Andrzej Mostowski and the notion of a model

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Abstract. Model theory became an independent discipline within logic during the first half of the 1950s. Andrzej Mostowski made several distinctive contributions to this development through papers of his. Also his 1948 textbook of logic covers material in the foundations of model theory, and in 1966 he published a survey book with several chapters on model theory. We examine his choice of technical terms and concepts during this period, and we discuss a criticism made by Abraham Robinson of the coverage of model theory in the 1966 book. On this basis we draw some conclusions about Mostowski's aims and attitudes, which were often different from those of other pioneers in the field.

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1. The emergence of a new discipline

The official birthday of model theory has to be the publication of Alfred Tarski's paper [50] in 1954, which began:

> Within the last years a new branch of metamathematics has been developing. It is called the *theory of models* and can be regarded as a part of the semantics of formalized theories. ([50] p. 572) (1)

But we can see propaganda for the new discipline, as yet unnamed, four years earlier in the addresses by Abraham Robinson [37] and Tarski [49] to the International Congress of Mathematicians in 1950. One theme of the propaganda was that the new discipline would provide new tools that algebraists and other mathematicians could use within their own disciplines. Thus Robinson:

> ... contemporary symbolic logic can produce useful tools—though by no means omnipotent ones—for the development of actual mathematics, more particularly for the development of algebra and, it would appear, algebraic geometry (Robinson [37] p. 694). (2)

Likewise Tarski ([49] p. 717) spoke of applications 'which may be of general interest to mathematicians and especially to algebraists'.

The new discipline absorbed various earlier pieces of work by Veblen, Löwenheim, Skolem, Gödel, Mal'tsev and others, going back to the beginning of the twentieth century. Some of the early contributors to the new discipline were seen at the time, and are still seen, as primarily model theorists (at least within logic—some like Mal'tsev and Robinson had mathematical interests outside logic). Besides Mal'tsev and Robinson this group includes Henkin, Vaught and Fraïssé. But Andrzej Mostowski, though he was a major figure in early model theory, was never primarily a model theorist. Like Feferman (his junior by fifteen years), Mostowski had interests that ranged across the whole of logic, including set theory and the study of Gödel's incompleteness theorem. For example one of his papers with 'models' in the title ([31], 1953) is mainly quoted for its proofs that Zermelo-Fraenkel set theory and first-order Peano arithmetic are not finitely axiomatisable.

The creation of a new mathematical discipline is always a challenge for historians of mathematics, to understand where the new discipline came from, what were the forces that drove it in the direction that it took, and what the creators of the discipline understood themselves to be doing.

As far as I know, Mostowski never published a reflective account of the aims of model theory, or of his own aims in this area. Of course we can infer something about his aims from the problems that he chose to work on. But in this paper I explore two other routes into his thinking. The first is his choice of terminology—not just the words that he chose to use, but the concepts that he chose to give names to. The second is an issue raised by Robinson, that Mostowski's historical book [34] (1966) might not be not entirely objective in its account of model theory. We will assess Robinson's criticism; to the extent that it is sound, it throws some light on Mostowski's understanding of what model theory is.

My wife and I met Mostowski at a party at Richard Montague's house in Los Angeles in summer 1967; we spent some time talking with him and found him genial and friendly. My wife recalls that we met him again a few days later at a picnic on the Dockweiler State Beach, and on that occasion he burned his feet on the hot sand. I was sorry I never got to know him better.

I warmly thank the editors of this volume for their kind invitation to submit this chapter, Jan Woleński for suggesting Mostowski as a topic, and Barbara Bogacka for checking my translations of some of Mostowski's Polish. Of course none of these people are responsible for my errors. A copy of Mostowski's book [29] came into my hands too long ago for me to remember where it came from; my apologies to anybody I should be thanking for it.

2. Mostowski's writings in model theory

We briefly review those parts of Mostowski's work that relate most directly to model theory.

There are three papers that had a major impact on model theory. The first is the paper [30] (1952) 'On direct products of theories', which analysed the sentences true in a cartesian power in terms of the sentences true in the factors. The paper [10] of Feferman and Vaught owes its existence largely to Feferman's study of ideas in [30] ([10] p. 58, [9] p. 37)—though Feferman may have learned them directly from Tarski rather than through reading the paper. (Possibly Tarski's reluctance to accept Feferman's early work on this topic as of dissertation standard was based on a concern about whether Feferman had added enough to what was already in [30]; see [8] p. 211.) The second is the paper [7] (1956) 'Models of axiomatic theories admitting automorphisms', written jointly with Mostowski's student Andrzej Ehrenfeucht. This paper introduced the notion of indiscernibles, which quickly became an indispensable tool of both model theory and set theory. The third is the paper [33] (1957) 'On a generalization of quantifiers'; this paper created one of the main strands of research in generalised model theory, with applications spilling over into linguistics [36].

Mostowski's earlier paper [28] (1947) 'On absolute properties of relations' seems to have had little impact, because it was written around a rather obscure and specialised question. But the paper contains a version of Henkin models of second-order model theory, three years before Henkin published his own account; and on his page 34 Mostowski comes close to formulating the notion of an elementary extension, nine years before Tarski and Vaught [54] published the definitive definition.

The paper [32] (1956) 'Concerning a problem of H. Scholz' was one of the earliest papers in a difficult area of work relating computation theory to the model theory of finite structures. See [6] for a recent assessment of the field and Mostowski's contribution to it.

