Foreword

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The book in your hands is an important contribution to Euclidean geometry. In order to put that contribution into perspective, we begin by reviewing its ancestors. Its primordial ancestor, of course, is Euclid's *Elements*. We know nothing about Euclid except what he wrote: not his date or place of birth or death, not how much of his book represented his own work, not even where he lived. Possibly Euclid lived and worked in Alexandria, which was the intellectual center of the Greek civilization at the time, around 300 BCE. His textbook comprised thirteen Books (a "book" was in those days more like a "chapter" today, but it occupied one papyrus scroll). This book is famous not only for codifying what was then known of geometry, but also for introducing the modern style of mathematics: it contains axioms and proofs of numbered propositions, each of which is supposed to be rigorously derived from the axioms and the previous propositions.

Over the centuries (beginning already with Geminus and Proclus[1]), there has been criticism of some of Euclid's arguments. In particular, Euclid's Postulate 5 was the center of attention. That postulate says that if K and L are two lines, traversed by a third line M making interior angles (totalling) less than two right angles on one side of M, then K and L must meet on that side of M. In other words, K and L are not parallel (lines are called "parallel" if they do not meet). It was felt that this postulate was not so "self-evident" as the other postulates, and great efforts were expended by several mathematicians in attempts to prove it from the other postulates, or from some new postulate that might be more self-evident. Along the way some errors in Euclid were discovered. For example, Euclid implicitly assumed that certain lines must intersect. He actually needed "Pasch's axiom", which asserts that if a line meets one side of a triangle, then it meets one of the other two sides or a vertex.

Several of these efforts foundered, as mistakes crept into the reasoning,

[1]Proclus, *A commentary on the first book of Euclid's Elements*, translated with introduction and notes by Glenn R. Morrow, Princeton University Press (1992).

for example by implicitly assuming some geometrical fact that is actually equivalent to Postulate 5 itself. These difficulties led to a greater focus on ways to set out the reasoning in such a way as to assist in verifying its correctness. In Italy and Germany, formal logic was born from these efforts: geometry was the mother of logic. Eventually it was realized that the fifth postulate *cannot* be proved from the others, because there are "models" of the other axioms in which the fifth postulate fails. This realization, that "non-Euclidean" geometry is possible, was an eye-opener, as people had previously thought that geometry was fixed by the nature of the universe, or (as Kant said) by the nature of the human mind.

Logic grew up, left its mother, and developed further in the hands of Peano (who introduced many of the modern symbols) and Frege. It married set theory, whose fathers were Cantor and Dedekind[2] , and together they gave birth to the "axiomatic method", according to which each branch of mathematics should be developed from appropriate axioms, by logical deductions from the axioms, *without regard to what the axioms were about.* Non-Euclidean geometry had shown that the same axioms could be "about" different things; the axiomatic method would allow mathematics to deal with several (or even many) examples simultaneously. This method was championed by David Hilbert, and in his 1899 book, *Foundations of Geometry*, he applied the method to geometry. He famously said that if geometry were done correctly, one could speak of "tables, chairs, and beer mugs" instead of "points, lines, and planes", and all the deductions should still be correct. For example, instead of assuming that there is exactly one line through two given points, you would assume there is exactly one chair through two given tables. That makes no sense as a statement about real tables and chairs, but if we *assume* it, and continue calling points, lines, and planes by these new names, all the reasoning should still be valid. That is, nothing about the nature of actual points, lines, and planes is allowed to be used, except what is captured in the axioms.

But the marriage of set theory and logic gave birth, not only to this beautiful and talented child (the axiomatic method), but also to a monster: the "paradoxes". For example, the well-known "Russell's paradox" about the set of sets which are not members of themselves. It appeared that self-evident postulates and correct logic could lead to a contradiction! Revulsion at these misbegotten creatures led to an even more careful examination of the principles of reasoning. The blame was laid on a too-liberal use of set theory; a restraining order was placed on set theory and mathe-

[2]Two fathers? Well, in Cantor's case the mother was Fourier analysis, since Cantor started by studying the sets on which Fourier series converge. In Dedekind's case the mother was number theory, since he started by studying "ideal numbers", needed to fix the failure of unique factorization of algebraic integers.

matics was henceforth to live with this restraining order. The new regime was called "first order logic".

Hilbert, however, had used set theory freely in his *Foundations of Geometry.* He needed some "continuity axioms" to guarantee that a line could not be extended to a larger line. He used the axiom of Archimedes to guarantee that it could not be made *longer.* That axiom mentions the concept of "natural number", which goes beyond geometry. Then he also needed to guarantee that a line cannot be enlarged by inserting new points, i.e. filling "gaps". This problem had been solved by Dedekind using Dedekind cuts, which are defined as sets of rational numbers. For example, the point on L at distance $\sqrt{2}$ from a point called 0 exists because we can define the set of points whose distance x from 0 satisfies $x^2 < 2$. In modern terminology, the axiom that Dedekind cuts can be filled is called "second order", because it involves sets of points. Hilbert did not take this for a completeness axiom, but stated more directly that a line cannot be extended; that axiom requires mentioning not only sets of points, but sets of unspecified "new points" in a hypothetical extension.

