The 1950 paradigm change

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1. The 1950 ICM

The conventional beginning of model theory:
International Congress of Mathematicians, Harvard 1950.

Alfred Tarski, 'Some notions and methods on the borderline of algebra and metamathematics', *Proceedings of the International Congress of Mathematicians, Cambridge, Mass. 1950.*

Abraham Robinson, 'On the application of symbolic logic to algebra', *Proceedings of the International Congress of Mathematicians, Cambridge, Mass. 1950.*

Alfred Tarski

Alfred Tarski

Abraham Robinson

Abraham Robinson

Two preliminary observations:

1. Tarski was born in 1901, PhD 1924, chair of Logic section of the ICM.
Robinson was born in 1918, PhD 1949. His contributed paper to the ICM was—very exceptionally—upgraded to invited paper.
The other invited speakers were Kleene and Skolem, and there were 16 contributed papers.

2. Neither paper mentions 'model theory'.
The name was proposed by Tarski in 1954
and adopted by Robinson at least by 1956.

Fast forward to June 1972. Gerald Sacks has given Robinson a copy of his book *Saturated Model Theory*. Robinson takes offence at a remark about Tarski in the book.
After brooding, he writes to Sacks (reported in Dauben *Abraham Robinson*, Princeton UP 1995, p. 450f:)

> '... The 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' [PhD, Birkbeck, London 1949] 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). ... 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. But it does upset me that *your* sense of fairness has not prevented you from perpetuating this myth.'

Robinson concludes by demanding that Sacks make a public retraction of the remark in his book.

Two corrections for the record:

First, Dauben says this letter was 'uncharacteristic'.
It was not. I myself was witness to two occasions in 1970 when Robinson responded in a similar way to people he claimed had disrespected him.

Second, Mostowski didn't put Robinson's entry into model theory in 1963. He put it in 1955—still too late.
Likewise the Fefermans in their book *Alfred Tarski*, CUP 2004 p. 224, seem to make Robinson's earliest contribution an application of work of Tarski in 1955.

2. 'Metamathematics', forerunner of model theory

Having established the human interest, we go to the maths.

'Padoa's Method', 3rd International Congress of Philosophy, Paris 1900. 'Logical introduction to any deductive theory'.

> In order to be able to assert that it is impossible to deduce, from the [axioms], a relation [defining x in terms of given undefined symbols], we ... [establish] an interpretation of the system of undefined symbols that verifies the system of [axioms], and that still does so if we suitably change the meaning of x only.

Padoa says this method is 'necessary and sufficient' for asserting the non-definability.
But his proofs of 'necessary' and 'sufficient' are exactly the same! They both prove sufficiency.
He could hardly prove necessity when he says nothing about the logic involved.
Did he have a precise logic anyway? Unclear.

Beth in 1953 proved 'necessary' just for first-order logic, using the completeness theorem and cut-free sequent proofs.

Padoa's argument couldn't conceivably count as mathematics, because *he proves nothing precise*.
At best it's a heuristic. It's a typical example of what Tarski used to call 'metamathematics' as opposed to 'mathematics'.
NB the title of his ICM 1950 paper.

Tarski around 1930 was trying to tidy up metamathematics and make it precise where possible.
For example he argued that Padoa's method must be made precise by removing 'interpretation' and converting the method into a proof-theoretic argument in type theory.
With Lindenbaum in 1926 he worked out some specific examples.

In 1953 Beth proved his definability theorem, using the completeness theorem to convert Padoa's criterion to a statement about cut-free proofs. He sent a copy to Tarski, who passed it to his student Feferman to discuss with Beth.
Feferman to Beth, June 1953:

> 'Your solution of the problem is really a solution of a problem in proof theory and only incidentally an application of [the completeness theorem]. Indeed, it seems to me that your main result has its proper phrasing as follows: If [...] is derivable [...]. I believe it is worthwhile putting the problem in this form, since then the difference between your problem and the problem of exhibiting models for independence of axioms is quite sharply pointed up.'

Feferman's words are pure 1930s Tarski, suggesting strongly that Tarski still took this syntactic view of Padoa.

