A History of Interactions between Logic and Number Theory

Lecture 1

I concentrate on logic and number theory, and the evolution of this interaction. The period under review is 1929-2003, beginning with Presburger's work [89]) on the metamathematics of addition and order on $\mathbb{Z}$.

Presburger's work was published two years before the dramatic discoveries of Gödel, which revealed the ubiquity of undecidability and incompleteness in global arithmetic. Presburger showed that a certain core mathematical structure is non-Gödelian, and it is to be stressed that even now, seventy five years later, one is learning interesting new things about definitions in this structure [84]). Moreover, we know now that the p-adic fields are uniformly non-Gödelian, and we understand that this depends mainly on how their definable relations are built over those of Presburger.

Since 1964 it has been evident that the local/global distinction in number theory corresponds to a non-Gödelian/Gödelian distinction in definability theory. Completing a structure tends to simplify definability theory, often changing a Gödelian situation to a non-Gödelian one. (There is one perplexing counterexample to this,the case of free groups and their profinite completions [14, 15]).

Heroic figures

Let us consider four heroic figures in logic,active around 1930, and their connection to number theory: **Gödel, Herbrand, Skolem, Tarski**.

The latter two made significant contributions to number theory. Herbrand made major contributions to number theory(ramification theory and cohomology [30]. Skolem has a well-known p-adic analytic method [51] in the area of finiteness of solutions to diophantine equations, and gave the first ultrapower construction of a nonstandard model of arithmetic [50]. Tarski [33] laid the foundations for model theory(now in need of redoing), and raised a beacon of the subject, his wonderful quantifier-elimination for real-closed fields [57]. Gödel [22] used the Chinese Remainder Theorem, certainly fundamental in the local/global perspective, to convert recursive definitions to closed arithmetical ones, inspiring the hope(fulfilled in 1970) that recursively enumerable relations on $\mathbb{N}$ or $\mathbb{Z}$ are exactly the diophantine ones (i.e,those definable existentially from $+$ and $\times$.

Herbrand is credited with the definition of projective limit, now fundamental in Galois theory and its model theory.

Skolem [125] did the analogue of Presburger's work for multiplication, but till now this has had no impact on the work to be discussed in these lectures.

Perspective

I present the material from the point of view of someone who has been in the subject for forty years and who has seen ideas come and go.I urge the younger participants to ponder Lang's statements from [85] (a propos algebraic number theory):

"It seems that,over the years,everything that has been done has proved useful,theoretically or as examples,for the further development of the theory.Old, and seemingly isolated special cases have continuously acquired renewed significance, often after half a century or more" (from Foreword):

"If there is one moral which deserves emphasis, however, it is that no one piece of insight which has been evolved since the beginnings of the subject has ever been "superseded" by subsequent pieces of insight. They may have moved through various stages of fashionability, and various authors may have claimed to give so-called "modern" treatments . You should be warned that acquaintance with only one of the approaches will deprive you of techniques and understandings reflected by the other approaches, and you should not interpret my choosing one method as anything but a means of making easily available an exposition which had fallen out of fashion for twenty years" (page 176).

All the lecture course at this meeting are of great interest to me. I have thought a lot,and more often daydreamed, over the years, about the ideas that go into them. It would have been indeed optimistic, in the 1960's, to imagine that our subject would have flourished to the extent that such a range of lectures is possible. My presentation must inevitably, for lack of time, marginalize important ideas, whether algebraic, geometric, analytic or modeltheoretic. Any distortion is likely to be compensated by a different emphasis in other lectures.

Core Structures

I regard certain mathematical structures ("core structures") as being fundamental. Thus one would not expect general logical ideas to be decisive in organizing research on those structures. But there are also analogies, and variations or uniformities, and the general compactness of first-order logic has proved valuable in understanding those.An intriguing recent development is the renewed relevance of such logics as Keisler's $L_{\omega_1,\omega}(Q)$ [132].

Among the core structures are:

- $\mathbb{Z}, \mathbb{Q}$, number fields and their rings of integers;
- finite characteristic analogues relating to curves;
- finite fields;
- $\mathbb{R}, \mathbb{C}, \mathbb{Q}_l$, and in general the locally compact fields;
- the ring of algebraic integers and relatives.

This is a provisional list. One expects new structures to appear from time to time, and some to come to be seen as fundamental(note that the $\mathbb{Q}_p$ are latecomers compared with the reals and the complexes).

A notable development is the emergence of new core theories, sometimes with no natural models (though any finite subset of the axioms should have a natural model).Examples are differentially closed fields, and existentially closed difference fields.

Early History: Tarski, Mal'cev and Robinson.

Tarski

Tarski, in the 1930's, contributed:

1) set-theoretic foundations of model theory, allowing precise definitions of structures, their syntax and semantics(not at all confined to first-order semantics);
2) the foundations of definability theory in the ordered field $\mathbb{R}$;
3) (via Presburger) the foundations of definability theory in the ordered group $\mathbb{Z}$.

Later,in Berkeley, Tarski (and students) isolated the crucial morphisms (elementary embeddings), the Limit Theorem,and the ultraproduct construction (and more general products, several relevant to number theory). Szmielew made a major contribution by a systematic analysis of the first-order theory of abelian groups (though not yet going as far as an elimination theory).But here,as in most of the ensuing work of the Tarski school, the emphasis turned to decidability. In [40]a systematic investigation is made of undecidability, for both complete and incomplete theories.The method of essential undecidability is prominent here. There followed,a decade later, the still useful update from the Mal'cev school [45]. Forty years later, almost all the open problems mentioned there have been answered, or, more importantly, have been exported to the world of definability. Regrettably, though, there is evidence that many of the important interpretations given there are unfamiliar to the younger generations.

No doubt the deepest work done in Berkeley on logic and number theory was that of Julia and Raphael Robinson. The former [56] gave a Π_3 definition of $\mathbb{Z}$ in the ring $\mathbb{Q}$ (never improved), and inspired the research that culminated in 1970 in Matejasevic's negative solution of Hilbert's 10th Problem for $\mathbb{Z}$. It is noteworthy that Julia Robinson used local/global considerations in her work, and that in (most variants of)Matejasevic's proof one uses the norm forms of quadratic extensions of $\mathbb{Q}$.

Poonen's lectures are of course devoted to Hilbert's 10th Problem, and the basic problem of the analogous result for $\mathbb{Q}$ and other global structures.

Mal'cev

In 1936 he gave the method of diagrams, and the general completeness/compactness theorem, and already gave some highly imaginative applications to group theory [37].