The paper [5] (1978) 'The elementary theory of well-ordering—a metamathematical study' was a reconstruction by Doner and Tarski, after Mostowski's death, of work that Mostowski did with Tarski around 1940, applying the (syntactic) method of elimination of quantifiers to the first-order theory of well-orderings. The paper itself is not model-theoretic, but like other early work in quantifier elimination, its results were of great interest to model theorists. Mostowski counted Tarski as the actual supervisor of his doctoral thesis (1938) in set theory, though formally Kuratowski was the supervisor since Tarski was not a professor at the time (Krajewski and Srebrny [23] p. 5).

The paper [31] (1953) 'On models of axiomatic systems' applies the model-theoretic notion of satisfaction. We will study its use of the word 'model'.

Mostowski also wrote two books which reviewed mathematical logic as a whole. One of these was his Polish textbook [29] *Logika Matematyczna: Kurs Uniwersytecki* of 1948, which competes with Kleene's more advanced *Introduction to Metamathematics* for the role of the last major pre-model-theoretic textbook of logic. The other was a historical survey *Thirty Years of Foundational Studies* ([34], 1966). The second of these books has two chapters on 'theory of models', and both books contain material close to the foundations of model theory.

3. Background and notation

We will be discussing the formalisation of several informal notions. It will be helpful to have a convention for distinguishing the informal versions from the formal ones. I will distinguish the formal versions by writing them with small capitals. Thus we talk informally of assigning 'interpretations' to symbols, but Tarski's formal version of this notion is called an 'INTERPRETATION'.

Several important papers in foundations of mathematics in the period 1910–1940 have the following setting, which for convenience we can call the 'archetypal pattern'. A formal language L is described, and explanations are given for the meanings of the symbols of the language. Some of the symbols have logical meanings, for example conjunction. Other symbols, called the 'nonlogical symbols' or the 'primitives', have meanings that come from the topic under discussion. The meanings of the nonlogical symbols add up to a description of a structure M with a given universe or domain of elements; in a modern usage we call M the 'standard interpretation' of the language L or the theory T. A set T of sentences of L is presented as sentences that are true of M under the given explanation of their meanings. The paper studies the structure M by analysing logical properties of the set T. Often T, called a 'theory', is presented as the main topic of study. Probably the best-known example of this general format is Gödel's paper [11] on the incompleteness of first-order Peano arithmetic.

This format came under strain as scholars asked new questions. Two particular areas of strain are worth noting at once. The first is that in the archetypal pattern, the nonlogical symbols of the language L have a fixed set of meanings that determine a particular structure. But sometimes one wants to talk about two or more structures that make the same sentences true. So it became necessary to have a way of detaching the fixed meanings from the nonlogical symbols.

The second area of strain was that logicians became increasingly interested in the justification of metatheoretical arguments. So these arguments should be formalised, and some axioms for them set down. But then there was a question of what to formalise, and in what language.

One example of these strains is the paper of Löwenheim [24], which shows that if a first-order sentence is true in some structure, then it is true in some structure with at most denumerably many elements. In today's terminology, Löwenheim takes a structure M and constructs a substructure N of M that satisfies some of the same sentences of L as M did. But Löwenheim's proof sometimes descends into obscurity, because he has no explicit notion of a substructure; see Badesa [1], particularly his sections 6.1 and 6.2, for documentation of this.

The word 'model', in a sense relevant to model theory, begins to appear in German mathematical writing of the mid 1920s, in order to handle the situation where a structure M is introduced and then a second structure N is constructed from M. The second structure is called a model. One of the earliest examples of this usage is Hermann Weyl's discussion [55] (written in 1925) of geometric 'models' for proofs of consistency. For example on his pages 30 and 31 we read of a 'Modell' of Lobachevsky's geometry within Euclid's geometry; here Euclid's geometry forms the standard interpretation of the primitives 'point', 'line' etc., and the model is the nonstandard interpretation of these expressions that proves the consistency of Lobachevsky's axioms.

Another example of this usage, again from 1925, is in von Neumann's paper [35] on 'models' of set theory. Von Neumann supposes that we have a set—call it T—of axioms for set theory, and he shows how to construct, within the universe of sets described by T, a denumerable 'model' (his word) of the axioms. In order to carry out the construction, he describes a second system of axioms—call it U—and claims 'there obviously exists a smallest' system of sets Σ' satisfying U. To support the claim, he describes a procedure for 'constructing' the system Σ' in infinitely many steps (p. 407f in van Heijenoort [12]).

If this procedure is to be justified in a formal system, what formal system should be used? Could it be the axiom system T presented by von Neumann himself in [35]? Or should it be some other more powerful system? And exactly what calculations need to be represented in the formal system? Besides formalising the construction of Σ', do we also need to formalise a proof that a system of sets satisfying U also satisfies T? And if we do, would it be enough to show how to formalise this claim separately for each axiom, or must we have a single formal proof covering all of them?

The moral of the von Neumann example is that a piece of metatheory may have different formalisations, not all equivalent. We will see in Section 6 below that even when we know what parts of the metatheory we want to formalise, there may be more than one way of choosing concepts to do the required job.