By the 1920s, it was realized that Hilbert's axiom system was not first-order, because of Archimedes's axiom and the completeness axiom, as well as because of treating segments, rays, and angles as sets. It was therefore necessary to go back over the axiomatization of geometry and remove this flaw, providing a first-order axiomatization of geometry. This task was undertaken by Alfred Tarski, in his 1926-27 lectures at the University of Warsaw. Readers new to this subject must understand that there are now *three* different kinds of Euclidean geometry to think about:

- Full Euclidean geometry, with a second-order completeness axiom, mentioning arbitrary sets of points. This theory has only one model, the Euclidean plane $\mathbb{R}^2$.

- First order Euclidean geometry, or Tarski geometry, in which Archimedes's axiom is not used, and the completeness axiom is replaced by a first-order schema, saying in essence that all first-order definable cuts can be filled. One might, like Tarski, have only variables for points, or one might have variables for lines, circles, etc. as well.

- Ruler and compass geometry, which is like first-order Euclidean geometry except that the completeness schema is replaced by "line-circle continuity", which says that a line that contains a point inside a circle (nearer to its center than some point on the circle) must meet the circle.

Once it was realized that the first order theories would have more than one model, and only the second order theories would uniquely characterize the plane, the question immediately arose whether one can characterize the models of such theories as planes F^2 coordinatized by certain kinds of fields F. Such a theorem is called a "representation theorem" for a geometry. Starting with his 1926-27 lectures, Tarski began working towards such a theorem, and by 1930 he had achieved some significant results: a first-order axiomatization of Euclidean geometry, and the representation theorem that its models are planes over real-closed fields. A real-closed field is a field in which "gaps" defined as places where a polynomial changes sign are filled. (Technically, polynomials of odd degree have roots, and positive elements have square roots.) Tarski showed that his first order geometry is equivalent (using coordinates) to the first order theory of real closed fields. Similarly, the models of ruler and compass geometry are planes F^2 over some Euclidean field F. (A Euclidean field is an ordered field in which positive elements have square roots.)

Tarski proved an amazing theorem about the theory of real-closed fields, known as "elimination of quantifiers", which meant that every formula of the theory is equivalent to one without quantifiers. It follows that there is a "decision method" for Euclidean geometry; in principle the truth or falsity of any given formula must (a) follow from the axioms and (b) be determinable in a mechanical way.

By contrast, there is no such decision procedure for the theory of Euclidean fields, and hence, also not for ruler and compass geometry, as was proved by Ziegler in 1982.[3] The result is not mentioned in this book, but would certainly have been if the publication dates had not so nearly overlapped. Ziegler's theorem applies to any finitely axiomatizable extension of field theory, so it gives another proof of Tarski's theorem that the theory of real closed fields is not finitely axiomatizable.

Tarski's results first were published in a RAND Corporation technical report in 1948, but without any details about geometry; the focus was on the decision procedure for algebra (i.e. real-closed fields). What happened between 1930 and 1948 will be discussed below. For now, we fast-forward to 1955. In 1955-56 Tarski lectured on geometry at the University of California at Berkeley, and enlisted his students to help with the problems of simplifying the axiom system, studying the relationships between the axioms, and checking that the axioms were really sufficient to develop Euclidean geometry. There were many details to check. In 1959 a *very* brief description of the theory and statement of the 1930 results was published.[4] In 1959–60, Wanda Szmielew and Tarski began the project of (as

[3]Ziegler, Martin, Einige Unentscheidbare Körpertheorien, *Enseignement Math.* **28** 269–280 (1982).

[4]Alfred Tarski, What is elementary geometry?, in *The axiomatic method, with special*

it was described much later by Steven Givant) “preparing a treatise on the foundations of geometry developed within the framework of contemporary mathematical logic.” The first part of this work was to be a systematic development of Euclidean geometry based on Tarski’s axioms. Drafts of this part were written, but the project was never completed along these lines. Szmielew’s well-known book *Foundations of Geometry*, with Karl Borsuk as co-author, was published in 1960 and, judging by its content, must have been completed before she worked with Tarski, as the axiomatic system is quite different, closer to Hilbert’s (and not first order). Szmielew did the writing, although she no doubt discussed matters with Tarski. When Wolfram Schwabhäuser came to work with Tarski in Berkeley in 1965, he was assigned to teach the course “Foundations of geometry” in 1965-66 that Szmielew had taught at Berkeley during her visit five years earlier; she sent him (or Tarski gave him) a copy of the manuscript to assist him in his lectures. When he returned to West Germany, he also taught the subject there. He probably had access not only to Szmielew’s manuscript, but also to the 1965 Ph. D. thesis of Tarski’s student H. N. Gupta, whose amazing proofs were never published separately, although he did publish a two-page summary of his results.[5] In 1967, Szmielew made another visit to Berkeley, and wrote another manuscript, as described in Schwabhäuser’s introduction, page 6 of this book. She and Tarski were planning to write a book together based on this manuscript, and indeed a project report to NSF in 1970 says their manuscript is “now being prepared for publication”, but that book was still unwritten in 1976, when Szmielew died.