The school he founded at Novosibirsk produced Eršov and Zilber among many others.These two are singled out here because of their outstanding contributions to the topic of our meeting (Ersov on p-adic and regularly closed fields, inter alia, and Zilber on geometric model theory and diophantine geometry).

Abraham Robinson

He strove to open roads (both ways) between logic and algebra (and so number theory). For a very apt comment on this image, see [140].The emphasis on model-complete theories, and Robinson's Test, in the form almost of a Nullstellensatz, initiated a development that has lasted fifty years (and flourishes still, because of a symbiosis with the geometric model theory started by Morley in a Tarskian setting).

Among Robinson's achievements, by these methods,[35] are:

1) very conceptual approach to real-closed fields and Hilberts' 17th Problem on sums of squares(with bounds);

2) definability theory for algebraically closed fields with valuation, important thirty years later in a local/global setting [23];

3) bounds in polynomial ideals, a topic still developing because of the needs of a logic of cohomology [38, 31];

4) functorial compactification in nonstandard analysis;

5) differentially closed fields, an entirely natural theory with no really natural models(its subsequent value is that it provides a rich "geometrical" completion for diophantine geometry over function fields);

6) generic structures by a variety of "forcing" methods, and the emphasis on finding axioms for those structures.

Saturated models and ultraproducts

These came from the Tarski school [124],and have proved, despite their set-theoretic trappings, very useful in applied situations,as a setting for converting quantifier-elimination results into results on extension of isomorphisms. In the early days the method was bound up with the ultraproduct construction,both in Keisler's work and in Kochen's paper [117] which remains a beautiful introduction to model-theoretic algebra.

Gödelian and non-Gödelian

Already in 1931 Gödel showed that the logical analysis of definitions in $\mathbb{Z}$ is subject to severe limitations in principle.Only after the heroic foundational work of the 1930's [52] did the full austere picture stand forth. The essential facts are:

1) arithmetic hierarchy-every quantifier-alternation increases the class of definable relations ;

2) the recursively enumerable sets appear already as projections of algebraic sets.

Note. Here too,there seems to be a risk of loss of folk memory, as logic becomes more compartmentalized. The extreme pathology of recursively enumerable sets (maximal or even more exotic) may actually be a tool in work on metamathematics of number theory. Rabin and the author used such things thirty years ago in connection with amalgamation property in full arithmetic, and there are also prospects for using the fine structure of r.e sets to get undecidability of Hilbert's 10th Problem for $\mathbb{Q}$ without giving an existential definition of $\mathbb{Z}$ in $\mathbb{Q}$. Another aspect is that tricks from early work on Hilbert's 10th Problem yield striking results about polynomials "enumerating " the primes.

Tarski was the first to locate a rich core structure, $\mathbb{R}$, which is non-Gödelian.Not merely is it decidable, but there is no hierarchy theorem. All sets are existentially definable, indeed quantifier-free definable if one special existentially definable relation, order, is taken as primitive. After 1964, and the work of Ax-Kochen-Ershov (henceforward AKE) [42, 41, 115, 114, 113] one began to hope that one might find other such structures, and one moved towards non-Gödelian territory.

Lecture 2

p-adic fields.

After 1964, one had a web of analogies connecting the logics of the completions of number fields. I give a revisionist account. I was beginning research in this period, and was influenced by many forces, such as Morley [36, 46]. Forty years on, there is general agreement that Morley's ideas belong with those of the Tarski-Robinson -Mal'cev tradition, though , literally, they do not apply directly to the key theories.

Prior to 1964 one understood, initially via Tarski's barehand methods, the basic metamathematics of algebraically closed and real closed fields, and no others. A variety of methods had , since Tarski, been deployed on the basic theories (for example, those of Robinson, Kochen, and the Shoenfield criterion using saturated models). I recommend that one remember all these possibilities.

As of now, the essential feature is a uniform definability theory (based on a uniform quantifier-elimination)for the local fields coming from classical number theory. Those fields are all locally compact (see Weil's book [129]for the unifying nature of this idea alone), and, in all cases but that of C (a base for) the topology is algebraically definable.So it is natural, first time round, to look at these fields in the pure language of field theory.

Defining the topology

In the real field, the order (and hence a basis for the topology) is definable thus:

$$x \geq y \Longleftrightarrow x - y \quad \text{is a square}$$

In a finite extension of $\mathbb{Q}_p$, the valuation ring(and hence the topology, is definable thus:

$$v(x) \geq 0 \Longleftrightarrow 1 + \pi x^2 \quad \text{is a square}$$

where π is a uniformizing element, i.e an element whose value is minimal positive in val(K). (Note that the use of pi can be eliminated by a standard trick of quantifying over possible uniformizing parameters).

When $p \neq 2$, one has to modify the definition thus:

$$v(x) \geq 0 \Longleftrightarrow 1 + \pi x^8 \quad \text{is a square}$$

Now is a good moment to introduce the power predicates $\mathbf{P}_n$, with the defining condition:

$$\mathbf{P}_n(x) \Longleftrightarrow x \quad \text{is an n} - \text{th power.}$$

So we have shown, uniformly for the real and the p-adic cases, that the topology is quantifier-free definable in terms of the $\mathbf{P}_2$. The other power predicates are needed in the p-adic cases for quantifier-elimination . Note their link to Presburger arithmetic, since the valuation of an n-th power is divisible by n. The quantifier-eliminations in the p-adic fields somehow reflect (among other features) the quantifier-elimination in the value group.

It is often more natural to use modifications $\mathbf{P}_n^*$ interpreted as the set of nonzero n-th powers (for the same reason as it is more natural to use strict less than rather than less than or equal in the real case).

That the topology of $\mathbb{C}$ is not field -theoretically definable comes from the existence of discontinuous field automorphisms of the complex field(the sort of phenomenon that distresses Deligne in [103]).

An important analogy between the three families of fields $\mathbb{K}$ (complex,real and p-adic)is that $\mathbf{Gal}(\mathbf{K})$ is prosolvable, and topologically finitely generated. Moreover, every finite extension of K is generated by an element of $\mathbb{Q}^{alg}$.

