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MODEL THEORY: GEOMETRICAL AND SET-THEORETIC ASPECTS AND PROSPECTS

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In Memory of Maurice Boffa

§1. Introduction. I see model theory as becoming increasingly detached from set theory, and the Tarskian notion of set-theoretic model being no longer central to model theory. In much of modern mathematics, the set-theoretic component is of minor interest, and basic notions are geometric or category-theoretic. In algebraic geometry, schemes or algebraic spaces are the basic notions, with the older "sets of points in affine or projective space" no more than restrictive special cases. The basic notions may be given sheaf-theoretically, or functorially. To understand in depth the historically important affine cases, one does best to work with more general schemes. The resulting relativization and "transfer of structure" is incomparably more flexible and powerful than anything yet known in "set-theoretic model theory".

It seems to me now uncontroversial to see the fine structure of definitions as becoming the central concern of model theory, to the extent that one can easily imagine the subject being called "Definability Theory" in the near future.

§2. Tarskian beginnings.

2.1. Tarski's set-theoretic foundational formulations are still favoured by the majority of model-theorists, and evolution towards a more suggestive language has been perplexingly slow. None of the main texts uses in any nontrivial way the language of category theory, far less sheaf theory or topos theory. Given that the most notable interactions of model theory with geometry are in areas of geometry where the language of sheaves is almost indispensable (to the geometers), this is a curious situation, and I find it hard to imagine that it will not change soon, and rapidly.

2.2. In Tarski's foundational scheme, everything happens within a (the?) universe of set theory. All the entities of the subject (semantic and syntactic) are sets (or, now and then, classes). **Structures** are defined as (rather

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gruesome) tuples of relations, functions and points living on a set. Once one has fixed what configuration of arities, etc, one wishes to consider, one has an associated syntactical apparatus for that configuration The main "syntactic" notion is that of **formula** (perhaps infinitary). Structures and formulas are related by **satisfaction**, the basic notion being: $M \models \Phi(\bar{a})$, M a structure, $\Phi(\bar{v})$ a formula, and $\bar{a}$ a tuple from M. This notion is set-theoretically definable, uniformly for structures and formulas. (Here the restriction of structures to sets is relevant.) The approach covers various "semantics", such as first-order, infinitary, higher-order, Boolean-valued, Heyting-valued, and real-valued.

2.3. While it is simply true that most "structures"of ordinary mathematics can be construed as Tarskian structures, few modeltheorists can have failed to notice how unappealing the formulation is to other mathematicians. Tarski clearly felt the need for a rigorous definition of truth, but I do not believe that such formal rigour was ever a sine qua non for the development of a mathematical theory of models. There are parts of model theory, notably the monumental (model theory of) set theory, and the more ensembliste parts of the model theory of arithmetic, where truth definitions (both Tarskian, and those based on the technology of indiscernibles) are truly indispensable. However, in those parts of model theory with more relevance for algebra and geometry, the set-theoretical, "rigorous" formulations seems to me to have given practically nothing, and arguably to be currently inhibiting. Let me say at the outset that this cannot reasonably be taken to suggest that the Tarskian tradition has contributed little to the current repertoire of "applied model theory". Tarski's Limit Theorem, ultraproducts, indiscernibles, and Shelah's profound contributions to definability theory (detached from their ensembliste setting), are all ideas evolving from the set-theoretic tradition, and now constituting much of what every modeltheorist must know. But there are other ideas of Tarski, equally fertile, and not at all ensembliste, namely those around quantifier-elimination. As I will observe below, there is a puzzle as to why Tarski did not take these ideas further.

2.4. Aside from the brutal formalism, there were various other (minor, but instructive) defects in early formulations. Structures could not be too large, and in particular the notion of functor did not easily fit (there is now a need for a "natural" fit). For no good reason, natural structures were forced into a one-sorted formulation (and by now it is a considerable nuisance that there is no basic model-theory text in a many-sorted setting). This has an instructive ideological effect. When the needs of his research forced Shelah to many-sorted formulations, to accommodate imaginaries, this was (surprisingly) regarded as noteworthy. Algebraic geometers had much earlier come to terms with their definable equivalence relations (in the context of algebraic spaces). From the perspective of the traditionally powerful strategy of getting to the essential structure behind the presentations, and the associated Kleinian ideology of the importance of invariance under symmetries, nothing could be more natural than the passage to M^{eq}.