Schwabhäuser started writing this book in 1974 (according to his wife). As originally planned, it contains two parts. The first part is a systematic development of Euclidean geometry from (a variant of) Tarski’s axioms. The second part presents metamathematical results about the theory. Schwabhäuser is the sole author of the second part. The first part (specifically sections 1-8, 10, and the first part of 9) is Szmielew’s first manuscript, with changes that Schwabhäuser himself calls “inessential” in his introduction, which is why she is named as a joint author, and the axiom system itself is due to Tarski, which is partly why he is named as a joint author. The joint authorship was agreed to by Tarski in 1978, two years after the death of Szmielew, and represented the conclusion of

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reference to geometry and physics. Proceedings of an International Symposium held at the Univ. of Calif., Berkeley, Dec. 26, 1957–Jan. 4, 1958, edited by Henkin, Suppes, and Tarski. Studies in Logic and the Foundations of Mathematics, North-Holland, Amsteram (1959). Available as a 2007 reprint, Brouwer Press, ISBN 1-443-72812-8

[5] Gupta, Haragauri N., An axiomatization of finite-dimensional Cartesian spaces over arbitrary ordered fields. *Bulletin de l’Académia Polonaise des Sceinces, série des sciences mathématiques, astronomiques, et physiques 13:551-552 (1965).*

a substantial dispute between Szmielew (represented by Tarski after her death) and Schwabhäuser about his intent to publish material based on her manuscript.[6]

Now that Tarski was a co-author, Schwabhäuser asked Tarski for suggestions for material to be included. Tarski and Steven Givant replied in a long letter. That letter, though not originally intended for publication, was polished up for publication 21 years later by its original co-author Steven Givant.[7] Very likely most readers of this book will also want to study that letter, which in published form is 39 pages long, containing a detailed list of the various forms of the axioms that have been considered and a history of their evolution. Tarski made no direct contribution to the book, i.e., no part of the text was written by Tarski.

What changes in Hilbert's system were required to achieve Tarski's goal of a first-order axiom system for geometry? Hilbert's planar foundations had two primitive objects (points and lines) and four primitive relations (incidence, betweenness, congruence of segments and congruence of angles). Veblen (in 1904) and Pieri (in 1908) used only one primitive object (point) and two primitive relations betweenness and equidistance. Tarski followed Veblen and Pieri's approach.[8] But the reduction to two primitives is not the main issue. Nor is the the fact that Hilbert treated segments and circles as sets of points an important issue; that is easily fixable. The main problem is the axiom of continuity. Hilbert had two axioms that are not first order. Tarski's system replaced Hilbert's continuity axiom with a schema that says certain Dedekind cuts can be filled, namely those cuts that can be defined in the first-order language, i.e. by reference to geometric concepts only, as opposed to allowing arbitrary definitions of sets or even sets with no definitions at all. In retrospect at least, this seems fairly straightforward. But Tarski, Szmielew, Gupta, and Schwabhäuser placed other demands on the axioms: They should be as

[6]Szmielew's 1967 manuscript apparently contained her proof of the representation theorem. Givant showed this manuscript to Schwabhäuser in 1975, but he wrote to Tarski that he had never read it, so the work in the last part of Part I was independently done. Tarski's correspondence can be found in the Tarski Nachlass in the Bancroft Library at the University of California, Berkeley.

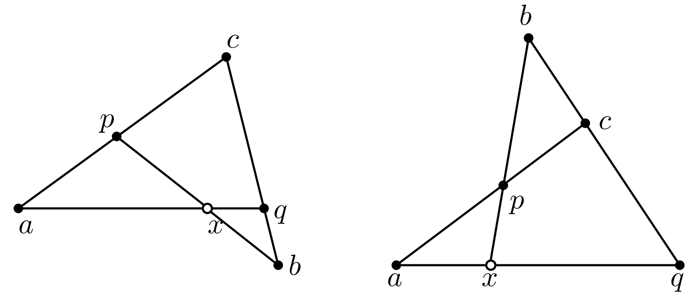
[7]Alfred Tarski and Steven Givant, Tarski's system of geometry, *Bulletin of Symbolic Logic* 5 (2), 1999.