The fundamental result connecting the three definability theories, most illuminating, at least on first reading, if restricted to the cases of the complexes, the reals and the unramified extensions of $\mathbb{Q}_p$ (the ramified case is a bit harder, see [90]) is that every definable relation is in the Boolean algebra generated by the algebraic sets and the sets defined by conditions

$$\mathbf{P}_n^*(f(\bar{x})$$

The main motivation behind my use of these predicates in the p-adic case was that taking powers has a lot to do with constructing solvable extensions. For $\mathbb{C}$ there are no algebraic extensions, and all the power predicates are redundant.For $\mathbb{R}$ there is only a cyclic extension of order 2, extracting a square root of -1. For the p-adics, all power predicates are needed.

An amusing,but not accidental,observation is that the unit ball in the reals is definable by

$$\mathbf{P}_2(1-x^2).$$

This shows a uniformity in definition with those for the p-adic unit balls,now taking $p=-1$. In fact,it suggests construing the reals as the - 1 -adic numbers.

Galois Groups

A propos the Galois groups of p-adic fields, there is the truly beautiful result of Neukirch [128], establishing another analogy with the real case. Gradually Neukirch's result is filtering into the modeltheoretic imagination, and I would expect it to lead to some helpful shifts of emphasis (for example in connection with elimination of imaginaries). Just as Artin and Schreier had shown that real closed fields are characterized by having their absolute Galois groups finite and nontrivial (and then necessarily cyclic of order 2), Neukirch inspired the analogous result for the p-adics [70, 73], namely that a field having absolute Galois group isomorphic to that of $\mathbb{Q}_p$ is elementarily equivalent to $\mathbb{Q}_p$. No doubt Pop will give full details on this result and the many others of this kind recently obtained.I have deliberately stated this result in an anachronistic way, looking to a future where one will interpret p-adic formulae in a more invariant Galois-theoretic way. I will delay a little yet before presenting the basics about axioms for the various completions, i.e Hensel's lemma and all that. All this, and more, will be needed before one can fully appreciate the uniformities underlying the model theories of the completions.

Analogies

We have not yet succeeded in finding a truly suggestive formulation of the above analogies, let us say in terms of the allegory [96] of definable sets and functions, and indeed I am not convinced we have the right category in mind. There are however some simple and useful observations that can be made now.

Subsets of the affine line

For $\mathbb{C}$,definable subsets of the line are (exactly) finite or cofinite, and, in particular ,have interior for the Zariski topology if they are infinite.

For $\mathbb{R}$, from Tarski's famous paper of 1935 [33], definable subsets of the line are finite unions of intervals (interpreted in the most liberal sense), or,equivalently, are the sets with finite boundary. Again, if infinite they have interior.

For $\mathbb{Q}_p$, and indeed for all the completions, definable subsets of the line have interior, provided they are infinite. Moreover, once one makes the obvious definition of the analogue of interval, they are finite unions of intervals.

These are all examples of what we now call minimality results (the original minimality comes from a paper of W.Marsh,written in the wake of Morley's great paper [46]). What is most important for us is that each of the minimality notions leads to a dimension theory for general definable sets, and a means of analyzing sets by fibering arguments ,leading to so-called cell-decompositions.

There are issues of uniformity here, and they are subtle.Two particular issues are:

- uniformity in K
- uniformity in p

The most direct uniformity is that in each case definable functions are piecewise algebraic. However, given a formula $\Phi(\bar{x}, y)$ of field theory, it is not easy to track the uniformity in the structure of the function it defines. This may very well depend on p, as one ranges over p-adic fields. A full understanding of what happens came only after Ax's work on the elementary theory of finite fields (the residue fields are finite). But in any case one readily proves for all K in question algebraic boundedness, that is a uniform bound on the cardinalities of the finite members of a definable family of sets(but this bound does depend on p). This notion was first seen in various papers written in Morley's aftermath, in particular the beautiful paper of Baldwin and Lachlan [39] In specific algebraic examples it was first investigated by Winkler and me. But an illuminating general discussion had to wait a while, till van den Dries, who, with Scowcroft, [26, 24] showed that it is the basis of a general theory of dimension and cell-decomposition. The notion of dimension is definable within families, a basic result in all that followed.

As regards the cell-decomposition, the basic sets proved to be those defined via

$$\mathbf{P}_j(f(\bar{x})).$$

In the case of the real field one needs only j=2.It is convenient to consider modified predicates

$$\mathbf{P}_j(f(\bar{x}))$$

corresponding to being locally on one side or other of a hypersurface. The orientation takes values in

$$\{\pm 1\}.$$

In the p-adic cases there are many more values for the "orientation". These are taken in the projective limit of the groups

$$\lim_{\rightarrow} \frac{\mathbb{Z}_p^*}{P_n^*}.$$

Denef [75] following Cohen, gave a thorough discussion of cell-decomposition in the p-adic case, for a fixed p. On the way he gave explicit definable Skolem functions (sections) for definable families of finite sets. His cell decomposition made serious use of the fact that the p-adics have only finitely many extensions of each dimension. The essential element in his induction involves working with a finite family of polynomials in many variables and producing definable cells on each of which the polynomials have uniform behaviour relative to the partial orientations given by the modified power predicates.

The issue of uniformity of the cell-decompositions, both in families and in p, has proved fertile.In the former, for fixed p, one gets uniformity of rationality for p-adic integrals. In the latter, following Pas [87, 88] and Macintyre [98], one gets uniformity across p for those integrals. But the really satisfying uniformity took longer to reveal, in the work of Denef and Loeser [60] on motivic model theory.

One uniformity across K, that has initiated a search for a general notion of minimality in these cases, is that definable sets are locally closed, and so measurable for the Haar measure of each K. Infinite definable subsets of the line of K have interior, and in general nonzero measure is equivalent to nonempty interior.

The issue of uniformity of measure across families, or across p, is delicate,and will be discussed in the project associated to my lectures. The work of Denef and Loeser certainly gives a beautiful uniformity across p, in terms of rational functions of p^{-s}. But for the real case one confronts rather serious problems about integrals in o-minimal theories.In the special case under consideration, work of Lion and Rolin [86] does give useful information.

In the analytic cases that came later, one does not yet have a satisfying uniformity even in the p-adic case, but it seems likely, in view of recent work of Cluckers [84], that this will be forthcoming.

For all the K in question, one has a category (in fact, allegory in the sense of Freyd and Scedrov [96]) of definable relations, including functions, and one can show that the model-theoretic dimension is preserved under isomorphisms. What else is? Remarkably,

for $\mathbb{R}$, the modeltheoretic Euler characteristic and nothing more ;

for p-adic fields, nothing more (a recent result of Cluckers).

Note, however, that this result is not uniform in p. This suggests to me that a small shift of emphasis is needed. I suspect that it is misguided to ignore that for any given shape of definable set Cluckers' result fails for almost all p.

