Model Theory

no. 2 vol. 3 2024

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Vol. 3, No. 2, 2024
https://doi.org/10.2140/mt.2024.3.203

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1. Model theory before Boris

To provide a context, let me say something about the state of model theory when Boris Zilber came into the field in the early 1970s.

An essay of C. C. Chang [1974] entitled "Model theory 1945–1971" should, at least from its title, be ideally suited to telling us what model theory was before Boris Zilber. Alfred Tarski [1954] had proposed the name "model theory" in 1954, on the basis of developments that had started to come together in the decade or so before; Chang's choice of 1945 makes a very reasonable start date. At the other end, Chang mentions some twenty papers dated 1972. This is still too early to include Boris; his earliest publication seems to be in 1974, though he himself cites unpublished papers of his from 1972 and 1973.

Chang divides up model theory into a few dozen "nodes", nearly all of which are either theorems or definitions. He draws a diagram to indicate which nodes were influenced by earlier nodes within model theory. Thus he has four "root" nodes which influence other nodes but are not themselves influenced by other nodes; for example the node "Löwenheim–Skolem–Tarski theorems". A fifth node "Omitting and realizing types (over a set A)" could have been counted as a root, but Chang sees it as influenced by several other nodes in complicated ways. This node is listed as influencing the following node among others:

(1) The notions of stability, rank, degree, finite cover property, etc.; categoricity theorems [Morley 1965; 1967; Baldwin and Lachlan 1971; Shelah 1971].

Chang doesn't reckon that this node (1) influenced any others. Probably if Chang had continued the diagram to include Boris, Boris would have been either in (1) or in a new node "influenced by" (1).

One major influence on Boris from this period is mentioned only indirectly by Chang; this is the collection of questions in circulation about theories categorical in some power. These questions include:

(2) If a countable first-order theory is λ-categorical for some uncountable λ, then must it be λ-categorical for every uncountable λ? (Łoś [1954] stated this as an

MSC2020: primary 03C45; secondary 03-03.
Keywords: history.

upward question plus a downward question. Morley [1965] proved it in both directions.)

(3) Is there a theory in a countable first-order language which is λ-categorical for every uncountable cardinal λ, but not ω-categorical, and is finitely axiomatisable? (Stated in the last section of [Morley 1965], but Morley says it is not his question. Mikhail Peretyat'kin proved an affirmative answer by methods very different from Boris's.)

(4) Is there a theory in a countable first-order language which is λ-categorical for every transfinite cardinal λ and is finitely axiomatisable? (I don't know who first stated this question, but it is a natural counterpart to (3).)

These and other questions of the time shared an important feature. The relevant first-order theories include those of some well-studied classical structures such as algebraically closed fields or vector spaces over finite fields. So it is natural to ask whether we can generalise from the classical structures to a class of structures defined by first-order theories. When Boris came into model theory, Morley had proved an affirmative answer to the question (2) by generalising transcendence rank to Morley rank, and Boris himself would later do something similar to prove a negative answer to (4). But Chang has no node that naturally covers arguments of this kind or the questions that generate them. For example he "exclude[s] from our consideration ... model theory applied to algebra, analysis, and set theory" (p. 173). With hindsight I think we have to say that this marked a blind spot in Chang's picture of model theory. But he was not alone in this.

In a recent online interview Boris puts a related point in his own words:

(5) The essence of model theory is an attempt — speaking in more general or philosophical terms — to interpret mathematics as a whole, analysing the language and the logic of it. ... You approach every mathematical area or problem, in number theory, in real or complex analysis, even in physics, and ask what is the adequate language and accordingly adequate formalism for this specific area. It might be that a specific problem requires a specific formalism. Then when you identify this formalism, you can approach it as a study of general patterns of formal theories. [Yeh 2018]

We will come back to this below.

Excuse me if I say a word about how I came into model theory. For my university education I signed up for a Hastings Rashdall scholarship at New College Oxford. This scholarship was intended to train future theologians by teaching them Latin, Greek, Greek and Roman history and some modern philosophy. The scholarship converted me to atheism, which didn't fit my proposed career. On the advice of the philosopher Gilbert Ryle (advice which I later learned had been crafted by my

philosophy tutor David Wiggins — let me thank them both for this) I applied to work for a doctorate in Literae Humaniores at Oxford, naming logic and with the intention of getting as far into maths as I could manage self-taught. There was some basis for this: for example, Richard Rado (who was a family friend) gave me some offprints on partition calculus. I joined John Crossley's recently formed research group in mathematical logic, and at an early meeting I met Saunders Mac Lane, who encouraged me to learn about vector spaces. The excellent lectures of two fellow students (John Bell and Alan Slomson) ensured I would call myself a model theorist, and I spent the rest of my career racing — with intermittent success — to catch up with the required mathematics.