4. Some of Tarski's proposals

The second quarter of the twentieth century saw attempts by various people, both to apply the archetypal pattern to a wider range of problems, and to improve the formalisation of the pattern. Tarski made a number of contributions. In [44] (1933) he showed how to define, within the setting of the archetypal pattern, the notion 'ϕ is a true sentence of L'. This notion makes sense, given that in the archetypal pattern the expressions of L all have appropriate fixed meanings, so it is determinate whether any given sentence of L is true or not. This definition was Tarski's original 'truth definition'. Along the way, Tarski also defined 'satisfaction' in the following sense. Suppose $\phi(\bar{x})$ is a formula of L with free variables $\bar{x}$, and we assign meanings to the variables in $\bar{x}$. Then it makes sense to ask whether the assigned meanings make $\phi(\bar{x})$ true, in other words, whether they 'satisfy' $\phi(\bar{x})$. Tarski analysed what set-theoretic content the assigned meanings would need to have in order for us to give a formal definition of SATISFACTION; the resulting definition defines when an assignment of set-theoretic objects to the free variables counts as satisfying the formula. To formalise the definition, Tarski introduced a second theory $T^\star$ (a 'metatheory'), which would contain an exact copy of T but also enough set theory to formalise the syntax of L and carry out some definitions by induction on the complexity of formulas of L.

This was useful work in itself, but no help for dealing with the problem of alternative structures that make the same set of sentences true. In [18] I documented how progressive advances in the aims of metamathematics forced Tarski to adapt his truth definition step by step, until eventually he had the model-theoretic form which he published with Vaught in [54]. Already in 1933 Tarski could handle the case of two structures, one a substructure of the other, so that he was equipped to formalise Löwenheim's argument discussed above. But this case is in a way degenerate, because the relations etc. of the substructure agree with those of the larger structure, so that all that is needed to specify the substructure is a formula expressing its domain.

In 1936 [47] Tarski adapted the truth definition to allow new meanings to be assigned to the nonlogical symbols of L. His idea was to consider an assignment α of appropriate set-theoretic objects to the nonlogical symbols of L, and a sentence ϕ of L. He would replace the nonlogical symbols in ϕ by distinct variables, thus getting a formula ψ. The assignment α was defined to be a MODEL of ϕ if it satisfied ψ, where the assignment is carried over from the nonlogical symbols to the variables put in place of them. For the particular purposes of the paper [47], Tarski wanted to talk about all possible assignments of meanings, so that the MODEL could in fact be exactly the same as the original assignment of meanings to nonlogical symbols in the theory T. But otherwise Tarski followed Weyl and von Neumann in using the expression MODEL for a new assignment of meanings.

The model-theoretic truth definition of Tarski and Vaught [54], which we are told was already available by 1952 or 1953 ([54] p. 82 footnote), dropped this rigmarole of replacing the symbols by new variables, and assigned the meanings (or rather the set-theoretic objects representing them) directly to the nonlogical symbols. It was no longer assumed that the nonlogical symbols came with preassigned meanings.

In [48] Tarski defined a notion that he called INTERPRETATION. (The book went through several revisions. In the 1994 edition the definition appears on page 114 in §37, but in the earliest editions the definition is in §33.) It relates two theories, say T and $T^\star$ in languages L and $L^\star$ respectively. Like a MODEL of T, an INTERPRETATION of T involves an assignment β to the nonlogical symbols of L. But instead of assigning set-theoretic objects that convey meanings, β assigns to each nonlogical symbol of L an expression of $L^\star$. For each formula ϕ of L we construct a formula ϕ^β of $L^\star$ by replacing each nonlogical symbol of L by the expression assigned to it by β. We call β an INTERPRETATION of T in $T^\star$ if for each sentence ϕ of T, ϕ^β is provable from $T^\star$.

Tarski gives some simple examples of INTERPRETATIONS. He takes T to be a theory expressing that $\cong$ is an equivalence relation on the set S. Then for example let $T^\star$ be a theory of the arithmetic of rational numbers, with a symbol Q for the rational numbers and a symbol Z for the integers. We can write a formula $x \equiv y$ of

$L^\star$ which expresses that x and y are in Q and the difference $x-y$ is in Z. Then let β be the assignment that assigns Q to S and $\equiv$ to $\cong$. Assuming that $T^\star$ is strong enough to allow us to prove that $\equiv$ is an equivalence relation on Q, the assignment β is an INTERPRETATION of T in $T^\star$. One can also construct geometric examples that interpret hyperbolic geometry in euclidean geometry, representing the Klein-Beltrami model of hyperbolic geometry as an INTERPRETATION in Tarski's sense.

In Tarski's terminology, INTERPRETATIONS and MODELS are really quite different kinds of thing. What they have in common is that they both consist of assignments of things to the nonlogical symbols of L. But the things assigned, and the condition for the assignment to be a MODEL or an INTERPRETATION, are quite different. The notion of an INTERPRETATION is purely syntactic: the assignment assigns strings of symbols to symbols, and the condition for the assignment to be an INTERPRETATION is that certain things are formally provable from a given theory. By contrast for a MODEL the assignment assigns set-theoretic objects, and the condition for the assignment to be a MODEL involves the notion of satisfaction; it requires that certain things are *true*, not that they are *provable*.