What happens for $\mathbb{C}$? The matter,in terms of Grothendieck rings,is subtle indeed [138].

I conclude this lecture with some miscellaneous remarks about language, axioms, ultraproducts, types.

Language. For algebraically closed fields,the field language still seems appropriate,and the right medium in which to express and derive the most simple Lefshetz principle, the first case in which model theory treats the impulse of letting p go to zero. Tarski [120] already dealt with this, and there is little more to be said, at this level, than he did. It does seem, however, worthwhile to make some comments on various topological spaces that appear in elementary modeltheoretic algebra. One has Tarski's spaces of complete theories in a first order language. These are Stone spaces, i.e compact, totally disconnected spaces. The complete extensions of a particular theory form a closed subspace of the Tarski space. For algebraically closed fields, the Tarski space has isolated points corresponding to each prime p, and a unique limit point at (characteristic) zero. This space is Hausdorff, in contrast to the more recent space Spec($\mathbb{Z}$), consisting of the prime ideals of $\mathbb{Z}$, with the spectral topology. The map sending the theory of algebraically closed fields of characteristic p (possibly 0) to the corresponding ideal in the spectral space, is a continuous bijection,but not a homeomorphism, and this is essentially the Lefshetz Principle.

As far as axioms are concerned,the obvious ones have no competitor. One says, for each n, that monic polynomials of degree n over K have roots in K. It is notable that for all the K so far discussed the standard axiomatization involves similar axioms, but for a restricted class of one variable polynomials.

For the real closed fields, there is again no real competitor to the language with order. But there is a bit of freedom in axiomatization. The standard thing is to say that positive elements are squares, and odd degree monics have roots. But there are alternatives here. One is to use the Sign Change Scheme, saying that if a polynomial changes sign on an interval it has a root there. Yet another possibility is to say that K(i) is algebraically closed. This points towards the beautiful results initiated by Neukirch, and mentioned earlier.

For p-adic fields, there are more alternatives even as far as language is concerned. As I pointed out, one can get by in the usual language of field theory, since the valuation structure is algebraically definable. But the analogue is true for the reals, and one is not thereby tempted to do without order. If one is studying general valued fields (and the work of Ax-Kochen and Ershov made this irresistible) one naturally used some many-sorted formalism (but note that Tarski's influence somehow inhibited this move). One possibility is a three sorted set-up, using value group and residue field. Another is to use one of them. Yet another option is to use the formalism of places. Another is to use a predicate for the valuation ring. Most of these options are worthwhile, in appropriate contexts, particularly in definability theory. Somewhat different is the option to use a cross-section, as was done in the first papers. Such a thing is never definable (in contrast to the notions mentioned above), but exists on suitably saturated models,and is a great convenience in proofs of completeness. There is however a slight defect of beauty. Similar remarks apply to the undefinable angular components use by Pas. But these have been used systematically in all the subsequent deep results, including Denef-Loeser.

The natural axiomatizations involve Hensel's Lemma (which has many equivalent forms, most worth knowing) , together with axioms for the value group and the residue field. When the residue field has characteristic zero, or in finitely ramified mixed characteristic cases, or in certain "tame" characteristic p cases, one thereby obtains a huge range of results on completeness, decidability, and definability. However, there are major gaps in our knowledge in important cases such as power series over finite fields. These relate to a limitation in the essentially uniform proof, which uses immediate extensions and uniqueness of maximal immediate extensions. The problem with Hensel's Lemma is that it may not be strong enough to prevent a field having an immediate algebraic extension, though it has this strength in the positive cases mentioned. Kuhlmann [136, 144, 145, 4]has stressed the need for other axioms involving additive polynomials ,but one seems far from understanding this situation.

There is, in certain cases, and notably in the p-adic cases ,the possibility of using other much more subtle axiomatizations. Koenigmann's Theorem, the culmination of work of Neukirch and others, shows that the p-adic fields have a Galois-theoretic axiomatization. From the work of Cherlin, Macintyre and van den Dries [34](and indeed earlier folklore) it is known that the condition on a field K, to have its absolute Galois group isomorphic to a fixed finitely generated profinite group, is first order. There is very considerable uniformity in the axiomatizations, as p varies, because of the beautiful shape of the p-adic Galois groups. One may reasonably hope that this will be useful in advanced definability theory. What I have in mind is a p-adic analogue, for Kochen's [111]work on integer-definite functions, of what Kreisel [155] did for sums of squares by what is essentially a Galois-theoretic argument.

In the light of the above one sees a new Lefschetz Possibility.And it fits well with the functoriality of ultraproducts. From a nonprincipal ultraproduct on the primes one gets , from the family of p-adics, a new Henselian field, with characteristic zero residue field. It was by no means obvious what axioms the new residue field satisfied (for that one had to wait four years), but it was routine from the general work of AKE that the new Henselian field was elementarily equivalent to the corresponding ultraproduct of formal power series over finite fields. From this, one could easily, using work of Lang, derive an almost-everywhere version of the conjectured C_2 property of the p-adics (and this turned out to be optimal). That the ultraproducts were isomorphic under CH had a certain drama at the time,and there are very difficult set-theoretic issues arising (dealt with later by Shelah),but currently there is little interest in this aspect of the matter.

Though type spaces were beginning to be important in 1964, no attempt was made to make any analysis, whether in a Lefshetz mode or any other, of the spaces for the reals and p-adics. It should startle the young reader that in 1964 logicians knew almost nothing about the structure of definable sets in dimension more than one over the reals. Of course, the beautiful stability theory then coming into fashion did not fit any of the above K, except the complexes.

Lecture 3

Model theory of finite fields

The decidability of the elementary theory of finite fields was a prominent problem around 1960, and the decidability of the corresponding problem for p-adics was implicitly seen to reduce to this by the AKE analysis. Note that this was before the negative solution of Hilbert's 10th Problem. One of course knew counterexamples to local/global principles for diophantine equations, and might therefore suspect a vast difference between the universal theory of the p-adics and the universal theory of the integers. In 1963 Nerode [116] proved the decidability of the universal theory of the p-adics by using the compactness of the ring of p-adics integers. His proof was for a single p-adic,and no generalization to all p-adics simultaneously was apparent.