[8]In *The Legacy of Mario Pieri in Geometry and Arithmetic* (Birkhauser, 2007), authors Elene Marchisotto and James Smith translate some of Pieri's work and discuss his important contributions. On page 357 one discovers that the Ph. D. advisor of Smith was Tarski's student H. N. Gupta, and that Tarski came to his thesis defense, and asked a single question: where did this field begin? Smith naively answered, with Hilbert, and it was Tarski who pointed him to Pieri. Section 3.10 of that book contains relevant historical notes.

simple as possible, and as few as possible, and they should be mutually independent. Achieving these goals meant that one had to investigate alternative forms of several axioms, check that simpler forms could prove more complex forms (or could not), and consider whether certain axioms might be superfluous or were really necessary. Except for the continuity schema, the axioms should have a particular simple form, which I here call "Euclidean form". This kind of formula asserts that if some points are given bearing certain relations, then some other points exist bearing certain relations to each other and the original points. All of Euclid's theorems are of this form (as has to be checked one proposition at a time). Moreover, one should also pay attention to the axiomatization of "ruler-and-compass" geometry, in which the full continuity schema is replaced by "line-circle" and "circle-circle" continuity. The line-circle continuity axiom asserts that if a line L has a point inside circle C, then L meets C. The circle-circle continuity axiom is similar, but with "line L" replaced by "circle K." As an example of the issues that arise: is there a way to construct the perpendicular to line L from point P without using any continuity axiom at all? The usual way involves constructing the intersection points of two circles. This problem was beautifully solved by H. N. Gupta, and the solution is in the book you are holding. Here is another issue: the axiom of Pasch, mentioned above, has an "or" (disjunction) in the conclusion, so it isn't in Euclidean form; it says that if a line meets side AC of a triangle ABC, then one of these alternatives holds: either it meets AB, or it meets BC, or it meets B, or it contains A and C both. This disjunction is ugly; disjunctions do not occur in Euclid. One therefore considers two disjunction-free versions, known as "inner Pasch" and "outer Pasch". Are they equivalent? Do they imply Pasch's axiom? Which one is simpler in the sense of developing the rest of the theory? Schwabhäuser chose "inner Pasch". A footnote in the Tarski-Givant letter indicates that Tarski favored "outer Pasch"; at any rate the relations between the two are studied in this book. Inner and outer Pasch are illustrated in Fig. 1.

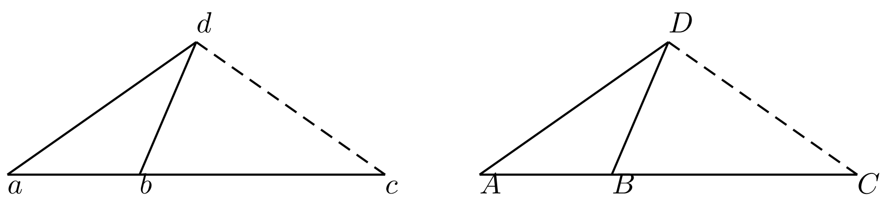
Another issue is the treatment of lines, rays, and angles. Hilbert treated segments and rays as sets of points, but Tarski's theories have only variables for points. Lines and rays are treated implicitly, as pairs of points. Hilbert defined an angle to be a pair of rays, so it is not even a set of points, but a set of sets of points. But then, although angles are defined, Hilbert takes the relation of congruence of angles to be primitive. On the other hand, Tarski treated angles indirectly, so theorems about angles become theorems about triples of non-collinear points. For example, the side-angle-side principle of triangle congruence (SAS) has to be expressed using points only; this becomes the "five-segment axiom" in

Figure 1: Inner Pasch (left) and Outer Pasch (right). Line pb meets triangle acq in one side. The open circles show the points asserted to exist on the other side.



Tarski's theories; see Fig. 2. In that figure, the congruent triangles are dbc and DBC; the congruence of angle dbc and angle DBC is expressed by the congruence of triangles ABD and abd (which is expressed by the three congruences of their sides, which in turn is expressed using the equidistance relation on points). It may take some time for geometers accustomed to thinking about angles as sets (or even as pictures) to learn to think about angles as being given by three points, but after this change of viewpoint, one often feels that the first-order versions state more succinctly "what is really going on."

Figure 2: Tarski's 5-segment axiom. cd is determined. That is, if $ab = AB$, $bc = BC$, $ad = AD$, and $bd = BD$, then the fifth segment $cd = CD$ as well.



Part I of this book, then, contains the details of these axiomatic developments; geometry is built up step by step, using first only a few axioms, then introducing more axioms as they are needed. In a sense this geometry is "pre-Euclidean", supplying the infrastructure on which Euclid is (or should be) based. Throughout this subject there is the danger of making a mistake by accidentally assuming something that is "intuitively obvious" but has not been rigorously proved. Therefore it is necessary to construct and exhibit detailed proofs. Schwabhäuser was, by accounts of those who knew him, a very careful man; and in modern times much of Part I of this book has been computer-checked for correctness.[9] In that computer-checking, it seems that only one trivial and easily fixable error was found.

