to give examples of logical notions in higher types than those in (iv) and (v), nor did he raise the question of characterizing the logical notions, and more generally of the operations on members of the type structure that are invariant under arbitrary permutations. This is understandable in view of the general audience to which his lecture was addressed. The first such characterization was provided by McGee (1996), who showed that an operation is logical according to Tarski’s permutation-invariance criterion if and only if it is definable in the language $L_{\infty,\infty}$; this is the language defined in set theory which allows--in addition to the operation of negation--conjunctions and disjunctions of *any cardinality*, together with universal and existential quantification over a sequence of variables of any cardinality. The most familiar examples of permutation invariant operations are the cardinality quantifiers $\exists_{\geq\kappa}$ ("there exist at least κ individuals such that...") for each cardinal κ, which had been treated first by Mostowski (1957).

I have critiqued Tarski's thesis in a recent paper "Logic, logics and logicism" (1999), on several grounds, the first of which, and the one that is of primary relevance here, is that it patently "assimilates logic to mathematics, more specifically to set theory." Of course, Tarski's criterion for logical operations as those invariant under arbitrary permutations of the underlying domain is already, in and of itself, clearly set-theoretical; what McGee's characterization adds is a better appreciation of how thoroughly this commits us to explaining logical operations in terms that only make sense under the assumption of reasonably strong systems of set theory. Of course, one may well ask, "What, if any, are the alternatives?" Well, there is, for example, a proof-theoretic alternative, the idea for which goes back to the work of Gentzen and Prawitz on systems of natural deduction, namely that the meaning of a logical operation is given by its rules of introduction. When that is spelled out in precise formal terms, a result due to Zucker and Tragesser (1978) is that the logical operations in that sense are exactly those of the first-order predicate calculus, thus quite the opposite to $L_{\infty,\infty}$. So, set theory does not offer the only reasonable approach to the notion of a logical operation.

In conclusion, I want to turn to one further aspect of Tarski's conception of logic. Given that Tarski was first and foremost a mathematician, it is not surprising that he should try to interest mathematicians outside of logic in its concepts and results. From early on he seemed to think that it was the metamathematical form in which those concepts were defined that was a principal obstacle to mathematicians' appreciation of the subject, if not outside of the purview of mathematics altogether. Thus, for example, at the outset of his 1931 paper "On definable sets of real numbers", which I quote at length, he writes:

> Mathematicians, in general, do not like to deal with the notion of definability; their attitude toward this notion is one of distrust and reserve. The reasons for this aversion are quite clear and understandable. To begin with, the meaning of the term 'definable' is not unambiguous: whether a given notion is definable depends on the deductive system in which it is studied ... It is thus possible to use the notion of definability only in a relative sense. This fact has often been neglected in mathematical considerations and has been the source of numerous contradictions, of which the classical example is furnished by the well-known

antinomy of Richard. The distrust of mathematicians towards the notion in question is reinforced by the current opinion that this notion is outside the proper limits of mathematics altogether. The problems of making its meaning more precise, of removing the confusions and misunderstandings connected with it, and of establishing its fundamental properties belong to another branch of science--metamathematics.

Tarski goes on:

> Without doubt the notion of definability as usually conceived is of a metamathematical origin. I believe that I have found a general method which allows us to construct a rigorous metamathematical definition of this notion.

But, he says,

> ... by analyzing [this metamathematical] definition it proves to be possible ... to replace it by [one] formulated exclusively in mathematical terms. Under this new definition the notion of definability does not differ from other mathematical notions and need not arouse either fears or doubts; it can be discussed entirely within the domain of normal mathematical reasoning.

I believe that it was because Tarski thought that formal syntax and metamathematics were principal obstacles to mathematicians' appreciation of, and interest in, logic that he was led to work on the algebraic reformulations of logic in terms of Boolean algebras, relation algebras and, finally and most extensively, cylindric algebras. Of course, this fit in with Tarski's natural bent toward algebraization in general, witness his development of cardinal algebras and ordinal algebras and the algebras of topology mentioned above. As substantial and interesting as all that work is--in and of itself--it did not succeed in attracting the attention of main-line mathematicians in the way that he hoped.

In that respect, let me quote from a 1955 letter that Tarski wrote, with Leon Henkin, to the committee for summer institutes of the American Mathematical Society urging support of the 1957 Cornell Institute in Symbolic Logic:

> ... [many] mathematicians have the feeling that logic is 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.
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> 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.

Certainly the contact that logic has made with other branches of mathematics since the 1950s has been remarkable, perhaps most significantly through the model theory of algebra that Tarski pioneered with his decision method for the theory of real numbers. I wish that he could have been present at the meeting in June 2000 of the Association for Symbolic Logic held in Urbana-Champaign, to help assess the achievements of logic in the 20th century and its widening prospects for mathematicians, philosophers, computer scientists, and linguists, among others, in the 21st century. Surely he would have been enthusiastic about the wealth of accomplishments, and the great continued promise, and surely he would have continued to want everybody to care as passionately about logic as he did, and moreover to give it primacy and not just use it as one particularly valuable tool among others. But I wonder what he would have said of his grand hopes for logic as providing a unified conceptual basis for all of human knowledge.

References

John Etchemendy, *The Concept of Logical Consequence*, Harvard Univ. Press (Cambridge) 1990; 2nd edn., CSLI (Stanford) 1999.

Solomon Feferman, “Logic, logics and logicism”, *Notre Dame Journal of Formal Logic40* (1999) 31-54.

Wilfrid Hodges, “Truth in a structure”, *Proceedings of the Aristotelian Society*, new series, *86* (1985/1986), 131-151.

Kazimierz Kuratowski, *A Half Century of Polish Mathematics*, Pergamon Press (Oxford) 1980.

Vann McGee, “Logical operations”, *Journal of Philosophical Logic 25* (1996) 567-80.

Andrzej Mostowski, “On a generalization of quantifiers”, *Fundamenta Mathematicae 44* (1957) 12-36.

Alfred Tarski, *Introduction to Logic and to the Methodology of Deductive Sciences,* 1st edn., Oxford University Press (New York) 1940 (and many subsequent editions).

Alfred Tarski, *Logic, Semantics, Metamathematics,* 2nd. edn., John Corcoran (ed.), translations by J. H. Woodger, Hackett Publishing Company (Indianapolis) 1983.

Alfred Tarski, "What are logical notions", *History and Philosophy of Logic 7*(1986) 143-154.

Jan Wolenski, *Logic and Philosophy in the Lwow-Warsaw School,* Kluwer Academic Publishers (Dordrecht) 1989.

Jeffery I. Zucker and Robert Tragesser, "The adequacy problem for inferential logic", *Journal of Philosophical Logic 7*(1978) 501-16.

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[i] References here and below to Tarski's work on truth, logical consequence and definability are made via the collection of translations in the 2nd edition of *Logic, Semantics, Metamathematics* (1983).