2. Boris appears

I believe the first time I was aware of Boris was in March 1975. Paul Henrard had organised a week of model theory at Louvain-la-Neuve. Shelah was the main speaker, and he gave several lectures on "The lazy model-theoretician's guide to stability". They were written up by Greg Cherlin, Janos Makowsky, Alex Wilkie and me, and published as [Shelah 1977] in the volume [Henrard 1977]. On page 17, Shelah writes:

> There exist papers of Zilberg [sic] on $\aleph_1$-categoricity of rings, partially overlapping Cherlin and Reineke. ... [Added in Proof June 76: Zilberg [sic] also proved independently that $\aleph_1$-categorical division rings are fields.]

This was the first time I became aware of logicians in Russia who were working in categoricity theory. (Oxford in 1976 hosted some Russian logicians at a meeting on word problems in algebra [Adian et al. 1980], and several model theorists were present including Shelah and Yuri Gurevich. Boris was not and I don't recall that he was mentioned.)

Soon after Henrard's conference the British Broadcasting Corporation announced a series of lessons in Russian, and my wife Helen and I signed up to learn. This was partly in the expectation that I would soon meet "Zilberg" or at least some of his papers. (The first Russian sentence that the BBC taught us was *kran ne rabotayet* ("The tap or faucet doesn't work"); several Russian friends assured me that the sentence was absolutely true.) As soon as I could start to read Russian, I looked up the Russian mathematical journals available in the library at Queen Mary University of London, and found several issues of *Matematicheskiye Zametki* with papers by Boris or his colleague Oleg Belegradek. Queen Mary also had *Algebra i Logika* with papers by Palyutin, Erimbetov and others, though these papers were available in translation at other London libraries,

Quite soon manuscripts from Boris did arrive in the West. Apparently Boris heard of a grant that would allow him to visit Wrocław in Poland in 1979–80, and he took it up in order to finalise his work on the third question (4) above. His fullest account at that date [Zilber 1980] appeared in the Proceedings of a conference in Karpacz. Soon afterwards a Polish model theorist came through Boulder, where I happened to be visiting. (I am fairly sure this was Leszek Pacholski, who was the editor of the Karpacz Proceedings. My apologies if this is wrong.) He and I made copies of Boris's paper and sent them to a few dozen model theorists. Unfortunately in writing up Boris had missed a required condition on a polynomial, with the result that he had written a paper in which he allowed division by zero. Very soon both Cherlin and some people in Paris pointed out the gap. Alerted to this, Boris went back to what he had learned in Poland, and on that basis he wrote a short note [Zilber 1981] correcting the error. Meanwhile Cherlin and C. Mills had independently seen how to plug the gap by using the classification of finite simple groups, a point noticed later in the 1980s by several group theorists. Boris's corrected proof made no use of this classification.

The next paper of Boris that I saw was an essay that he had archived with the large Russian database VINITI in Kemerovo in 1977. This essay contained preparatory material for the paper [Zilber 1980] mentioned above. Cherlin had the essay and at the Logic Year in Jerusalem in 1981 he gave copies to some Russian-reading participants. I took a copy back to London, and for the next term I met my student Simon Thomas for coffee each Friday morning to dictate a translation of the essay to him. Simon took thorough notes and made an edited version of them. We sent a copy to Boris in Kemerovo. Later we learned that Boris sent it on to Sasha Borovik in Omsk with a note that by reading it Sasha could simultaneously find out what Boris was working on and learn some English. Unfortunately the top sheet naming Boris went missing, and thus it happened that the first published notice in Russian of Boris's work on groups with finite Morley rank was a summary by Sasha attributing the work to Simon Thomas. Another copy of Simon's writeup found its way to Ali Nesin and influenced his work on simple ω-stable groups. Meanwhile Simon himself rapidly completed an elegant doctoral thesis on classification of simple locally finite groups [Thomas 1983]. The thesis was strictly algebra and not model theory — it came to light that Gary Shute at Michigan State had independently reached the same results within algebra. But I think it is fair to cite Simon's thesis as an example of a convergence of interests between model theorists (as Boris) and specialists in algebraic groups (as Sasha).

Another of Boris's early papers that we in England translated into English was his [Zilber 1991], from a Russian original. David Evans was one of the translators, and it's worth a comment that David knew no Russian (at least at that date — he may have learned some since). But he had a good knowledge of the algebraic background

that Boris was assuming. This allowed him to reconstruct Boris's argument from my patchy English translation, and in several places to correct the translation to fit the mathematics. This impressed me as an example of mathematicians communicating through the mathematics itself rather than through a Russian or English text.