Ax considered the general ultraproduct of finite fields, with a view to understanding the almost all theory of finite fields. His most striking observation was that Weil's deep result, the Riemann Hypothesis for curves over finite fields (or, rather, the ensuing Lang-Weil estimates) would provide the dominant axiom for the nonprincipal ultraproducts. Eršov had detected some special cases of this, too. The ensuing Weil Axiom Scheme, together with a Galois-theoretic scheme, completely axiomatized the ultraproducts. While the Galois axiom is true for all finite fields,the Weil Axiom is true for none, but comes true in the limits provided by ultraproducts.

Uniformity across Finite Fields

Ax's axiomatization depends on a nontrivial uniformity:

There exists a function F from N to N, whose exact form is irrelevant, at least in elementary situations, such that if V is an absolutely irreducible affine curve over $\mathbb{F}_q$ and $q \geq F(\text{genus}(V))$ then V has an $\mathbb{F}_q$-valued point.

Now, the essential point is that the genus of a curve is bounded above by a simple function of the degree of polynomials defining the curve, independently of the coefficients of these polynomials, and independent of the ambient field. More generally and abstractly, if V belongs to a family of varieties (indexed, say, by a constructible set) there is a function G of the family so that if $q \geq G((V))$ then V has a $\mathbb{F}_q$-valued point. It follows easily by the Los Theorem that the nonprincipal ultraproducts satisfy the property now known as PAC, or (in my opinion better) regularly closed. This is :

(PAC) Every absolutely irreducible variety has a point.

This was sharpened by Geyer [1], who saw that it is enough to demand that every absolutely irreducible curve has a point.

Axioms

To establish the above as an elementary axiomatization, one needs to know that the property absolutely irreducible is first order, that is has a definition not depending on coefficients or ambient field.There are many ways to see this,equally good unless one has constructive inclinations. For example, one can use the result, from the Robinsonian theory of bounds in polynomial ideals, that prime is elementary, combined with Tarski's quantifier-elimination for algebraically closed fields.

To complete the axiomatization for the nonprincipal ultraproducts, one imposes two other conditions. One is the obvious one, that the fields are perfect, as finite fields are. The other axiom scheme is more significant. We know,by counting, and using the Frobenius automorphism of finite fields, that each finite field has exactly one extension of each finite dimension. Counting has no obvious useful elementary version, but one can use old fashioned algebra (Tchirnhausen transformations and the like) to show that the property of a field to have exactly one extension of each dimension) (called quasifinite by Serre) is elementary [1, 34].

Putting PAC, perfect, and quasifinite together one obtains a set of axioms for what we now call pseudofinite fields. There is by now a rich theory of these structures. One may have had reservations about the origins of those fields, in the farout world of ultraproducts (Mumford) , but their appearance in so many important results over the last 40 years has surely established their credentials. They have appeared "in nature" since, in several ways. Jarden showed that a generic element of the Galois group of a countable Hilbertian field has fixed field pseudofinite. Much later van den Dries observed that the fixed fields of an existentially closed difference field is pseudofinite. Later still, Pop showed that the field of totally real algebraic numbers is PAC.

Galois Aspects

What I like about regularly closed is that it reveals a model theory (of fields) for the category of regular embeddings (which includes the category of elementary maps).The regularly closed fields are just the (Robinsonian) existentially closed structures for the category of regular maps.

The model theory of regular maps is rather rich, because it has a dual model theory of Galois groups [34].Regular maps of fields induce (contravariantly) epimorphisms of absolute Galois groups. The model theory of profinite groups is a thoroughly many-sorted affair, with no quantification over group elements, but rather a whole range of "bounded " quantifications over finite quotients. Its utility depends largely on Herbrand's inverse limit construction. Finitely generated profinite groups play the role of finite groups in the comodel theory, and the Iwasawa or Frobenius groups behave like homogeneous models.

The Galois group of a regularly closed field is subject to a fundamental cohomological limitation, namely that it is projective. The richness of the theory depends on the fact that this notion is first-order (for the field), and has many equivalent formulations. The pseudofinite fields have Galois group $\hat{\mathbb{Z}}$ and this group is finitely generated.

Ax showed that every pseudofinite field is elementarily equivalent to an ultraproduct of finite fields. For this, in characteristic zero, Cebotarev's Theorem is used.

Ax uses the conventional isomorphism approach to elementary equivalence, and his proof yields that two pseudofinite fields are elementarily equivalent if and only if they have the same characteristic and the same "absolute numbers", i.e the same monic polynomials, in one variable over $\mathbb{R}$, are solvable in each. It is suggestive to give this a more invariant, less syntactic, formulation. Until one fixes an algebraic closure of the prime field, the notion of algebraic numbers has little sense. In fact, what Ax is attaching to a theory of pseudofinite fields ,say in characteristic zero, is a conjugacy class of closed procyclic subgroups in $\mathrm{Gal}(Q)$. Moreover, his analysis shows that this assignment defines a homeomorphism form the Tarski space of complete theories of pseudofinite fields of characteristic zero to the Vietoris space of conjugacy classes of closed procyclic subgroups of the compact group $\mathrm{Gal}(Q)$. James Gray has elaborated this considerably to fit the entire Tarski space, with no restriction on characteristic, into a Vietoris space attached to $\mathrm{Gal}(Q)$.

From these considerations (but with less abstraction than employed above) Ax [112] readily proves decidability of the theory of pseudofinite fields, and then, by attention to the form of his axioms, decidability of the theory of finite fields.

Elimination and the Solvability Predicates

Ax's student,Katerina Kiefe [34], made the natural extension of the above to give a quantifier elimination for the theory of pseudofinite fields, in terms of predicates that Robinson had already used, namely the solvability predicates

$$\mathrm{Sol}_n(\bar{x})$$

where $\mathrm{Sol}_n(x_0, \ldots, x_{n-1})$ means that $x_0 + x_1 y + \ldots + x_{n-1} y^{n-1} + y^n$ has a root.

In terms of our emphasis on regular maps, all this is natural. In fact the basic fact underlying the elimination is that any regular embedding of pseudofinite fields is elementary, giving a Robinson's test in the language with the solvability predicates. This point of view was first stressed by Eršov, and discovered by me later and independently.Note that the solvability predicates include the power predicates, but only in special cases of pseudofinite fields will the power predicates give quantifier elimination.

It seems to me worthwhile to go on to give a Galois theoretic interpretation of the type spaces, along the lines of what was said above for the Tarski space of complete theories. Indeed, I favour looking for an interpretation in terms of etale fundamental groups. The currently most atractive version of quantifier elimination is the Galois stratification devised by Mike Fried (a beautiful and important idea, naturally linked to cell decompositions and stratifications) and developed by him, Jarden and Haran [1].Recently it has been used systematically by Denef and Loeser, and in his Beijing talk Denef makes some progress in giving this an invariant formulation. At issue is the extent to which the Galois stratification formalism goes beyond first-order logic.