Some of Tarski's early moves were well ahead of their time. He attached to structures their theories, syntactic entities, and made (various collections of) theories into spaces via what is essentially the spectral topology of algebraic geometry. Gödel's Finiteness Theorem (Completeness Theorem) becomes the Compactness Theorem for these spaces, a still suggestive reformulation. However, nearly thirty years passed before Morley spelled out the functorial picture of the various type spaces (also spectral) associated to theories. This sheaf picture did not appeal to many modeltheorists even in the 1960's. It is to be noted that much of model theory can be formulated in terms of these type spaces, and/or their Boolean algebra duals, without even bringing in many **models** (just as the points aspect of schemes, in its old-fashioned set-theoretic sense, is only one of many aspects of modern algebraic geometry). One point I wish to make in this speculative paper is that the Tarskian semantics may easily lose its dominance in model theory.

2.5. Tarski himself, in the 1950's, contributed to a more palatable version of model-theory. The work of his school, and later Shelah, allowed one to **define** "syntactical" notions, such as elementary equivalence, in terms of the (functorial) notion of ultraproduct (which is definable in category-theoretic terms, close to those in Grothendieck's definition of Spec). Apart from a few distinguished exceptions, the subject was relentlessly setified, and thus, as far as being suggestive to the outside world, stultified.

But really Tarski's main contributions, though usually guarded by the formalism of set-theory, owe little to set theory. Elementary maps are clearly the right notion and his simple Direct Limit Theorem has a Grothendieckian inevitability about it.

2.6. The Tarskian emphasis, enormously expanded by Shelah (but now somewhat neglected), makes no one first-order L, or L-theory Σ, privileged. What matters most is classification within $Mod(\Sigma)$, the class of models of Σ, and then via cardinal invariants.

There are, in fact, very few isomorphism theorems in real mathematics that depend only on cardinal invariants. A useful contribution of post-Morley model theory is to explain these extreme classifications in terms of a geometrical independence theory (ultimately of much greater importance – no one could reasonably argue that uncountable-categoricity is the fundamental fact about algebraically closed fields). From these explanations one does understand why there are so few extreme classifications in algebra, and one understands some absolutely new things, for example that there are no such extreme classifications in ordered algebra.

The relevance of the set-theoretic emphasis is much less clear in later developments (say, post 1980). The later highlights have little to do with the diversity (or otherwise) of models, but rather the fine structure of definitions.

2.7. What is the legacy of the Tarskian development?

2.7.1. The general versions of Cantor's back-and-forth method remain useful, though nowadays they are rarely tied to cardinality considerations. Criteria for extendibility of maps, or characterizations of certain kinds of definability in terms of extension of maps (particularly Shoenfield's Criterion) are part of the basic repertoire of the subject.

2.7.2. The Omitting Types Theorem (essentially the Baire Category Theorem in a certain type space) is normally construed as a method for constructing models more complex than those provided by the basic Compactness Theorem. The right setting is surely $L_{\omega_1,\omega}$ (or its fragments). The method has many presentations (forcing, consistency properties). There is no reason to think that the resources of the simple ideas involved have been exhausted. Hrushovski's subtle variant, involving a predimension constraint, has already become a standard tool, and a startling insight of Zilber relates Hrushovski's "forcing" to Schanuel's Conjecture, the theory of the complex exponential, and diophantine geometry.

2.7.3. Saturated and atomic models (and various associated relativized notions) remain fundamental, some 45 years after their first appearance. Saturation is a general model-theoretic version of the algebraic geometer's universal domain. Though the models in question are rarely explicitly constructible, their utility is beyond doubt, not least in that one often gets simple proofs, via saturation, of nontrivial theorems about standard objects. One manifestation is in bounds in algebraic geometry, where nice results have appeared sporadically for over forty years.

2.7.4. Historically, saturation, ultraproducts and automorphisms were studied together. How much saturation an ultraproduct has is a natural and difficult set-theoretic question, not, however, one fertile for geometric applications. That nonprincipal ultraproducts are (normally) saturated over countable subsets, combined with all the functorialities of the ultraproduct construction, is usually exactly what one needs in applications. That saturated models have enough automorphisms to support a primitive Galois theory remains an attractive and useful fact. That one cannot just read off automorphisms in ultraproducts is perhaps less well-known. Iteration makes them visible, but this has never been useful outside of set theory or models of arithmetic.