As we see from the example of the Klein-Beltrami model, the names 'interpretation' and 'model' have sometimes been applied to the same things. On p. 114f of the 1994 edition of [48] Tarski makes some remarks that seem to be intended to show how an INTERPRETATION in his sense could sometimes be regarded as a MODEL in his sense. He notes that if $L^\star$ has a standard interpretation, then the expressions assigned by β all have meanings determined by this interpretation, and we can think of β as assigning these meanings rather than the expressions. Then *if the sentences of $T^\star$ are true for the standard interpretation*, anything provable from $T^\star$ will be true too, and one can infer that the assignment of meanings (rather than expressions) is a MODEL of T in $T^\star$. These remarks are correct, but Tarski may have created some confusion by making them. In general MODEL and INTERPRETATION are different notions, and neither is a special case of the other. The jump that Tarski describes from INTERPRETATION to MODEL is not just a change of viewpoint; it needs a substantial mathematical proof.

In [53] (1953) p. 20f, Tarski gives another definition of INTERPRETATION, which agrees with the one above but removes the assumption that the theories have standard interpretations. Also instead of physically altering the formula ϕ to ϕ^β, Tarski achieves the same effect by adding the assignment β in the form of explicit definitions of the nonlogical symbols of L in the theory $T^\star$. With this new definition of INTERPRETATION Tarski's attempt above to bring MODELS and INTERPRETATIONS together loses its purchase. (Strictly the new definition is of 'INTERPRETABLE IN', but Tarski still speaks of an 'INTERPRETATION', as in the footnote on his p. 22.)

5. 'Theories'

With this much background in place, we can begin to look at Mostowski's papers, starting with [30] 'On direct products of theories' . This paper considers a structure A that is a cartesian power B^I of a structure B. (Mostowski also considers a variant of cartesian power.) Mostowski asks how the first-order theory of A can be calculated from information about what sentences are true in B.

Mostowski introduces the symbol T for the set of first-order sentences true in the structure B, and he writes T^I (where I is the index set of the power) for the set of first-order sentences true in A. The sets T and T^I are called 'theories'. This notation 'T^I' implies that T^I depends only on T and I. But this creates a problem: might T^I not also depend on B? For example T might also be the set of sentences true in some other structure B'; how do we know that $(B')^I$ satisfies the same sentences as B^I?

In fact one of the main results of the paper is that we do know exactly this: T^I is determined by T and I, or even (as Mostowski points out) by T and the cardinality of I. But it seems as if Mostowski has begged the question by assuming this result when he sets up his terminology.

Closer inspection shows that no question is begged, but Mostowski is using an archaic terminology. At the beginning of the paper he tells us

> Elementary mathematical theories are always concerned with certain functions defined in a set **I** (called the *universe of discourse* of the theory) and certain relations with the common domain **I**. ([30] p. 1) (3)

In other words, a theory always has a standard interpretation. What Mostowski means when he talks of 'products of theories' is exactly the same as what the modern reader would express by talking of products of structures. Mostowski is working in the archetypal pattern.

Of course Mostowski can choose to restrict the word 'theory' to theories that come with a certain standard interpretation, provided that he makes it clear that he is doing this (as in fact he does). But how sensible is this, at a date when other researchers in the area are freely talking about 'all the models of a set of axioms' (as e.g. in Robinson [38] (1951) p. 36)? Mostowski's use of terminology suggests that he is simply not aware that other researchers are using model theory as a framework for studying axiomatic classes in algebra or other parts of pure mathematics.

A glance at what Mostowski says about 'elementary theories' in his textbook [29] of 1948 gives no reassurance at all. Under the head 'elementary theory of groups' Mostowski writes:

> The specific constants [i.e. nonlogical symbols] of this theory (apart from the signs of equality and inequality) are L and σ. The constant L is of type $(\star)$, so it is the name of a set which in this theory we call the *group*. The constant σ is of type $(\star, \star, \star)$, which means that it is the name of a ternary relation ... ([29] p. 234) (4)

He really does seem to be telling us that for purposes of logic there just is one group!

It seems to me that we should draw the conclusion that at least by December 1949—the date when [30] was submitted—Mostowski had not bought into the propaganda that Robinson and Tarski would present for model theory at the 1950 International Congress. At that date it was not one of his aims to provide tools for algebraists. In fact no move in that direction appears in any of his later model-theoretic papers either. To my eye, none of them contain anything that invites the description 'application of model theory to algebra'.

When he came to write the historical book [34], Mostowski did find himself reviewing other logicians' work in this area, and he seems to have adopted a more relaxed notion of 'theory'. But in [34] he gives no definition of 'theory', or any indication of how it differs from 'axiomatic theory' or from 'set of sentences', or any sign that he is now using it in a different meaning from his earlier papers.

6. 'Models'

In his textbook [29] of 1948 and his paper [31] 'On models of axiomatic theories' (published in 1953) Mostowski introduces five different definitions of MODEL. For convenience we can call the two definitions in [29] definitions (48:1) and (48:2), and the three definitions in [31] definitions (53:1), (53:2) and (53:3). In brief they are as follows.

(48:1): This definition is on page 270 of [29]. It defines 'MODEL of T in $T^\star$', and it agrees with Tarski's definition of 'INTERPRETATION of T in $T^\star$ as in [48].

(48:2): This is on page 356 of [29]. It defines 'SEMANTIC MODEL of T', and it agrees with Tarski's definition of 'MODEL of T' in [47].

(53:1): This definition is on page 136 of [31]. It defines 'MODEL OF THE FIRST KIND of T in $T^\star$'. The definition is the same as (48:1), except that Mostowski requires $T^\star$ to contain enough set theory to code up the syntax of T, so that the requirements 'ϕ^β is provable in $T^\star$' take the strong form of saying that in $T^\star$ we can define ϕ^β from ϕ and β, and $T^\star$ proves a statement expressing that for every ϕ in T, ϕ^β is true.