3. Meeting Boris in person

During the 1980s the iron curtain still divided Europe. East German logicians were increasingly frustrated at not being allowed any contact with their colleagues in the West. Then Ingo Dahn and Helmut Wolter in East Berlin, specialists in the model theory of fields with exponentiation, discovered a way in which East German logicians could host conferences to which non-German logicians were invited. Thus the Easter Conferences on Model Theory came into being; the first was in 1983 and the last in 1991. The German Democratic Republic ("East Germany" for short) ceased to exist on 3 October 1990, and henceforth there was just one Germany. Easter Conferences before 1991 were sometimes held in the conference centres of East German trade unions. But 1991 was different: we used a STASI training centre, where some of the STASI staff had been allowed to stay on as managers of the training centre, provided that they retrained as staff for the new uses of the building. Nothing to do with logic, but it was an extraordinary experience being served in the restaurant by scrupulously polite ex-STASI staff.

From 1983 onwards, the Easter Conferences had started to bring together logicians from both the Eastern and the Western blocs in Europe — excluding only the West Germans. The Russians were slow to join, but in 1986 Sergei Goncharov came from Siberia. In 1987 Boris came, with his colleague Oleg Belegradek. We met in those eerie halls below Friedrichstrasse Station, where travellers passed between East and West Germany under close inspection by the East German guards and their dogs. The conference went well and was the first of two Easter Conferences that Boris attended.

In the mid 1980s the group theorist Otto Kegel proposed to me that we should organise a Durham Symposium in *Model Theory and Groups*. Since Otto was based in Germany, Peter Neumann joined us as a second British organiser. The symposium took place on 18–28 July 1988 with seventy-five participants. Boris was the central speaker and he gave four lectures on "Finite homogeneous geometries 1–4". Another memorable lecture connected with Boris's work was the announcement by Ehud Hrushovski of his counterexamples to Boris's trichotomy conjecture, using a highly ingenious adaptation of Fraïssé's limit construction. From the discussion at the end of Ehud's lecture, and remarks of Boris elsewhere, I came away with the impression that Boris didn't really have a precise trichotomy conjecture. Rather his view was that some form of trichotomy was to be expected as a classical property,

and that on general principle it should follow that some natural abstract (in Boris's words, "logically perfect") conditions could be found under which trichotomy was provable. Ehud's result showed that the categoricity conditions that Boris had used so far were not sufficient. A few years later Ehud and Boris published their joint paper [Hrushovski and Zilber 1993] proving the trichotomy conjecture for "Zariski structures", which added an axiomatisation of a Zariski topology as a further condition. Other sufficient conditions have been found. Meanwhile Ehud's new construction has turned out to be extraordinarily versatile for generating interesting structures.

4. Boris in Kemerovo

In the 1990s Boris and his wife Tamara kindly invited me to their apartment in Kemerovo in Siberia. One of the first items to be explained here was Boris's telephone and its role in Russian history. When Boris Yeltsin was planning his coup, it was important that he could rely on the support of various groups, among them the coal miners in the Kuzbas coal fields, which formed the main industry supporting the town of Kemerovo. In order not to leak his plans to his political rivals, Yeltsin had to use contacts via private telephones. Tamara was a journalist with links to the Kuzbas miners, and so it happened that when Yeltsin was ready to move, Boris's telephone carried the message that Yeltsin could rely on the miners of Kuzbas.

One of the few facts about Kemerovo that did reach the British press was that a man in Kemerovo had killed several people and made them into meat pies which he sold at the Kemerovo railway station. Tamara confirmed to me that there was such a man, and told me that she had visited this man in prison in hopes of learning what had driven him to these actions. But she was too discreet to tell me what if anything she had learned from him.

Boris told me of one morning when his five-year-old son opened their front door and found a dead body on the stairs outside. Kemerovo was at times quite a disorderly town. Anti-Jewish attitudes were not uncommon in Kemerovo — or indeed in other places in Russia. Tamara told me of an occasion when she and Boris had been sitting several rows apart in a crowded bus. The woman sitting next to her launched into a fierce attack on Jews, and Tamara could see that the woman was staring at Boris as she spoke.

In the West we were broadly aware of this situation, and we tended to assume that Boris would want to come to the West as soon as he could. But at first it didn't happen. Boris explained that his father knew no Western languages at all; it would be unconscionable to abandon his father in Siberia, and cruel to bring him to a Western country where he couldn't communicate with anybody. But

then in 1999 Oxford University invited Boris to apply for the Professorship in Mathematical Logic in succession to Dana Scott and Angus Macintyre. I was told this professorship was first proposed by the Oxford philosophers, who wanted the teaching of logic in Oxford to be under the guidance of a professor who was expert in mathematical logic but also aware of the needs of philosophers in that area. The philosophy faculty was housed in Merton College, and accordingly Merton College had pride of place on the electoral board for the professorship. When Boris was elected, the warden of Merton College was the sinologist Dame Jessica Rawson; she conscientiously took her duties to include helping to bring over and settle Boris's father. No doubt there were other factors, but this was certainly one of them.