2.7.5. The isolation, by Ehrenfeucht and Mostowski, of the notion of order-indiscernibles (and the underlying Ramsey technology), has always seemed to me a key event in the subject. Clearly the basic result about automorphisms remains isolated, but other uses of the idea have driven the subject. E-M models are of basic importance in Classification Theory, though very rarely in applications. But serious combinatorics of independence started here, and would dominate the pure side of the subject for 35 years, eventually reaching a sophistication adequate for major applications.

I want to stress that the indiscernibles per se are important only for calculation (or representation) in connection with various notions of dependence. (The same can fairly be said in algebraic geometry.)

2.7.6. A very remarkable example of the impact of set theory on the finite combinatorics of model theory is Shelah's work on not having the Independence Property, which has turned out to be a version of finiteness of Vapnik-Chervonenkis dimension [20]. Shelah's original proof that not having IP in dimension one (for a given theory) implies not having IP in all dimensions, gave a result apparently beyond the means of the probabilists who looked at the notion in geometrical contexts. Shelah's methods give no bounds at all, Laskowski gave astronomical Ramsey bounds, and later in specific (and most interesting) geometrical situations a mixture of o-minimal model theory and differential topology gave optimal (and useful) bounds [16].

The point is that in the hands of a master like Shelah, set-theoretic methods reveal finite combinatorial methods of great flexibility, but that different ideas, more adapted to algebra/geometry, are needed to make these tools give convincing applications.

2.7.7. The model theories of set theory and arithmetic remain in the Tarskian foundational tradition. In the case of the former this seems natural. In the case of the latter one hopes that someday serious diophantine issues will be confronted, but the time is surely not ripe.

Both these model theories are involved (and the rest of model theory is not) with notions of relative consistency. Typically, one constructs models to resolve problems of relative consistency (in set theory or computational complexity). There are exceedingly deep results and flexible methods. In both settings model theory should not be separated from recursion-theoretic definability (descriptive set theory in one case, subrecursive hierarchies in the other), giving these subjects a dimension missing in geometric model theory.

2.7.8. To summarize, the legacy of the set-theoretic development is rich and obvious in the model theory of set theory and arithmetic. For the rest of model theory, many of the fine combinatorial ideas were distilled from set theory, and have become more geometrical. I have no sense at all as to how set theory may again influence the geometry.

§3. The early Tarski (definability theory).

3.1. With his work on a decision method for elementary algebra and geometry, Tarski initiated a quite different development, which still flourishes and owes very little to the set-theoretic development.

Here the basic object of study is the real ordered field. The formal language takes $+, -, 0, 1, \cdot, <$ as primitive, and (at least from a modern perspective) one's central concern is the structure of (parametrically) definable sets.

Other models of $Th(\mathbb{R})$ are not of special interest at the outset. Tarski himself showed that $\mathbb{R}$ and the field of real algebraic numbers are elementarily equivalent (so, for example, π is not first-order definable), but only much later in the work of the Robinson school were other nonstandard models of real significance.

3.2. There is a basic stock of definable sets, S, defined using polynomials over $\mathbb{R}$, namely: Zero-sets, positivity sets, negativity sets, all sets in $\mathbb{R}^n$ defined from $f \in \mathbb{R}[x_1, \dots x_n]$.

Tarski did not address the issue (much later of basic importance for geometers and computer scientists) of the topological structure of such sets (except in dimension one, where it is trivial). In some ways this is odd, given his close links to outstanding topologists. And it is a missed opportunity, for Tarski's elementary methods are powerful enough to prove such basic Finiteness Theorems as Whitney's, that sets in S have finitely many connected components (with uniformity in families).

Tarski's main concern was the effect of the logical operations of $\wedge, \vee, \neg$ and $\exists$. The latter is crucial, and is best viewed geometrically as projection. (An aside: the rather natural notion of cylindric algebra has never been popular with model theorists. What was missing?)

From S one builds other sets, via repeated applications of

(A) Boolean operations;
(B) (A) and projections.

Using (A) alone one gets the semi-algebraic sets, and (via their graphs) the semi-algebraic functions. There is an extensive and beautiful literature on the geometry and topology of such sets (with the same Finiteness Theorems as for S). The dominant fact is Tarski's result: (B) is redundant, that is, the class of semi-algebraic sets is closed under projection, or, maybe, the semi-algebraic category has direct images.