Part II of the book is devoted to metamathematical results about formal theories of geometry. This part of the book does not share the joint authorship of Part I; it is Schwabhäuser's alone; yet Tarski's presence is felt, because many of the theorems either are Tarski's, or are generalizations to other geometrical theories of theorems Tarski proved for his "elementary geometry" (that corresponds to real-closed fields). Schwabhäuser considers questions of decidability, completeness, finite axiomatizability (or not), and definability; and discusses model-theoretic proofs as well as proofs by quantifier elimination. By 1983, there was considerable discussion in print of quantifier elimination, so there was no need to publish the details that had been missing in 1959; instead Schwabhäuser extends the results to hyperbolic geometry, absolute (sometimes called neutral) geometry, as well as affine and projective geometry. To study these different geometries, one needs to study the process of coordinatizing them, and check that coordinatization can be done within the confines of certain specified first-order axiom systems. Hyperbolic geometry is coordinatized in a different way than Euclidean geometry; and each of these geometries offers some complications. The struggle to complete Part II was Schwabhäuser's alone, so we know less about the steps along the way than we do about Part I.

Every reader naturally has some curiosity about the authors. What kind of people were Tarski, Szmielew, and Schwabhäuser, and what else happened in their lives besides the work in this book? Tarski was a very colorful character, and his life has been chronicled in the biography by the Fefermans[10], where one can find numerous anecdotes and details about Tarski's life. We therefore give only the barest outline here. (Quotes about Tarski's life below are from this biography.)

Tarski was born in 1901 in Warsaw, as Alfred Tajtelbaum (or Teitelbaum in the Germanic spelling). His mother was "brilliant, well educated, and willful", as well as an heiress. His father was a businessman from a family of businessmen. Polish was spoken at home, not Yiddish, although the family was Jewish. Poland was the center of the world in

[9] Frédérique Guilhot, Formalisation en coq et visualisation d'un cours de géométrie pour le lycée, *Revue des Sciences et Technologies de l'Information, Technique et Science Informatiques, Langages applicatifs*, **24**, 1113–1138, 2005.

[10] Feferman, Anita, and Feferman, Solomon, *Alfred Tarski: Life and Logic*, Cambridge University Press (2004).

logic in the post-World War I period. Tarski's teachers included Sierpiński and Kuratowski (big names in set theory and topology) and in logic, Jan Łukasiewicz and Stanislaw Leśniewski, whose work established the "Polish school" in axiomatic logic. Tarski's first two papers were about problems posed by Leśniewski; the first was published under the name Tajtelbaum, the second under "Tajtelbaum-Tarski", and from 1924 on, he was officially Tarski. There are three amusing stories in his biography about how that particular name was chosen; but the point was, "Tarski" did not sound Jewish. To make the point certain, Tarski converted to Catholicism in 1924. This conversion and name change made possible a professional life in Polish academia, which would not have been open to a Jew. His biographers speculate that more was involved: it was fashionable in certain circles to be "more Polish than Jewish." Tarski was a professed atheist, but "if you were going to be Polish then you had to say you were Catholic."

He received his Ph. D. degree in 1924, from the University of Warsaw, under Leśniewski. That same year he and Banach published their famous "Banach-Tarski paradox", about cutting a sphere into pieces that can be re-assembled into a larger sphere. He was appointed docent at Warsaw University, which gave him the right to lecture, some recognition, but not much money. So he continued to live in his parents' home; and once when he asked his father for money, Ignacy Teitelbaum replied, "Money? You need money? Well, why don't you go see your old man Tarski?" He lived with his parents until 1929, when he married Maria Witkowska. She was "small, dark, pretty, socially adept, and above all warm, understanding, and loyal.... She was never in competition with Alfred and had no need for the limelight; in short, she was the perfect mate for him."

As mentioned above, already in 1927 Tarski had done significant work on axiomatic geometry. How did he get from set theory to geometry? Through logic. To understand the intellectual setting of Tarski's work on geometry, we must review the development of logic 1900-1927. Russell and Whitehead's work *Principia Mathematica* was a masterpiece of axiomatic formalism; remember that first-order logic did not exist at the time, and Russell and Whitehead went straight to a theory of first-order, second-order, third-order, etc. classes called "types". Intuitively, we have some "objects" and then sets of those objects, sets of those sets, and so on to sets of sets of sets of sets, level after level. This was seen as the way to avoid the paradoxes, for the "set of sets that are not members of themselves" cannot occur at any level; no set can be a member of itself since it would have to have two different levels. Zermelo worked out an axiom system based on this idea in 1904-1908, but it seems to have not made an impact in Poland. Russell and Whitehead's theory was more influential among logicians. It was very complicated, and the natural way to proceed was to isolate the first-order part and study that more deeply. That is the background to the famous theorems of Löwenheim and Skolem, which in the period from 1915 into the 1920s began the subject of first-order logic.