Jarden led the effort to detach the study of PAC fields from the special case of pseudofinite fields, and the book of Fried and Jarden conveys how much can be achieved. The projectivity of the Galois groups allows the comodel theory to flourish over a huge range. However,I think it fair to say that till now the general case has had little relevance for number theory. An exception can perhaps be made for the efforts of Fried and others on the inverse Galois problem.No doubt Pop will discuss these matters.

Separably Closed Fields

Eršov [42] early on dealt with the special case of separably closed fields,before any of the work on regularly closed fields.The metamathematical analysis is by no means as easy that that for algebraically closed fields,and on the other hand has special features not shared by all regularly closed fields.Such notions as p-basis and separable transcendence basis are crucial to his analysis.Moreover,he was aware of a link to differential algebra,as was I when I lectured at Yale in the early 1970's.Carol Wood [147]was for a long time the only author except Eršov to write on these matters.One odd twist is that the interesting result that separably closed fields are stable (and they are widely believed to be the only stable fields) came somewhat indirectly during audience participation at a Shelah lecture at Yale in 1975.Shelah had proved stability for differentially closed fields in characteristic p,without remarking that this showed that separably closed fields are stable. It is remarkable that the model theory of those fields,so long regarded as of little interest,is now of central importance,because of Hrushovski's use of it in applications to diophantine geometry (see the Pillay-Scanlon lectures).For the modern theory of separably closed fields,see [139, 141, 143, 119, 123].

Making Frobenius Explicit

I have stressed that Ax's work is inspired by Weil's, but it uses only the most superficial aspects of Weil's vision .One has gradually come to see how to bring more of it into the logical understanding. Firstly, Chatzidakis, Macintyre and van den Dries [20] brought into the definability the rudiments of the numerology of the Riemann Hypothesis for curves, assigning to definable relations dimensions,and counting definable sets over finite fields. In particular one again encounters the phenomenon of algebraic boundedness.

An important effect of this paper was to inspire Hrushovski to observe that pseudofinite fields are simple, in the sense of a partially forgotten notion of Shelah. This restarted the study of simple theories, and led to a series of impressive papers. Chatzidakis [118, 121]went on to characterize simplicity, for PAC fields, in terms of smallness of the Galois group. In addition, she made a deep study of the notion of forking in the setting of PAC fields.

Note that as in the earlier cases definable maps are piecewise algebraic. Moreover, definable sets may not be quantifier-free definable, but they have etale covers by quantifier-free definable sets, an observation suggestive if one wants to make this area more geometric.

But the most obvious feature of the Weil analysis not yet made model theory is the role of the Frobenius map, with its action on cohomology, and its associated eigenvalues, whose sums count points on varieties. We do not, even now, have all this under logical control, but the ongoing attempt has produced much of value.

If we think of the origins of pseudofinite fields, we are tempted to carry along more structure. Thus, the field $\mathbb{F}_q$ is the fixed field of the Frobenius automorphism on the algebraic closure of the prime field. The inevitable question is:

What is the theory of the ultraproduct of the automorphisms?

This question includes the one Ax answered, since the pseudofinite field is the fixed field of the ultraproduct of the Frobenii (by functoriality of ultraproduct). So it must be difficult, and indeed it is. The two known solutions involve at a minimum detailed information from Deligne's [103] work on the Weil Conjectures for affine varieties.

I posed essentially this problem in the 1980's in the context of structures consisting of algebraically closed fields carrying a Frobenius automorphism, at the outset the basic Frobenius map of exponentiation to power p. The point of view was that of the Lefshetz Principle. What happens if we let not merely the characteristic, but also the Frobenius, vary? The difference from the classic case of algebraically closed fields is that there is no Frobenius in characteristic zero. But of course the ultraproduct creates one.

I began by thinking that the theory of the Frobenius, as p varies, might be undecidable.Later I stepped back, and asked the more fruitful question :

Is the class of existentially closed fields-with-automorphism elementary?

The question, once asked, was not so hard to answer. Robinson's work,for example on differentially closed fields, provided a strategy. One looks to see how much one would need to know about systems of equations in the extended language in order for Robinson's Test to give one model-completeness. The axiomatization is definitely harder to find than Robinson's, but it could have been found in Robinson's time, and by Robinson's methods. Once I saw the axiomatization [94],now known as ACFA (which would later be put in a more suggestive form by Hrushovski) I asked myself if this might in fact be the axiomatization for Frobenius as well. Early,but slight, grounds for optimism came from van den Dries' observation that the fixed field of such a generic automorphism is pseudofinite.So ACFA enriches the theory of pseudofinite fields.

The axioms were better aligned with Robinson's when one made the essentially trivial observation that these axioms were the axioms for a generic difference field. Later this would have unexpected consequences for a very deep problem of Jacobi [140] in difference algebra.(Later work,by more conventional ideas,succeeded in mastering the metamathematics of the lifting of Frobenius to the Witt vectors over an algebraically closed field.This was done by Belair,Macintyre and Scanlon.The full version is not yet accepted for publication,but a short version is available [92].It is of definite interest that the analysis is given partially in terms of the p-derivation associated to the Frobenius,thus giving a link to the logic of differential algebra).

My axioms were formulated in terms of iterations of the automorphism. This keeps the formal dimension of the problem down, but it conceals a feature which Hrushovski brought out, namely that the axiom scheme (now called H) crucial for generic automorphisms has the same shape as the main diagram one meets in one of Weil's formulations for the Riemann Hypothesis for curves. It differs in that it considers conjugate varieties, whereas Weil need only consider varieties over the fixed field. Once one sees this formulation,one sees what one will need to prove about Lang-Weil estimates in the difference algebra situation. Unfortunately,the literature was silent on this at the beginning of my work on the problem. Eventually I learned that related issues are involved in the work of Pink and Fujiwara on a Conjecture of Deligne (used more recently by Taylor and Harris in connection with the local Langlands Conjecture for general linear groups). From these papers I eventually found my way to the proof that the axioms for Frobenius are the axioms for a generic automorphism. Hrushovski found an essentially different proof independently.