Projection is a very powerful operation. In arithmetic, or other "Gödelian" settings, it leads rapidly from the intelligible to the unintelligible. In semi-algebraic geometry, in contrast, we have closure under all the natural topological constructions. It is popular nowadays to see the semi-algebraic universe as the first of the "tame" universes for geometry. The basic geometrical/topological structure of the sets and functions is intelligible and the fundamental geometrical/topological constructions may be carried out in this small universe. There is none of the pathology of set-theoretic topology. Here all sets are locally closed, and positive measure coincides with having nonempty interior. Maps are $\mathbb{C}^k$, with $k > 1$, off sets of measure 0.

Tarski stressed the (constructive nature of the) quantifier-elimination, and the characterization of definable sets in $\mathbb{R}^1$ as those with finite boundary (i.e., finite unions of intervals in the most general sense). No attempt was every made to characterize definable sets in $\mathbb{R}^k$ for $k \geq 1$ (far less to study definability on real algebraic manifolds). Why not?

Well over forty years were to pass before van den Dries realized that on the basis of Tarski's result for $\mathbb{R}^1$ one could axiomatically derive Finiteness Theorems in $\mathbb{R}^k$ for all k. He was certainly inspired by the work of Lojasiewicz and Gabrielov on subanalytic subsets of $\mathbb{R}^k$, and realized that their inductive arguments by fibering were of potentially great generality. (The definitive treatment of the inherent uniformities is given by [31]). Note that aside from the semi-algebraic case the only known example of an o-minimal category (as these universes would be called) came from the subanalytic subsets of $\mathbb{P}^k(\mathbb{R})$ for varying k. But already people had reason to hope that the universe of sets in the $\mathbb{R}^k$, definable from semi-algebraics and the graph of the global exponential, would be o-minimal (with as consequence that it was not Gödelian, as $\mathbb{Z}$ has infinitely many connected components). And so it turned out, in Wilkie's landmark result of 1991 [36].

I want to stress that here little model **theory** was involved, and essentially nothing from the set-theoretic tradition. Here one learned from examples outside logic, from real analytic geometry, an appropriate discretion in the choice of geometrical axioms. My impression is that there are many who think this is not model theory at all. But the depth of understanding achieved in real geometry by these methods is comparable to that achieved via high theory from the stability tradition. Nonstandard models are sometimes useful, mainly for obtaining uniformities in Finiteness Theorems. But elaborate methods of model construction have proved irrelevant till now.

Van den Dries' insights are certainly close to those of Grothendieck in [9], though my feeling is that the potential of [9] is far from exhausted. According to [29] Grothendieck was known to Bourbaki as "logical". For my taste, he is unrivalled in terms of ability to select notions, axioms and theorems of maximum potential. The beginning quotation in Deligne's [5], and Deligne's corroboration, witness what I am hinting at. This is a kind of "atomic" model theory, where set theory is again largely irrelevant.

3.3. By finding a "right" level of generality, the basic theory of o-minimality helped enormously in the discovery of deeper Finiteness Theorems. An o-minimal combination of the subanalytic universe with the global exponential function confirmed its interest by a major application in Lie theory (to characteristic classes of contructible sheaves). The same theory has a natural "algebraic" nonstandard model which is now a powerful tool in asymptotics [34]. Wilkie [37], in a second tour-de-force found a new axiomatic method for establishing o-minimality from Khovanski-style Finiteness Theorems, thereby proving o-minimality of the Pfaffian universe.

3.4. Another missed opportunity. The notion of VC-dimension was discovered around 1971 [20] and has proved fundamental in mathematical learning theory. Examples were at first very hard to come by, except in the (o-minimal) universe of linearly definable sets. Thus it is truly remarkable that in any o-minimal theory (and there are many of great geometric complexity) all definable families of definable sets have finite VC-dimension. Set theory is not needed here, though it led to the first insights [20]. But the internal geometry of o-minimal theories leads to the best results, including some, on density, quite unknown to the probabilists [17]. But it should not be forgotten then even in the semi-algebraic case the result is not trivial, but can be deduced from Tarski's Theorem by **elementary methods.**

3.5. Decidability vs definability. Tarski's primary purpose was surely to give a decision procedure for elementary algebra and geometry. Nowadays, while it it technically interesting (and typically rich in spinoffs) to prove decidability for some classical structure, this is no longer an end in itself. Rather one wants to find constraints on, or normal forms for, definable relations in the structure. This has been achieved for many basic structures since 1950, never by essential use of set-theoretic methods.