Thus in 1925 and 1926, first-order logic was in its infancy; but it was clear that Hilbert's famous 1899 book *Foundations of Geometry*, which at the time had been at the cutting edge of rigor, was no longer "rigorous enough". It made too much use of set theory; it could speak of arbitrary sets of points. In 1927–29, Tarski conducted the "exercise sessions" for Łukasiewicz's logic seminar, which apparently involved some lecturing and independent research. He took up the subject of quantifier elimination.

For readers who are not logicians: a "quantifier" is either "there exists" or "for every". If one has to decide whether a statement involving such phrases is true or false, in general there will be infinitely many cases to consider, so it will be hard to give a rule. But if the vocabulary is sufficiently restricted it may be possible to "eliminate quantifiers" and show that every statement is equivalent to one without quantifiers. As an example, "there exists a (real) solution of $ax^2 + bx + c = 0$" is equivalent to $b^2 - 4ac \geq 0$, as readers who know the quadratic formula will recognize. This equivalence eliminates the "there exists" in the first statement. We don't need to check infinitely many possible values of x.

Several examples of situations in which quantifiers could be eliminated were already known at this early stage; Skolem had given a couple, and the American logician C. H. Langford had already obtained the result about discrete linear orderings, which every logic student now learns. Tarski found some new results, and since he was running an "exercise session", he suggested to his student Mojżes Presburger that he find a quantifier elimination procedure for the theory of natural numbers with addition. That theory is known today as "Presburger arithmetic", testifying to Presburger's successful solution of the problem in the spring of 1928. It served as his master's thesis in 1930.[11]

In these early years, Tarski must have begun to formulate a first-order version of Euclidean geometry, and realized that it would, or should, be equivalent in some sense to a first-order algebraic theory. In geometry one can erect two perpendicular lines as coordinate axes, and give geometric definitions of addition and multiplication, as was done already by Descartes in his *La Geometrie*, which was at the cutting edge of rigor in

[11]In a just world this would have been the beginning of a long and brilliant career. Presburger's paper was only f pages; and its significance may not have been fully recognized at the time; so he only got a master's degree, not a Ph. D., and left the university to work in the insurance industry. But there are fates worse than that: Presburger was a Jew, and died in a Nazi death camp in 1943.

its time. And, in the subject known as "analytic geometry", one can discuss points, lines, and circles in terms of algebraic formulae. Once these theories were formulated, even roughly, the question whether they admit elimination of quantifiers would have occurred to Tarski. One needs something like the quadratic-formula example above, but *much* more general: given any system of algebraic equations and inequalities (for example $ax^2 + bx + c = 0$ is a very simple case) needs a way to find conditions on the coefficients (analogous to $b^2 - 4ac \geq 0$, which involves the coefficients a, b, and c, but not x) that tell you whether the system has a solution or not. There can be many variables x, not just one, and many equations and inequalities. Of course this is complicated, but special cases were already known (Sturm's theorem, for example); nevertheless it took Tarski until 1930 to work it out. However, *nothing* was published about it until 1948, and that publication was only a statement of results, with no details about the actual method.

The reason, of course, was that a lot of other pressing things happened in the period 1930-1948. Tarski went to Vienna, met Carnap, Gödel, and Quine. He lost a competition for a professorship in Lvov; then in 1937 he was a candidate for a professorship at the University of Poznan, but (in spite of his name change and conversion) was not hired because he was a Jew. On his way back from a conference in Holland in 1938, he stopped in Berlin, and heard Hitler "giving his violent speech after his return from Munich", and then stayed up until three or four o'clock discussing logic (not politics) with "the three logicians who had not yet fled the Nazi terror."