(53:2): This definition is on page 142 of [31]. It defines 'MODEL OF THE SECOND KIND of T in $T^\star$'. It relates to (48:2) in the same way as (53:1) relates to (48:1); in other words, Mostowski requires that $T^\star$ contains enough notions—and in particular enough set theory—to allow us to define satisfaction of formulas of L, and then prove that every axiom of T is true under the given assignment. (Strictly Mostowski says that formulas expressing the truth 'hold', not that they are provable from $T^\star$. But then his argument uses the assumption that 'the existence of a real model of the second kind is provable' in $T^\star$, so the effect is the same.)

(53:3): This definition is on page 149 of [31]. It defines 'MODEL OF THE THIRD KIND of T in $T^\star$' like (53:2), but using arithmetic instead of set theory.

Let me make a few remarks first about (48:2) and (53:2), which correspond to Tarski's MODEL.

In [29] Mostowski proves Gödel's completeness theorem, using the notion of SATISFACTION but without mentioning models. For example he says that every first-order sentence is either satisfiable in the natural numbers or formally refutable (this paraphrases Theorem 7 on his p. 353). Then he moves on to the downward Löwenheim-Skolem, and this is where he introduces the definition (48:2). In fact the form of the Löwenheim-Skolem theorem that he proves is that a satisfiable theory has an at most denumerable model, and he proves it by deducing it from the form of Gödel's theorem that says that a syntactically consistent theory is satisfiable in the natural numbers. So his version of the downward Löwenheim-Skolem theorem needs no new concepts beyond the ones that he has already used for Gödel's theorem. Then why does he choose this place to introduce the notion of SEMANTIC MODEL? One possible guess is that he has at the back of his mind a form of the Löwenheim-Skolem theorem which says that a theory which is true in a standard interpretation has an at most denumerable model; in general the denumerable model will not be the model given by the standard interpretation, so we are changing the interpretations of the nonlogical symbols. If this is right, then Mostowski is harking back to the 1920s notion of 'model'.

In (53:2) Mostowski arranges the formal definition of satisfaction in $T^\star$ in such a way that there is no mention of replacing nonlogical symbols by variables. In effect, Mostowski anticipates the Tarski-Vaught definition of satisfaction in [54]. But this fact is well hidden in the technical details, and quite possibly none of Mostowski's readers realised that he had cleared away the syntactic complexities of Tarski's definition in [47].

Mostowski's aim in (53:2) was to prove a mathematical fact about Zermelo-Fraenkel set theory, taking this set theory to be $T^\star$. The proof proceeds by taking $L^\star$ as an object language, not as a metalanguage for expressing metamathematical properties of L. This quietly undermines Tarski's efforts to keep mathematics and metamathematics distinct. A few years later Robinson created nonstandard analysis in a similar way, by treating the language in which we do mathematics as one of the objects that we handle in the mathematics that we do; there is a breakdown of levels. Mathematicians appreciate this sort of move, which discovers new mathematical facts by looking at familiar things from an unfamiliar point of view.

Tarski certainly appreciated good mathematics, but it's hard to imagine moves like these of Mostowski and Robinson coming from Tarski himself. Feferman ([8] p. 223) makes the interesting remark that Robinson 'had a certain looseness of presentation that annoyed Tarski'. It never struck me that Robinson was a careless mathematician, and I wonder if the remark has to do with Robinson's willingness to ignore the 'right' way of looking at things, as for example in nonstandard analysis. If that be so, then Mostowski's definition (53:2) is loose in much the same way.

Tarski by contrast had a programme to tidy up metamathematics by giving formally correct definitions of the needed concepts, and it was his view that the question 'Which are the needed concepts?' has an objectively right answer. In a telling passage near the beginning of [43] (p. 112) he says:

> In geometry it was a question of making precise the spatial intuitions acquired empirically in everyday life, intuitions which are vague and confused by their very nature. Here [i.e. in metamathematics as opposed to geometry] we have to deal with intuitions more clear and conscious, those of a logical nature relating to another domain of science, metamathematics. To the geometers the necessity presented itself of choosing one of several incompatible meanings, but here arbitrariness in establishing the content of the term in question is reduced almost to zero. (5)

This view is likely to appeal to philosophers who regard conceptual analysis as one of the basic tools of mathematical foundations—a view encouraged by Frege's analysis of number and Turing's analysis of computability. But it's also a view that stands in the way of the kinds of conceptual sleight of hand that Mostowski and Robinson exploited. In this respect Mostowski and Robinson were the mathematicians and Tarski was the philosopher.

Turning to (48:1) and (53:1), an obvious question is why Mostowski blurs the distinction between syntax and semantics by using the word MODEL in these two cases. Again we see Mostowski disregarding distinctions that Tarski put in place. But there are some further things to be said about this case. I have discussed and documented Tarski's position on the issues elsewhere (chiefly [20] and [21]), so I beg leave to give the conclusions rather than the supporting evidence.

Mostowski notes in [31] that:

> Models of the first kind [i.e. for (53:1)] are the ones with which one has to do in the usual proofs of consistency and of independence of axiomatic systems. ([31] p. 138) (6)

This comment closely matches the applications that he gives in [29] for models of the kind (48:1); these are the main contents of his Chapter XI on 'Methodological questions'. One case that he discusses in Chapter XI is Padoa's method for proving independence of concepts within a theory.