In o-minimal structures, the ring $\mathbb{Z}$ is known not to be interpretable, thus showing that o-minimal structures are not Gödelian. In contrast, in number theory simple definitions may lead to incomprehensible sets (e.g., in the ring $\mathbb{Z}$ projections of quantifier-free definable sets are exactly the recursively enumerable sets, the strong negative solution of Hilbert's 10th Problem). In $\mathbb{Q}$ one does not know any nontrivial constraint on existentially definable sets, but Π^0_3 sets are already incomprehensible.

3.6. Other early Tarskian themes of an algebraic nature. Tarski had many ideas which link to ideas now current, 60 years on.

The Tarski-Chevalley Direct Image Theorem, that projections of constructible sets in algebraically closed fields are constructible, is surely of permanent importance. (It is **much** easier than his theorem for $\mathbb{R}$). Nowadays it occurs in advanced texts in commutative algebra [26], in a generalized scheme version, with a proof using induction on dimension. It is to be noted that a rearrangement of this proof occurs in the landmark Hrushovski-Zilber paper [15], there to derive ω-stability from natural axioms including a Direct Image Theorem (and thereby initiating the model theory of complex analytic manifolds).

Tarski was the first to point out a rigorous derivation of a Lefschetz Principle by means of his quantifier-elimination for algebraically closed fields. Nowadays there is much life left in Lefschetz Principles for other languages and structures.

He stressed that the real algebraic numbers are exactly the absolutely definable elements of the real field, and constitute the "prime" or "atomic" model of the theory.

General theory shows that o-minimal theories have prime models, but only in rare cases have these been explicitly characterized. The identification of suitably saturated models has been done too in some important cases, using generalized power series over groups defined using Hausdorff's η_α-sets, and this representation has been suggestive.

§4. Definability in "core" structures.

4.1. The rings $\mathbb{Z}, \mathbb{Q}, \mathbb{Q}(x)$ are Gödelian, and their logical definability theory is essentially recursion-theoretic.

4.2. In contrast, Robinson took up the definability aspect of Tarski's work, and contributed some fundamental generalizations of quantifier-elimination. Over a long period he and his followers, often using very primitive methods, detected patterns of definability in many core structures of geometrical significance. I think the key notion is always that of a Direct Image Theorem (and it seems that it is only lately that we modeltheorists discovered the existence of deep examples of such theorems, for example Remmert's, out in the world of geometry). The pattern is always to identify a category of definable sets and functions, with sufficiently intelligible structure that one may bring standard geometric techniques to bear, and such that one has a Direct Limit Theorem for the category. The basic category should, loosely, be "tame", i.e., devoid of the kinds of pathology that unrestricted set theory brings. Among the obviously basic structures for which one has a viable Direct Image Theorem are:

(1) Various real analytic situations;
(2) p-adic and rigid analytic structures, and Witt vectors with Frobenius;
(3) the ring of algebraic integers;
(4) many Henselian fields, including power series fields.

(1) includes Gabrielov's famous theorem of the complement for subanalytic subsets of projective space. The Direct Image result is basically the classical result that proper images of real subanalytic sets are subanalytic. A very different (series of) example(s) is provided by Wilkie's model completeness for the real exponential. Here the basic setting is as for Tarski except that one considers, instead of merely polynomials, (iterated) exponential polynomials. One **does not** have the naive analogue of Tarski's Theorem. The basic category is not that of quantifier-free definable sets and maps. Rather the basic sets are the projections of quantifier-free definable sets, and the basic result is now closure under complement. In fact one does not need arbitrary images of quantifier-free definable sets, but only étale ones.

There are basic examples relating to (1) where one has something very constructive but not a Direct Image Theorem. The most interesting example is the Pfaffian category, shown by Wilkie to be o-minimal.

(2) Here the starting result analogous to Tarski's is that one needs only the direct images under the (étale) power maps. Note that Tarski's original theorem falls into this pattern.