In August, 1939, Tarski was on the ocean liner *Pilsudski* bound for New York, not to flee the Nazis, but to attend a conference in Cambridge, Massachusetts called the *Unity of Science*. He thought he was going to stay a month, and came with just one suitcase, no winter clothes, and no wife. On September 1, the Germans bombed Warsaw and invaded. At the conference, Tarski met the American logicians, including Church and Curry, and Church's students Kleene and Rosser. (He had met Quine in Vienna.) These people and many others wrote letters resulting in Tarski being granted a permanent U.S. visa, but he still had no job. As his biographers put it, "jobs were scarce and competition was fierce. To make matters worse, there was an influx of brilliant intellectual refugees from Europe also desperate for positions; and finally, ... mathematical logic was not a mainstream field at any university. To tide him over until he found something, funds were cobbled together at Harvard to appoint him as a research associate." Not only did he have job trouble, his family was still in Poland, in the middle of the war. He tried to get his family to Copenhagen and from there to the U.S. But the Germans invaded Denmark, blocking this escape route. He later tried to get them out through Sweden, with the aid of a philosopher whose father was on the Swedish Supreme Court, but this failed too, since submarine warfare had intensified and it was now impossible to travel from Sweden to the U.S. Meantime, Russell and Carnap were also at Harvard that year, and had a seminar with Tarski and Quine. In January, 1942, Tarski became a visiting fellow at the Institute for Advanced Studies, where he became friends with Gödel and his wife. Finally in the fall of 1942, Tarski moved to California to accept a new job at the University of California in Berkeley, where he worked for the rest of his life. Not, however, without a nerve-wracking encounter with his draft board in Princeton. Since he was now a permanent resident, he could be drafted, and it took "a flurry of letters" to get him an occupational deferment. He remained separated from his family until after the war.

Tarski thus had a number of concerns more pressing than publishing his work on geometry, but he *had* already in 1939 prepared a manuscript for publication. The Fefermans (in Interlude IV of their biography) have assembled a number of details about the various stages in its publication. The 1939 monograph, entitled *The Completeness of Elementary Algebra and Geometry*, was to be published in Paris. But the German invasion "disrupted the publication process." Tarski did have two sets of page proofs that remained. In 1948, J. C. C. McKinsey, a logician who had befriended Tarski at Harvard, was working at RAND Corporation. He "may have suggested that the procedure could be programmed for computer calculations of the optimization of strategies in the theory of games." As a result of this suggestion he wound up revising the 1939 manuscript under Tarski's supervision, and it was published as a RAND technical report in 1948, under the new title, *A Decision Method for Elementary Algebra and Geometry*, which was reprinted more publicly by UC Berkeley in 1951, and again in Volume I of Tarski's *Collected Works*. In 1967, a "lightly edited" version of the original 1939 page proofs was published in France, 27 years late. But better late than never!

These presentations of Tarski's work focused on the algebraic side of *Elementary Algebra and Geometry.* The metamathematical results are about polynomials and inequalities, not about points, lines, and circles. All one needs about the geometry is that it should support coordinatization, so that one can prove in an appropriate sense that it is "equivalent" to an algebraic theory. What went unpublished (until this book appeared in 1983) was Tarski's work on the axiomatization(s) of geometry itself; his improvements on Hilbert, so to speak. Of course he had *some* axiomatic theory of geometry in his 1926-27 lectures at the University of Warsaw, in the "exercise section". Thanks to the Tarski-Givant letter (*op. cit.*) we even know what this axiom system was, and the story of how Szmielew and Gupta and Schwabhäuser improved and studied the theory, resulting in this volume, has been told at the beginning of this foreword.

The second author of this book is Wanda Szmielew. Wanda was born April 5, 1918, in Warsaw, and started her university studies in 1935. She met and worked with Tarski and Lindenbaum, and formed a personal relationship with Tarski, as we know because she and her husband went hiking in the mountains with Tarski (who kept a journal of his hikes and hiking companions). During the war, she worked as a surveyor, but also found time to do the mathematical work that eventually became her dissertation. After the war, she worked at the Łódź Institute of Technology and at Łódź University. She earned her M. A. at the University of Warsaw in 1947 and became senior assistant to the Chair of Mathematics.[12]

She came to Berkeley in 1949 under Tarski's sponsorship. She had already proved the decidability of the theory of Abelian groups, and the plan was to write this up as a Ph. D. dissertation. This plan was successful. Since the theorem had already been proved, though, there may well have been time for Tarski and Szmielew to discuss geometry, but no details are known about their mathematical discussions that year. For details of their personal lives, the reader may consult the Tarski biography *op. cit.* Wanda returned to Poland in 1950, Ph. D. in hand, and became assistant professor at the University of Warsaw, rising to docent in 1954 and to associate professor in 1957.

Nine years after her first visit, she spent another year in Berkeley (1959–60). Her book *Foundations of Geometry*, co-authored by Karol Borsuk, was published by North-Holland in 1960, so it must have been close to final form by the time her visit to Berkeley began; but as discussed above, it does not follow Tarski's theories. On this visit, she and Tarski definitely discussed geometry: they worked together on the manuscript already discussed on the first page of this foreword. Tarski had worked with students on problems in the axiomatization of geometry since 1955, so he must have hoped that Wanda would help him bring the axiomatization and results about it to final form. But as discussed above, that didn't happen until Schwabhäuser wrote this book. Wanda did have a Ph. D. student, Zenon Piesyk, who wrote a dissertation in 1965 (in Polish) entitled *Axiomatic System of Alfred Tarski.* Ten years later, Piesyk published a book (also in Polish): *Wykłady z geometrii elementarnej* (Lectures on elementary geometry). It seems one would have to go to Warsaw to see

[12] These details are from *Wanda Szmielew, 1918–1976*, an obituary in *Studia Logica* **36** 4 (1977). The original Polish obituary is referenced in that English translation.

either of these works.