In 1900 Padoa gave a loose description of a procedure for showing that in a formal theory T with nonlogical symbols $R_0, \ldots, R_n$, the concept expressed by R_0 is not definable in terms of the concepts expressed by $R_1, \ldots, R_n$. The procedure was to give two different interpretations of T which agree on all of $R_1, \ldots, R_n$ but disagree on R_0.

Suppose we want to make Padoa's method formally correct. How should we proceed? For example in terms of the notions introduced by Tarski, should we treat the 'interpretations' of T as INTERPRETATIONS or as MODELS? When Tarski considered the question in the 1920s and 1930s, he came down on the side of INTERPRETATIONS. In his reading, Padoa's procedure was to give a second theory

$T^\star$ and two INTERPRETATIONS β, γ of T in $T^\star$, such that β and γ assign the same expressions to $R_1, \ldots, R_n$, but it is a theorem of $T^\star$ that the expressions assigned to R_0 by β and γ are not equivalent. On this account, Padoa's method is purely syntactic. There is no reference to MODELS or SATISFACTION anywhere in it. Mostowski [29] pp. 283–291 follows this account.

Today I think most logicians would say that Tarski captured the essential mathematical content of Padoa's method, but he threw away the intuition behind it. That intuition is better captured by formalising Padoa's procedure in terms of MODELS, as we normally do today. The issue came to a head in 1953 when Beth proved that in first-order logic Padoa's condition is both necessary and sufficient for R_0 to be not definable from $R_1, \ldots, R_n$ in T. Beth expressed the condition model-theoretically, though for technical reasons he used cut-free derivations in his proof. When he sent his proof to Tarski, Tarski responded through his student Feferman that Beth had misconstrued a syntactic theorem as a semantic one, and he should rewrite so as to remove the reference to models. (Feferman later thought he remembered saying the opposite to this. But the correspondence is available in Van Ulsen's doctoral thesis; I quote the relevant passage in [21].)

To return to the obvious question mentioned earlier: why did Mostowski blur the distinction between syntax and semantics by using 'model' for Tarski's INTERPRETATIONS? One naturally asks why these five notions are all called 'model'. In [29] Mostowski says nothing to answer this question, but a footnote on page 356 does call attention to the clash of terminology. In the footnote Mostowski says

> We need to distinguish the concept of model defined thus from the concept of a model of one theory in another, as defined in Chapter XI (§2 p. 270). This is the reason for using here the term *semantic model.* (7)

The puzzled reader might well insist that it would be a better reason for *not* using the expression 'model' for both these notions.

The case of Padoa's method discussed above suggests a reason for using 'model' in all these cases, namely that Mostowski realised that Tarski's INTERPRETATIONS and Tarski's MODELS were to some extent solutions of the same problems. They could both be used to formalise earlier informal metamathematical discussions involving variation of interpretations of symbols. So Mostowski could naturally see all his definitions of MODEL as related tools in a general logical toolkit. If this is correct, then Mostowski chooses his terminology more on the basis of possible mathematical applications than on the basis of conceptual analysis. Here again, comparing him with Tarski, Mostowski is the more typical mathematician.

There is a feature of [31] that supports this reading. As he introduces each of his three definitions of MODEL, Mostowski lists some 'general facts' about it. The lists are given in similar formats for the three kinds of MODEL, 'for comparison with other notions of model'. (For (58:1) this is on p. 138f, for (58:2) on p. 142 and for (58:3) on p. 208.) The effect is as if Mostowski is providing a set of tools together with notes on where the tools can appropriately be used.

Mostowski is probably responsible for one more use of the word ‘model’, namely its use to mean ‘structure’. This use appears already in [28] (1947) where he speaks of an ‘absolute model’ without any reference to any theory that it is a model of. In [34] he often uses ‘model’ for ‘structure’—which puts an extra burden of interpretation on the reader who has to work out whether or not he means ‘model of’ some salient theory. In mitigation it should be added that when he wrote [28] the word ‘structure’ was not yet in use among model theorists; at that date one usually said ‘system’, which is even more open to confusion. Among model theorists the word ‘structure’ came into common use in the late 1950s, probably under the influence of Robinson and Bourbaki. But whatever the merits of this use of the word ‘model’, it once more shows Mostowski disregarding Tarski’s careful analysis of concepts.

7. Thirty years of foundational studies

In 1964 Mostowski gave a series of lectures ‘on the development of mathematical logic and of the study of foundations of mathematics in the years 1930–1964’ at a summer school in Vaasa, Finland. Two years later he published a revised version of these lectures [34]. He remarks in his Foreword that he hopes to convey ‘some of the enthusiasm with which I witnessed the creation of theories reported on in the following pages’. The lectures certainly live up to that hope.

They can also serve another purpose for today’s reader. Round about 1960 a consensus was forming that mathematical logic should be classified under the main heads of Proof Theory, Set Theory, Model Theory and Recursion Theory. For most of Mostowski’s career there was no such classification. He draws the divisions in quite different places, and as a result he makes connections between different areas of logic in ways that a modern reader may find fresh and stimulating. Just to pick one example at random, the chapter on ‘Semantics’ begins with the truth definition and finishes with speedup theorems in proof theory.