Already elaborations of this yield important information on p-adic Poincaré series. Later one was able to develop a p-adic analogue of the subanalytic theory [7], yielding important consequences in p-adic group theory, and even later one was able, in the face of major difficulties, to derive a Direct Image Theorem in the rigid cases, using existing deep Direct Image Theorems.

The Witt vector case, recently dealt with, subsumes the original 1976 result. Here one needs no pre-existing theorems. The model-theoretic patterns revealed since 1964 provide sufficient guide. One uses nonstandard models (via, as always, Shoenfield's Criterion). The main task is to find **axioms** permitting an elimination.

In other cases, for example Wilkie's [36], it required substantial work [24] to figure out what axioms one was using. The bonus is a decidability result. In other cases, notably the p-adic exponential, one does not know what axioms one has used, though one has a Direct Image Theorem of Wilkie style.

(3) is currently a result of sporadic type. There seems considerable scope for sharpening the presentation in [32], converting the quantifier-elimination to something more like a Direct Image Theorem, and for emphasizing, at the level of local-global, or sheaf-theoretic principles, the link between local and global definitions (à la Feferman-Vaught).

(4) This development began with the work of Ax-Kochen-Ersov, beginning the model-theoretic analogy between the p-adics and the reals. Hensel's Lemma turned out to be the analogue of the signchange property, and this insight opened a whole new area relating definitions on Henselian fields to those in their value groups and residue fields. The ideas involved turned out to be very relevant for the study of real exponentiation, where a striking Direct Limit Theorem was obtained in which logarithms are central [33].

4.3. Despite the beauty and clarity of [35], I feel that the model theory of analytic functions is somehow defective, in that it has no "local" formalism. It works perfectly well for compact manifolds (by crude patching), but there really ought to be a thoroughgoing sheaf semantics at work. In a similar spirit, one expects the needs of current work to force a new foundation of model theory, to support invariant formulations, and in particular to let us pass the stage when definable sets are given in terms of a redundant affine embedding.

4.4. The overemphasis on decidability had the effect of deterring any serious study of the model theory of the global complex exponential (which Tarski knew to be undecidable). The decidability of the real exponential is bound up with (the real case of) Schanuel's Conjecture in transcendental number theory [24]. Intriguingly, Schanuel's Conjecture is bound up with the fine detail of definability for the complex exponential [38]. If one uses Hrushovski forcing "relative to Schanuel's Conjecture" one obtains a very natural "Robinsonian" theory of exponentiation, which may agree with the actual theory of exponentiation. One is operating here just outside first-order model theory, but in a realm that seems to me more category-theoretic than set-theoretic. The model theory of the complex exponential is ripe with problems and links to diophantine geometry. It is no doubt the prime example in geometry where decidability is out the question (it is Gödelian) but a geometrical definability theory may exist.

§5. New theories.

5.1. Robinson's emphasis on model completeness has led, over the years, to the discovery of certain **theories**, of basic current importance, but not having any "natural" models. (The theory of exponentiation mentioned in 4.4 may turn out to be one such.)

General nonsense gives the existence of existentially closed models of universal theories, but only rarely is the class of existentially closed models first-order axiomatizable. The core structures given in 4 almost always turn out to be existentially closed for a suitable formalism, and a crucial step in understanding them is to find the axioms expressing this (sign change, Henselian, ...).

The first case of an interesting **theory** got this way is differentially closed fields. The class of (even natural) differential fields is Gödelian, but the class of differentially closed fields is elementary (there are several possibilities in finite characteristic, linked to the theory of separably closed fields), and has a Direct Image Theorem (easy in characteristic 0). The definability theory is in a natural way an elaboration of that for algebraically closed fields. It seems fair to say that DCF, as studied by Robinson, contributes little to the study of ordinary differential equations. However, once it was realized that DCF is stable (ω-stable in characteristic 0), nontrivial stability theory did allow the development of a theory of closures of definite interest to differential algebraists. But the real interest of DCF came with Buium's use of an essentially arbitrary model of DCF_0 in connection with the Mordell-Lang Problem in diophantine geometry, followed by Hrushovski's brilliant use of deep geometrical model theory of separably closed fields (or a variant of DCF_p) to go beyond the algebraic geometers in the characteristic p case of Mordell-Lang [10]. I stress that it is the technology (here very advanced) of definability theory that matters, not at all any special model or set-theoretic construction.