We turn now to Wolfram Schwabhäuser, who was born on May 20, 1931, in Riesa, a small town on the River Elbe about forty kilometers from Dresden. He remained in Riesa, attending high school while World War II raged, more or less all around him. In 1950, he began his university studies in mathematics at Humboldt University in Berlin, earning his degree in 1956, and then his Ph. D. in 1960. From 1956 on, he held a position as Scientific Assistant in the Institute for Mathematical Logic at Humboldt University under Prof. Schröter, and with his Ph. D. in 1960 he was promoted to "Oberassistent". Schwabhauser was invited to speak at both the 1960 Congress at Stanford and the 1963 Model Theory Symposium at Berkeley, but in both cases he could not obtain a visa from the U. S. Department of State. In 1964, he attended the International Congress for Logic, Methodology, and Philosophy of Science in Jerusalem, where he presented a paper with the same title as this book. There, he met Tarski, and they discussed the possibility of Schwabhäuser coming to Berkeley the following year. After the conference, he did not return to East Germany. According to his wife, this decision was not at all spontaneous, but was carefully planned; however, he dared not tell anyone, even his parents; he had given no sign of his plan. For example, he had even ordered the coal for the winter, knowing he would not be there to use it. The need for secrecy led him to abandon all his personal items, including books, diaries, and letters. After the Congress, he took a position as assistant to Prof. Hans Hermes, at the Institute for Foundational Studies of the University of Münster, West Germany. In July 1965, he earned the post-Ph. D. credential known as "habilitation" at Münster. Then in the academic year 1965–66, the conversation with Tarski in Jerusalem came to fruition: Schwabhäuser visited Berkeley, where he worked with Tarski on the foundations of geometry.

The year of Schwabhäuser's visit to Berkeley was also the year of Gupta's Ph. D. thesis, so Schwabhäuser had company. During that year, Schwabhäuser taught two courses: one on mathematical logic, and one on the foundations of geometry; and he also received support from Tarski's NSF grant "Foundations of Geometry." Schwabhäuser returned to West Germany in 1966, and became docent at the University of Bonn, where he became Professor in 1969. In 1967, he married Inge-Marie Scholl, who had studied mathematics and physics at the Free University of Berlin (in West Berlin). In his final career move, he became professor of Informatik (Computer Science) in Stuttgart in 1973, where he served the rest of his life.

Schwabhäuser's hobbies were playing the piano and the flute, photography, and hiking. His wife, Inge-Marie, wrote that "Our greatest pleasure was the birth of our son Thomas in 1976."

Schwabhäuser's book was published in 1983; and according to Inge-Marie, he worked on it intensely for ten years. So his 1978 letter to Tarski asking for material for inclusion came in the middle of the effort. Two years after the publication of his book, he became ill, in spring 1985, and his student, research assistant, and friend Uwe Schöning filled in for him as a teacher. He died December 27 of that year, of cancer. His obituary[13] mentions that he is survived by his nine-year-old son.

Schöning described Schwabhäuser as "a nice, calm, friendly, serious person, a gentleman of science, very well respected in the faculty." Schöning also has some memories of the book project: "Writing this book was a tough work and effort which lasted many years. The epsilon which he tolerated for having mistakes in the book was very very small, if not to say, zero. According to this small epsilon, the time to finish this work was accordingly long. His secretary, Ms. Sonnenschein, worked with an IBM ball-head typewriter. She had to use six such heads to supply all the needed special symbols and Greek letters." Mathematicians of a certain age will fondly recall those IBM Selectric type balls; you who grew up with TEX will never know what hard work it used to be to write a book.

This book is a testament to the enduring attractions of points, lines, and circles, and to the dedication and perseverance of its authors, who kept at their work as long as the spark of life flowed in their blood, and who, when that spark was gone, passed the torch to others. Though their progress was impeded by world wars and by the Cold War, and though they, like the rest of us, had jobs and families (or sometimes, had no jobs, and had families halfway around the world, or a country to which they could not return), they still managed to think about points, lines, circles, and axioms, and check details.

Now that the book is no longer out of print, it is also in digital form, which makes it safe to say: nine-tenths of the readers of this book have not yet been born. Five hundred years from now, when technology has developed beyond our wildest imagination, today's pdf (portable document format) files will still be readable and a small minority of people will still be interested in points, lines, circles, and axioms. They will have no difficulty finding this book, and the fundamental results it contains will still be studied.

[13]This obituary, by Walter Knödel, was published in the Stuttgart university magazine *Uni-kurier* in February, 1986.