About his own contributions, Mostowski’s account in this book is modest to a fault. His paper with Ehrenfeucht on indiscernibles [7] is in the bibliography but not mentioned in the text; the papers [30] and [33] are not even in the bibliography. He mentions his paper [31] (at [34] p. 142), but only to record that its proof that Zermelo-Fraenkel set theory is not finitely axiomatisable contained a mistake, and the result should be credited to Richard Montague [27]. (I haven’t been able to find out what mistake he has in mind, or whether it is significant.)

8. Robinson’s complaint

Joseph Dauben in his biography of Abraham Robinson [4] records that Robinson was severely critical of the treatment of model theory in Mostowski’s book [34].

The criticism was made privately in a letter to Gerald Sacks. Robinson wrote:

> This term ['model theory'] was indeed coined by Tarski in the early fifties and this is where Mostowski in his "Thirty years of Foundational Studies" (according to which I apparently started my career in 1963) places the beginning of the subject.
> However, if you were to look at my "On the Metamathematics of Algebra" you will find that it contains not only algebraic applications but also the general framework of model theory (e.g. the general scheme of classes of sentences *versus* classes of models). At the same time I do not wish to belittle Henkin's influence on later developments. (8)
> In any case, I am not surprised to observe, again and again, that Tarski has trained his students (and that includes Mostowski) to see history in the way he wants them to. ([4] p. 450f, quoting a letter to Sacks dated 8 June 1972)

Dauben ([4] p. 449) describes this as an 'uncharacteristic letter'. Certainly it's uncomfortable. But I was glad that Dauben included it, because it does represent a side of Robinson that I and other people witnessed in the early 1970s. Possibly it was an early sign of the illness that took him soon afterwards.

Let it be said straight away that Robinson's specific complaint about [34]—that Mostowski places the start of Robinson's career in 1963—is not true. The first of Robinson's papers listed in Mostowski's bibliography is [39] from 1955. On his pages 125f Mostowski describes the contents of this paper, noting that one of Robinson's 'most important applications' of these results, in a paper of 1959, 'could hardly be obtained' by Tarski's own preferred methods.

But it is true that Mostowski's book shows no awareness that Robinson made any contributions before 1955. These contributions include his address [37] to the International Congress of Mathematicians in 1950, reporting the main results of his PhD thesis [38] submitted in 1949. Robinson had submitted a paper to the Congress, but Tarski as chair of the Logic section had intervened to elevate Robinson to an invited lecturer; in order to do this Tarski had had to argue for allowing four invited papers in logic, as opposed to the three allowed to other subjects ([4] p. 170). Mostowski must have been aware that Robinson was an invited speaker at the International Congress, since he was present at the Congress himself ([4] p. 171); but apparently Robinson's paper didn't register with him.

In fact Mostowski does mention in [34] two of the innovations that appear in Robinson's PhD thesis, but he attributes neither of them to Robinson.

One of these innovations is the use of Steinitz's theorem to prove the completeness of the theory of algebraically closed fields of a given characteristic ([34] p. 123, [38] p. 60). This was one of the more startling ideas that Robinson presented to the International Congress in 1950 [37]. Robinson showed that any two algebraically closed fields of the same characteristic are respectively elementarily equivalent to fields that have transcendence degree $\aleph_0$ and hence are isomorphic by Steinitz's theorem. Vaught later gave a simpler and more general form ('Vaught's test') to this argument by pointing out that every denumerable first-order theory that is categorical in some uncountable cardinality and has no finite models is complete—an adjustment that Robinson himself praised for its 'remarkable efficacy' ([40] p. 11). Robinson's result could be recovered by deducing from Steinitz's theorem that the theory of algebraically closed fields of a given characteristic is categorical in every uncountable cardinality. Mostowski attributes Vaught's test to Vaught but gives no attribution for the use of Steinitz.

With hindsight we can see that Robinson's use of Steinitz's theorem was a major step forwards in model theory. It showed that model theorists could apply algebraic embedding or isomorphism results to prove facts about first-order definability. This very quickly became, and remains still, one of the main themes of the subject. But it took some time for the novelty to be appreciated. For example Henkin, in his review of [38] in 1952, mentioned Robinson's result that the theory of algebraically closed fields of a given characteristic is complete, but added the dubious claim that

> the techniques underlying these derivations ... have been obtained earlier by others. ([13] p. 206) (9)

As in Section 9 below, Henkin might have been better advised not to write reviews of publications that he regarded as in competition with his own work.

The second innovation that appears in [38] and is mentioned by Mostowski without an attribution to Robinson is the notion of the formally defined class of all models of a given set of first-order sentences. Mostowski ([34] p. 119) writes this class as $E(X)$, where X is the set of sentences. People who know Tarski's work on the truth definition might reasonably expect that the notion $E(X)$ appears in papers of Tarski. But in fact Tarski had considerable misgivings about using this notion in mathematics, as opposed to having it available as an informal notion of metamathematics. It doesn't appear at all in [50] where we would certainly expect to find it; and in [49] it appears only for the case where X is a single sentence (p. 710). As far as I know, free-wheeling mathematical use of the notion $E(X)$ is found first in Robinson's PhD thesis [38] (see his p. 36f), which is a passage that Robinson himself refers to in the letter quoted by Dauben. (See [22] end of §2 for some further discussion of this point. There may also be earlier mathematical arguments that explicitly use this notion in papers of Mal'tsev or the doctoral thesis of Henkin; I have not checked these in detail.)