Another example, discovered surprisingly late [22], is ACFA, the theory of an existentially closed difference field (equivalently field with automorphism, in this case). The theory was discovered because of an insight that it might have to do with Frobenius morphisms, as p varies (and so it turned out [14], [23]). The most striking feature of the axiomatization is that the axiom allowing the Direct Image Theorem has the shape of diagrams of Weil in the theory of varieties over finite fields.

The theory is unstable, but a penetrating analysis of definitions in terms of geometric model theory [3] leads to yet another contribution to diophantine geometry, this time to the Manin-Mumford Conjecture [13]. Again, no specific model is needed, but much technology from definability theory. The new theory comes from that of algebraically closed fields by a procedure analogous to that for differential fields. One may ask if there are other possibilities. Yes, but they are few [2], and the work on Witt vectors involves the only other case.

5.2. Nonstandard finite fields, and Frobenius. Tarski had raised the problem of the decidability of the theory of finite fields (and there is evidence he thought the answer would be negative). Ax [1] not only proved decidability but (much more importantly) characterized exactly, by geometric and Galois-theoretic axioms, the infinite ultraproducts K of finite fields. The Galois-theoretic condition is that $Gal(K) \cong \hat{\mathbb{Z}}$, i.e., K has exactly one extension of each degree (and K is perfect), and of course implies that K is not algebraically closed. But, for the K in question, only just. From one of the great achievements of diophantine geometry, Weil's Riemann Hypothesis for curves over finite fields (precisely, from the Lang-Weil estimates), it follows that every absolutely irreducible variety over K has a K-valued point. This is an elementary condition. The miraculous result is that the conditions now displayed give exactly the **theories** of all possible K.

While it is true that the K are scarcely visible without some (actually minimal) ultraproduct technology, the K occur in nature in the sense that $Fix(\sigma)$, for σ a generic element of $Aut(\mathbb{Q}^{alg})$ is a model of Ax's axiom (a "pseudofinite" field).

The theory of pseudofinite fields has a Direct Image Theorem, formally very similar to those for $\mathbb{R}$ and $\mathbb{Q}_p$, and most suggestively presented in the Galois stratification of [8]. Here the basic entities are not quite classical formulas, rather such formulas with a Galois flavour. Moreover, all this fits the model completion picture. One is completing (theories of) procyclic fields, with only regular extensions considered (Ersov uses the notation "regularly closed").

Much later, when ACFA was discovered, Hrushovski and I undertook the difficult task of lifting Ax's analysis from the $\mathbb{F}_q$ to $(\mathbb{F}_q^{alg}, x \mapsto x^q)$, and showing that one gets exactly ACFA. This requires very serious diophantine geometry [14, 23]. A beautiful spin-off is Hrushovski's [12] application to a problem of Jacobi in difference algebra.

5.3. In none of the above examples are specific models of much interest. What is fertile is to understand the definability theory. One uses geometric model theory to pass to applications in geometry.

§6. Geometric model theory.

6.1. The transition from the Tarskian to the geometric emphasis came with the work of Morley. The most obvious new notion is that of (some kind of) rank for types (or their duals, formulas). The emphasis on the number of models, and the appearance of Ehrenfeucht-Mostowski models, certainly made Morley's notion of ω-stability (characterized in terms of the ordinal-valued Morley rank) an appealing generalization of the dimension theories of pure sets, or modules, or algebraically closed fields. Note however that real closed, or p-adically closed, fields have a dimension theory of obvious geometric importance, and these theories do not fit into **any** of the stability classifications that were to follow.

The fifteen years after Morley's paper [27] were dominated by the work of Shelah, which produced numerous subtle ideas around dependence of types. These arose without obvious geometric antecedents, and were embedded in an uncompromisingly set-theoretic framework. As already stressed, what seems of most interest now are the (finite) combinatorial notions underlying the set theory. Shelah typically gave many different characterizations of his stability notions, and after revision by the French school these began to lead to such beautiful insights as Poizat's on the implications for differential Galois theory of the ω-stability of DCF. But applications of pure stability theory were (and are) rare, principally because there appear to be very few interesting stable theories. Currently an old, and partially discarded, notion of Shelah, that of **simple** theory, has resurfaced, probably because ACFA is **simple**. The pure definability theory of simple theories has thrived [18], and there is some reason for optimism that the theory of nonabelian free groups is simple [30]. In any case that theory has a profound and beautiful Direct Image Theorem, due to Sela [30], whose discovery needed radical insights from low-dimensional topology.