The complete extensions of ACFA are classified ,say in characteristic zero, by conjugacy classes of elements in $\mathrm{Gal}(Q)$. They are simple, and no doubt the most interesting simple theories which are not stable.Because of the presence of the automorphism, one cannot hope to have the definable functions piecewise algebraic, but they admit an obvious analogous description [94]. There is a quantifier-elimination involving an obvious elaboration of the solvability predicates. In general the metamathematical analysis, at Robinsonian level, follows the lines of Ax's for pseudofinite fields. For example, one easily gets decidability. However,by the time ACFA appeared one was ready to apply to it analogues of notions from Morley theory [62, 63] and this has proved surprisingly powerful in Hrushovski's [64] application to the Manin-Mumford Conjecture (see the lectures of Pillay and Scanlon).

For me personally, the main surprise arising from the discovery of ACFA was how much there was to be done in terms of a model-theoretic reaction to the development of etale cohomology and its relatives. Ultraproducts of Weil cohomology theories [91] make some small contribution to the motivic vision, and bring out some uniformities connected to the Standard Conjectures (but unnoticed by the experts). This particular development is directly in the line of Robinson's early work on bounds in polynomial ideals. Independently, Schoutens has carried on significant investigations in this line, for advanced commutative algebra.

Again, in a different direction, one begins to see cohomological ideas coming up all over applied model theory, for example in o-minimality.They are certainly present in the work of Denef and Loeser, currently a high point of the subject. However, they are not so obvious in p-adic settings. There seems no obstruction in principle now in making an analysis of crystalline cohomology from a modeltheoretic perspective.

Lecture 4

p-adic integrals

A great advance was made here, initiated by Denef. Hensel's Lemma is the main axiom scheme underlying p-adic fields, but there is more that can and should be said. In trying to find out if equations are solvable in the p-adic integers, one succeeds if there are nonsingular solutions modulo p, but otherwise one has to keep on trying. By compactness of the p-adic integers, there is a global solution if one has solutions modulo all powers of p. But what is the uniformity in how far one has to go? Indeed it is not obvious there is any bound or uniformity. Before AKE, Greenberg and others had uncovered uniformities, and Igusa and others had considered various generating functions coding the numerology of solutions modulo powers of p. Though Hensel's Lemma is not always directly applicable, there are strong regularities suggesting that the generating functions (Poincare series) should be rational. Such conjectures were made by Borevich and Shafarevich, as well as Serre and others.

Denef used a known device for converting such sums over the p-adics to integrals against the Haar measure, and then exploited the cell-decomposition to calculate the integrals by systematic use of Fubini's theorem. For a model theorist, the beauty of the method is that it works for arbitrary integrals coming from definable functions on definable sets, and so reveals, albeit in a slightly compressed way, a measure of how truth or satisfaction in the p-adic integers relates to truth in the residue rings (an idea already prominent in Cohen's paper).

The method had an immediate impact on group theory,when it was realized that certain generating functions attached to nilpotent groups are of the form calculated by Denef. When,rather later, Denef's method was extended to an analytic setting, there were deeper applications to p-adic analytic groups [148, 149]. And when Denef and Loeser made the uniformities transparent in a motivic setting, the group theory profited. Indeed,there is hope that the method will allow the solution of some old, and fundamental, problems of Higman on counting p-groups.

Denef inevitably turned his attention to uniformities in his result. First he considered, as usual, uniformities in parameters, in terms of how the rational function varies. The ideas for this are essentially in his cell decomposition. However, when one turns to uniformity in p, i.e how rationality (in the complex power of p) as p varies, the matter is more complicated. This led to the papers of Pas [87, 88] and me [98]. Pas' paper uses the undefinable primitive , angular component, and has been the source of choice for later applications. I used a many sorted formalism which took account of the residue rings as p varied, and ultimately was to take account of the Weil numbers and the like.B ut my formalism is not very suggestive. The ultimate uniformity is revealed by motivic methods, and Pas's formalism is used.

As p goes to infinity, one converges in the Tarski topology from the p-adics to power series over pseudofinite fields, and the uniformities are ultimately to be explained in terms of these objects and their motives.

Real and p-adic exponentiation

From the mid 1970s onward some people in the area of applied model theory began to confront Tarski's notorious problem about the decidability of the real field enriched by the exponential function. Tarski had known that the complex field with its exponential is Gödelian, because one can define $2piZ$ and thereby Z .(However, at the end of these lectures I will discuss a refinement of this judgement due to Zilber).In any case there was no evident definition of Z in real exponentiation, and people began to rediscover the result of Hardy that real exponential polynomials (iterations of exp allowed) in one variable have only finitely many zeros on the real line, with a uniformity in terms of the complexity of the exponential polynomial. One was also aware of Strassmann's theorem (used long before by Skolem!) about finiteness of zero sets of convergent power series on the p-adic integers, which of course gives an analogue of Hardy's result.

Van den Dries and I, and the group at the Humboldt University [95, 100, 150, 153, 156, 151, 152, 154] used power series methods, and ideas from Rosenlicht's work, to make some progress on the nature of definitions in real exponentiation. Little did we suspect that we were already dealing with a formal nonstandard model of real exponentiation!(See [83, 5]. But it seems fair to say that our secure understanding of the model theory of valuations was useful for us then, and for the subject in future developments.

From the early 1980's I was aware of the possibility that one could use the commutative algebra around Weierstrass Preparation to reduce analytic problems to algebraic ones,and perhaps make inroads on the real and/or p-adic exponential. For the p-adic case this is explicitly spelled out in a paper given at a computer science meeting in 1983 [101]. What I knew was missing was a uniformity in noetherianity around the Weierstrass theory. I could not prove this, and people I asked gave me little encouragement. But in fact it was the right way to go, and it was taken in splendid style in 1986 by Denef and van den Dries [27]. They used a uniformity of the kind I had sought to lift the elimination theory for the p-adics, in terms of the power predicates, to the setting of the p-adic integers equipped with a vast array of functions defined by suitably convergent power series.

The consequences were dramatic. Van den Dries had been well aware of Gabrielov's work on subanalytic sets [133], and, indeed,it was its analogy with Tarski's real theory that led van den Dries to the basic insights about o-minimal theories [16, 9].Now one had a p-adic analogue, not merely of Gabrielov's notions, but of most of the main results, due to him, Hironaka and Lojasiewicz, about subanalytic subsets of real compacta. And, to cap a lovely achievement, they used the p-adic analogue to give a new treatment of the real case, entirely parallel to the p-adic.