Beth's paper was given to Craig to review for JSL.
Craig analysed Beth's use of cut-free proofs and extracted his Interpolation Theorem, 1957.
The now standard proof of Beth's theorem from the Interpolation Theorem was given by Craig.

In 1959 Lyndon (independent but in touch with Tarski's group) published a strong extension of the Interpolation Theorem that distinguishes positive from negative occurrences, still using cut-free proofs.

Lyndon 1959:

> 'Tarski has emphasized the desirability of establishing the Interpolation Theorem by methods independent of the theory of proof.'

This is a reversal of Tarski's view of the 1930s.
What are these 'methods independent of the theory of proof'?

One answer: In 1956 Robinson published a proof of Beth's Theorem by using *mappings between structures.*
When Craig's Interpolation Theorem appeared,
it was seen that Robinson's technique also gives it.

Here Tarski, and Lyndon quoting him, were ahead of the general attitudes in Tarski's group in the mid 1950s.
In that group, the idea of 'model-theoretic methods' took hold very slowly.
Model-theoretic methods tended to be viewed as less rigorous than proof-theoretic.

E.g. in Henkin's review of Robinson's PhD thesis, JSL 17 (1952) 205–207:

1. Henkin notes that Robinson's result, that the theory of algebraically closed fields of a fixed characteristic is complete, is among results that 'have been obtained earlier by others'.

2. Henkin claims that Robinson's technique for proving this result has also been 'obtained earlier by others'.
This is grossly false, as we will see.

3. Henkin criticises Robinson for using methods in which we assume that every element of a given structure is named by a constant.
He justifies this criticism by reference to Tarski's truth definition of 1936, which doesn't make this assumption.
(But it doesn't mention models either.)

I think 1, 2 and 3 are all reflections of the general attitudes in Tarski's group in the early 1950s.

3. Robinson's 1949 thesis

The thesis was published as *On the Metamathematics of Algebra*, North-Holland 1951, with only very minor alterations.
It was also the basis for his 1950 ICM talk.

A result in the thesis and in the talk:

> Theorem (p. 59). Any sentence of the first-order language of fields that is true in one algebraically closed field is true in all algebraically closed fields of the same characteristic. (My paraphrase.)

Proof: Let M and N be ACFs of characteristic χ, and $M \models \phi$.
By compactness, $M \equiv M'$ and $N \equiv N'$ for some ACF fields M', N' of infinite transcendence degree.
Let $\bar{a}$ list the elements of a countable a.c. subfield of M' of infinite transcendence degree, and put $T = Th(M', \bar{a})$.
Then T has a countable model M'' by Löwenheim's Theorem, and M'' is an ACF of characteristic χ satisfying ϕ.
Let N'' be constructed similarly from N'.
Then as fields, $M'' \cong N''$ by Steinitz's Theorem,
so $N'' \models \phi$ and thus $N \models \phi$. □

Observe:

1. Robinson's proof begs to be improved by using elementary embeddings. M is a model of $Th(N, \bar{a})$, where $\bar{a}$ lists the elements of N, if and only if N is elementarily embeddable in M. But elementary embeddings were first defined by Tarski and Vaught 1956.

2. Robinson's proof uses algebraic relationships between *pairs* of structures. Viz. any two ACFs of the same characteristic and transcendence degree are isomorphic, hence $\equiv$.
This was generalised by Vaught in 1954: If all models of T in some infinite cardinality are isomorphic then T is complete.
Robinson notes (1956) that his argument above 'essentially involves the application' of Vaught's result.

At these two points—both associated with Vaught—model theory learned from Robinson a change of direction.
Instead of analysing a single structure syntactically,
we can use mappings between structures.

Robinson represented a mapping $f : M \to N$ by introducing constants to label the elements a of M, and then using the same constants to label the image elements $f(a)$ of N.
('Method of diagrams')

This was not just a new idea. It overruled Tarski's systematic attempts to avoid using a symbol with two different references simultaneously. (Henkin's review was part of the backlash.)

Tarski's objections had a long prehistory, including Frege:

> 'If it were a matter of deceiving oneself and others, there would be no better means than ambiguous signs'.