In practice historical surveys always leave out something, and there are plenty of other things that Mostowski could have mentioned but didn't. I would just say here that his lack of interest in the applications of model theory to algebra and other disciplines of pure mathematics made it less likely that he would appreciate the significance of Robinson's use of Steinitz's theorem. Likewise a relative lack of interest in questions of conceptual analysis would make it less likely that he would appreciate the fine details of Tarski's views on metamathematical definitions. There is not the slightest reason to attribute either of these features of [34] to how Tarski 'trained his students' (as Robinson's letter suggests).

9. Mostowski's attributions in general

An unspoken implication of Robinson's complaint is that Mostowski attributed too much to Tarski. Mostowski does say that

> The systematic development of model theory was initiated by Tarski in the early fifties' ([34] p. 119) (10)

This is true in the sense that Tarski pointed the efforts of leading members of the Berkeley group into this area of logic during the 1950s. He did this partly by pressing some questions (e.g. can elementary equivalence be defined without reference to satisfaction?), and partly by giving basic definitions (e.g. truth in a model, elementary extension). Tarski himself proved very few mathematical results of model theory—far fewer than Robinson. But the one result that is regularly credited to him, namely the Łoś-Tarski theorem on formulas preserved in substructures, was proved in the early 1950s ([51], 1954), and it certainly set a trend.

On p. 121 of [34] Mostowski attributes to Tarski [49] (1950) the notions of submodel and extension. This is correct in the sense that Tarski included these notions in a list of basic notions in [49]. But the notions were already in wide use, not least in Garrett Birkhoff's notion of subalgebras ([3], 1935). Earlier than Tarski's paper, Robinson had given careful definitions of substructures and extensions of structures in his PhD thesis [38] pp. 65–8. But very likely Mostowski was unaware of the contents of [38].

Mostowski also attributes the notion of elementary equivalence to Tarski [49]. This is one place where Mostowski gives Tarski less credit than Tarski is entitled to: Tarski had already defined elementary equivalence in [46] (1936). Tarski implies on p. 283 of [52] that he had a 'correct and precise' definition of the notion as early as 1930. What Tarski claims in [49] is a mathematical (as opposed to metamathematical) definition of elementary equivalence; today this distinction is probably invisible to most logicians.

One other case is worth mentioning, because it shows Mostowski dealing with an attribution that was sensitive for some people. In [14] Henkin credited Mal'tsev [25] with giving the first completeness proof for first-order logic with arbitrarily many symbols. In Henkin's later paper [15] about the history of Henkin's own proof of this result, Mal'tsev doesn't even get a mention. What happened between these two papers of Henkin was that Henkin and Mostowski together wrote a review [16] of work of Mal'tsev, in which they commented on the proof of the completeness theorem in [25]. Mal'tsev uses the Skolem normal forms ϕ' of sentences ϕ, more precisely the normal forms that add new relation symbols. The two reviewers comment that Mal'tsev uses the fact that every model of ϕ expands to a model of ϕ'; Skolem himself claimed only that if ϕ has a model then so does ϕ'. They remark 'This stronger result does not seem to be formulated explicitly in the literature, although it can be discerned by a careful reading of the usual proofs of Skolem's theorem' ([16] p. 56f). For what it's worth, when I included this result of Skolem in my Model Theory text ([17] Theorem 2.6.5, p. 63) I included the stronger statement as part of the theorem; it never occurred to me that it wasn't obvious from the proof.

Gaps like this in proofs are an embarrassment. I think usually we size up whether we believe the author knows what he or she is doing, and if the answer is Yes then we credit the theorem—though we may still point out the gap. This is exactly how Mostowski handles the issue in [34]:

> The possibility of applying the completeness theorem to such problems was first pointed out by Malcev ... who also published the first proof of the theorem independent of the cardinality of [the set of symbols]. (Malcev's proof was not entirely correct but his mistake can easily be corrected.) ([34] p. 56) (11)

Mostowski's statement is faultless.

10. Conclusion

Andrzej Mostowski saw model theory mainly as a source of tools for answering questions raised in metamathematics, not as a source of new tools for other mathematical disciplines such as algebra. In this respect his view was quite different from those expressed by Robinson and Tarski in their 1950 manifestos.

As a corollary, Mostowski was insensitive to some features that were characteristic of model theory from 1950 onwards. These include the use of embeddings as a tool for analysing definability properties of structures. Tarski shared this insensitivity (cf. [22] §3(a)), and it set Mostowski and Tarski apart from Robinson who pioneered this use of embeddings.

But at the same time, Mostowski and Robinson were alike in that their work was driven by the need to find mathematical tools to solve certain problems, and not by any programme of conceptual analysis such as we find in Tarski. In fact Mostowski's use of the word 'model' shows little regard for Tarski's conceptual concerns.

Robinson saw the account of model theory in [34] as playing down Robinson's own contributions to the subject. There is truth in his criticism—though he exaggerated the point. But the main cause seems to have been Mostowski's own lack of interest in the more algebraic aspects of the subject. Where Mostowski had reason to take an interest in the historical details, he comes across as careful and scrupulously fair. Possibly he gave too much credit to Tarski; but if he did, he was certainly not the first or last student to exaggerate the achievements of his own doctoral supervisor.

Very few new mathematical disciplines are created by a single person, and certainly model theory was not one of them. But without Mostowski's contributions, model theory could easily have evolved in a different direction from the one that it took. As often happens in mathematics, some of his best ideas were adopted and used in ways that he could hardly have anticipated; he played a much more significant role than is suggested by his own modest account in [34].

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