5. How mathematicians communicate?

Some events took place that I heard about partly from Oleg Belegradek and partly from Boris himself. The two accounts are compatible but interestingly different. Oleg told me that Russian universities have a set of necessary and sufficient conditions for a lecturer to be raised to the rank of professor. One of these was that the person concerned must have published a book in the relevant discipline. This was the only condition that Boris failed to satisfy. So Oleg said to Boris "Let me write up for publication the notes of the course that you teach" (naming a course), "and we can arrange to get them published".

The next part I heard from Boris sometime later. Boris told me that he had accepted Oleg's offer and Oleg had given him the write-up for him to check. But in Boris's view, Oleg's teaching style was too formalistic and included an unhelpful amount of detail. So Boris went through Oleg's volume and struck out with a red pen maybe a third of the text, adding nothing.

I heard the next step from Oleg. Boris had returned the write-up to Oleg with extensive deletions marked in red pen. After looking over the deletions Oleg had decided to ignore them, and he sent to the printer a copy of the volume as it was before Boris's deletions. All went well and Boris became professor.

There is a point in telling this story. It's agreed that different mathematicians can have widely different strategies for constructing proofs. For example some mathematicians are happiest if they can construct the proof like a logical deduction, adding line by line to what has already been deduced, until the required theorem emerges as the last line. Others prefer to construct the whole proof in a vague or intuitive form and then fill in the details. (If I understood him correctly, Saharon Shelah once told me that in his experience the first sort of mathematician is unlikely to make mistakes, but the first sort is also more likely to meet questions which he or she will never be able to answer.) Obviously there are other dichotomies between different styles of mathematical thinking.

Boris once told me that there was a first time when he knew that another logician understood what he (Boris) was trying to do. This was when Ehud Hrushovski sent him a preprint in which Ehud cited Bézout's theorem in a context not obviously within algebraic geometry. Boris had allowed Bézout's theorem to guide his thinking in a similar context, but he didn't suppose other logicians would understand this and so he hid Bézout's theorem behind an argument with Morley rank.

How does this relate to Boris's remark quoted at (5) above? Both Boris and Ehud were opening up a new area of research which borrowed a picture (though not apparently formal details) from Bézout's theorem. Were they both asking, from their own points of view, what is "the adequate language and accordingly adequate formalism for this specific area"? I leave this as a question, for fear of fabricating what Boris and Ehud were thinking.

But in any case I came to realise that my gappy mathematical education would never equip me to keep up with recent developments in model theory. So as retirement came into view I chose to move across to the history of logic in Arabic in the middle ages, particularly the logic of Ibn Sīnā (Avicenna); this was connected more directly to my undergraduate training. After working for a couple of decades in this historical field, it strikes me as uncanny how many Boris-like features Ibn Sīnā's logical thinking had. One was the constant pressure to identify and codify features of logical thinking that had not previously been noticed. Another was Ibn Sīnā's decision, apparently in his late teens, to abandon large parts of Aristotle's modal logic and replace them by a new logic based on an "adequate language and accordingly adequate formalism for this specific area". Incidentally both Boris and Ibn Sīnā were born in Uzbekistan (Boris in Tashkent, Ibn Sīnā near Bukhara).

Time rolls on and I travel less. I might not see Boris again. But Helen and I were hugely pleased to meet Boris and Tamara again at the conference *Logical Perspectives* 2018 in St Petersburg. Boris is one of those valuable individuals who enrich not only the lives of the people they meet, but also in a more abstract way the world itself and its culture.

Acknowledgments

It should be obvious that I am in debt to dozens of model theorists for the contents of this paper. I particularly thank the editors of this volume, the referees of the paper and Boris himself for their helpful comments and information.

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Received 24 Jan 2023. Revised 22 Mar 2023.

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Model Theory

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Model Theory (ISSN 2832-904X electronic, 2832-9058 printed) at Mathematical Sciences Publishers, 798 Evans Hall #3840, c/o University of California, Berkeley, CA 94720-3840 is published continuously online.

MT peer review and production are managed by EditFlow® from MSP.

PUBLISHED BY
mathematical sciences publishers
nonprofit scientific publishing
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Model Theory

no. 2 vol. 3 2024

Special issue on the occasion of the 75th birthday of
Boris Zilber