6.2. Geometric model theory is the study of notions of independence of types. Classification in set-theoretic terms is no longer the goal. "Geometry" here has the sense, say, of projective or affine geometry based on a notion of dimension. or of analogous notions in complex algebraic geometry. Coordinatization has the same interest as it has in projective geometry, where one can interpret a field in incidence-theoretic terms. Already from the work of Malcev one had interest in, and techniques for, interpreting in certain algebraic groups fields over which they are defined. So, quite early in the stability era, Cherlin and Zilber conjectured that simple groups of finite rank are algebraic groups over algebraically closed fields, an insight which has inspired much beautiful work, and which may well be correct.

As the subject matured, one began to imagine an atomic theory for dependence (in ω-stable situations). In this setting the analogue of the notion of irreducible algebraic curve (the basis of many inductions in algebraic geometry and étale cohomology) is **strongly minimal set** (definable set of Morley rank one and degree one), and Zilber was bold enough to imagine that the scarcity of examples of ω-stable finite rank theories was because all strongly minimal sets (as "geometric" objects) were either

i) essentially pure sets;
ii) essentially like modules;

iii) algebraic geometric in the sense that they interpret algebraically closed fields.

Modular situations had become prominent in the remarkable **representation theory** of ω-categorical theories of finite Morley rank [4], shown to be built, in a potentially intelligible way, from classical linear geometries over finite fields. This was a serious confirmation of the principle that logical stability hypotheses imply the involvement of classical geometric structures. Of course the cohomological issue of how to build the models from strongly minimal sets can be expected to be hard, as in real life.

Hrushovski from the mid 1980's gave refined criteria for detecting interpretable groups (or fields) in more general structures. Then he came up with his new "forcing" method, a kind of Robinsonian procedure where subtle dimensional (or maybe cohomological defect) considerations limit the kind of extensions allowed. This yielded various quite new o-minimal structures, and refuted Zilber's "Trichotomy Conjecture". But Zilber's urge to represent rather general geometrical model theory situations as genuinely geometric was crucial for the last decade of the subject. Maybe one should limit the geometric model theory a bit? Perhaps the logical generality was limiting access to real world situations?

In their marvellous paper [15] Hrushovski and Zilber axiomatize a notion of Zariski topology. Into it go something of a Direct Image Theorem, and some other dimension axioms peculiar to the classical geometries. Dimension is defined as in noetherian spaces (i.e., as in Grothendieck's Foundations). In a tour-de-force they show that the strongly minimal sets in these **Zariski geometries** are of Zilber type, a marvellous generalization of the Fundamental Theorem of Projective Geometry.

The dénouement came when one succeeded in showing that Zariski geometries occur in DCF. Together with a fundamental result of Hrushovski-Pillay on definitions in modular situations, this allowed Hrushovski to make the first really essential use of model theory in diophantine geometry, namely, Mordell-Lang in characteristic p [10].

For a marvellous account of contemporary geometric model theory, see [11].

§7. Prospects.

7.1. There are various hints in the literature as to categorical foundations for model-theory [21]. The type spaces seem fundamental [28], the models much less so. Now is perhaps the time to give new foundations, with the flexibility of those of algebraic geometry. It now seems to me natural to have distinguished quantifiers for various particularly significant kinds of morphism (proper, étale, flat, finite, etc), thus giving more suggestive quantifier-eliminations. The traditional emphasis on logical generality generally obscures geometrically significant features [19].

7.2. We seem to be missing a classification of "tame" theories that would give $Th(\mathbb{R})$ and $Th(\mathbb{Q}_p)$ the same status as their stable or simple cousins.

7.3. I sense that we should be a bit bolder by now. There are many issues of uniformity associated with the Weil Cohomology Theories, and major definability issues relating to Grothendieck's Standard Conjectures. Model theory (of Henselian fields) has made useful contact with motivic considerations, including Kontsevich's motivic integration [6]. Maybe it has something useful to say about "algebraic geometry over the one element field" [25], ultimately a question in definability theory.

7.4. We have, in elementary terms, cohomological invariants in o-minimality [31], and in some Henselian fields (not $\mathbb{Q}_p$ though). Why not dare to be more motivic?

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