In his 1948 textbook *Logyka Matematyczna*, strongly influenced by Tarski, Mostowski gives a truth definition where the clauses involve replacing the given relation symbols by higher order variables, so as to avoid interpreting the relation symbols in different ways in different structures.

Robinson probably never knew about Tarski's objections.
He learned his logic not from Tarski but from Carnap.

Rather than structures, Carnap had 'state-descriptions'.
These are maximal consistent sets of sentences T, with

$$\forall x\phi(x) \in T \quad \Leftrightarrow \quad \text{for every constant } c,\ \phi(c) \in T.$$

Carnap reached these by studying the 'possible states of affairs' in Wittgenstein's *Tractatus*.
The link to Robinson's diagrams is obvious.

Rudolf Carnap

Robert Vaught

In spite of encouraging the use of mappings between structures in the mid 1950s,
Tarski was never really reconciled to them.

In the mid 1930s he did some work with Mostowski on first-order theories of ordinals, in which he analysed the theory of a single ordinal at a time by 'elimination of quantifiers'—more syntax than model theory.

This work was lost during the war,
and reconstructed by Tarski and Doner in 1978.

By that date, model-theoretic methods were widely used.

Tarski commented:

> 'Several methods (usually semantical) other than [syntactic] quantifier-elimination have been developed which can be used for the same purposes and which often prove more efficient. ... Nevertheless ... it seems to us that the elimination of quantifiers ... provides us with direct and clear insight into both the syntactical structure and the semantical content of that theory—indeed, a more direct and clearer insight than the modern more powerful methods to which we referred above.'

So why is Tarski called the founder of model theory?

I won't answer that here, but there is a wide range of answers.
In his 1954 paper he gave the subject a name
(and a programme, though better programmes were soon discovered).
He provided important background results,
he analysed and removed his own earlier theoretical objections to model theory,
he was a charismatic leader of the extraordinarily powerful Berkeley group.
His own *mathematical* contributions to model theory were relatively weak.

4. Robinson in the 1950s, behind the maths

After a wartime career as a specialist in wing theory at the Cranfield College of Aeronautics,
Robinson decided in 1947 to go back to an old student interest and get himself a logic PhD at London University.

Within two years he had his thesis, including the result on ACFs. He submitted an abstract of a talk to the ICM, probably in early 1950.

Unknown to Robinson, Tarski had already published this result in an abstract of 1949, as a corollary of his quantifier elimination for real-closed fields, proved without any consideration of mappings.

Robinson found himself suddenly promoted to invited speaker (GOOD!), but he also learned that his best result was already known by a totally different approach that gave further results which he couldn't hope to reach by the methods in his thesis. (BAD! BAD!)

He also learned that in spite of Tarski's personal generosity to him, Tarski's group were unimpressed by results already proved by other methods. Nor did this group accept Robinson's methods until they had internalised them and forgotten that they learned them from him. (I do believe that the letter to Sacks was a longterm consequence of this.)

We can see Robinson's immediate reaction.
He worked furiously hard to find genuinely model-theoretic arguments to replace Tarski's syntactic proof of quantifier elimination for real-closed fields.

This is how we got the notions of model-complete, model-companion, the amalgamation criterion for elimination of quantifiers, and eventually model-theoretic forcing.

(Also maybe non-standard analysis, but as Carol Wood records, he 'wished that he would be recognized for something other than non-standard analysis'.)

Of course many other people contributed to the development of model-theoretic methods in this period.

Mal'tsev in Russia was perhaps the first hundred per cent model theorist,
earlier than Robinson but at first unknown in the West.

Fraïssé's use of partial isomorphisms was also a move towards respecting mappings.
Fraïssé made sure to keep in contact with Tarski's group.

Vaught seems to me central.
He put ideas of Robinson in a more fruitful form.
(He did the same service for Feferman around the same time.)

His construction of saturated models by repeated elementary extensions (found independently by Morley) established elementary maps as the basic morphisms of model theory, even for the study of single structures.
This gave the 'monster model' context in which stability theory usually takes place.