The consequencs for the logic of exponentiation were many. On the one hand,one now hoped that the real exponential was o-minimal, a hope realized by Wilkie in 1991. For the p-adic exponential, I was able to prove a model completeness result in a language enriched by p-adic trigonometric functions (this owed a lot to ideas of van den Dries). I made essential use of the fact that the Galois group of the p-adics is small.

Some natural possibilities were not explored. The analytic functions from the p-adic case are defined also on the maximal unramified extension of the p-adics, but no one seems to know exactly what goes on with their elementary theory.The difference with the p-adics is that one has given up local compactness. Another issue only partially explored is the uniformity in p of the results. Yet another, only now sorted out, is the issue of cell decomposition in the p-adic analytic setting [84].For o-minimal theories, cell decomposition, and more refined results, are part of the general theory.

As van den Dries and Denef observed, their results do nevertheless give rationality results for p-adic integrals based on analytic data. This proved very important for group theory, notably in the work of du Sautoy. . But as far as I know, one has not yet mastered uniformity in p for the analytic situations.

What was done, and it is very hard, and arguably as important as the p-adic case, was a metamathematical analysis of rigid analytic problems. Here one works on the completion of the algebraic closure of the p-adics, in the sophisticated world first understood by Tate.The key players were the respective pairs Lipshitz-Robinson and Gardiner-Schoutens. I do not have time to say more. Fortunately there is a detailed account in the Asterisque volume of the first pair [77, 78, 80, 81, 79] .We still await applications of this beautiful work.Note that there is an important paper [8] where one uses elementary results from the rigid case to get uniformity in "minimality" for the $p - adic$ analytic case.

Schanuel's Conjecture

Schanuel formulated his conjecture in 1960 in a complex setting.It says:

If $\lambda_1, \ldots, \lambda_n \in \mathbb{C}$, and are linearly independent over $\mathbb{Q}$, then

$$\mathbb{Q}(\lambda_1, \ldots, \lambda_n, \exp(\lambda_1), \ldots, \exp(\lambda_n))$$

has transcendence degree $\geq n$

It seems to explain all transcendence results about exp known or hoped for. The reader new to it should try to derive the transcendence of e, pi and so on.

I was aware for a long time that some aspects of this had to be faced in the setting of Tarski's problem on the decidability of real exponentiation. After all, if exp(e) is algebraic, there will be a sentence of Tarski's language expressing the particular equation it satisfies over Q. If one is confronted with a statement that exp(e) is a root of a particular equation, how does one settle this? Easily,if exp(e) is transcendental. But otherwise,how? There is a p-adic Schanuel's Conjecture too, relevant to the decidability of the p-adic exponential.

Since Ax is already marked as a hero of our subject, it seems reasonable to draw attention to his beautiful proof, using differential algebra, of a power series analogue of Schanuel's conjecture [106, 107, 108, 109, 110].

Van den Dries and I, in the early days of work on exponentiation, considered various universal algebraic issues around E-rings, that is, rings equipped with a map E satisfying

$$E(0) = 1, \quad E(x + y) = E(x)E(y).$$

We proved various completeness theorems for identities, in complex, real, and p-adic settings.I showed,with more effort, that Schanuel's Conjecture implies that the E-subring of the reals generated by 0 is free on no generators [97]. And I proved the p-adic analogue. This gives a decision procedure for testing if two exponential constants are equal, something not known unconditionally. In any case, it is a tiny contribution to the original Tarski problem.

In 1980 Hovanskii [126] raised our hopes by giving uniform bounds, across families, for the number of connected components of quantifier-free definable sets in the real exponential field. The methods were ultimately Morse-theoretic. Over the next decade Wilkie worked patiently to make this yield the o-minimality of real exponentiation. He first proved a series of model completeness results for restricted Pfaffian fragments of the real field [25]. Curiously,it turned out that Gabrielov [122] could remove the Pfaffian assumptions here, but in fact the details of Wilkie's proof were to be crucial in the denouement of the Tarski problem. One striking thing he discovered was that , as in the original real case, and in the p-adic case, definable sets,though not quantifier-free definable, have etale covers which are defined by exponential equations. And it is this feature which brings Schanuels's Conjecture into the denouement.

The final push by Wilkie, to deal with unrestricted exponentiation, used the accumulated wisdom of o-minimality and some valuation theory not very far from the classical ideas on immediate extensions. The conclusion was model completeness,and thereby o-minimality , for the real exponential field [7] showing in particular that it is nonGödelian. In effect this was the solution of the Tarski Problem, in the terms that now dominated the subject. But it certainly did not yield decidability, and for this Schanuel's Conjecture proved crucial.

Decidability

Wilkie's final proof depended on his earlier one for restricted exponentiation. In neither proof is there an explicit axiomatization of the theory that is proved model complete. Even more surprisingly, once one had an axiomatization one did not see directly that it was equivalent to a universal-existential axiomatization (as it is, since it is modelcomplete). Wilkie and I [3] looked carefully at what axioms are being used, first for the restricted case. The logic of the situation is particularly interesting. We found, unconditionally, a recursive set of axioms which is modelcomplete. These axioms schemata involve natural principles, but their syntactic complexity is daunting. For the unrestricted case ,we do not directly find a recursive set of modelcomplete axioms. The point is that to get such we need first to have a complete set of axioms for the restricted case, and for this we need the universal theory of the restricted case to be decidable. Schanuel's Conjecture gives us that. We put solvability questions into the special etale form alluded to above;

$$\begin{aligned} \exists\, x_1, \ldots, x_n \quad [F_1\,(x_1, \ldots, x_n, \exp(x_1), \ldots, \exp(x_n)) &= 0 \\ \ldots \\ F_n\,(x_1, \ldots, x_n, \exp(x_1), \ldots, \exp(x_n)) &= 0 \\ \\ \wedge \qquad \text{Jacobian of } (F_1, \ldots, F_n) \text{ at } \bar{x} \text{ is} \neq 0 \;\;] \end{aligned}$$

where the F_i's are polynomials over $\mathbb{Q}$ in $x_1, \ldots, x_n, y_1, \ldots, y_n$.

Note that we are asking about the presence on an algebraic set in affine 2n space of a point on the (multidimensional) graph of exponentiation. Note the close formal similarity to the axioms for generic automorphisms. The nonsingularity assumption, coming from the etale condition, forces the graph point to have dimension no more than n over Q. But Schanuel forces the dimension to be no less than n. So we get a direct dimension estimate for the point sought. After that, fairly routine considerations involving our Newton Approximation Scheme (Hensel's Lemma in effect!) allow us to decide if there is a point or not. In this way we end with a complete, recursive, model complete set of axioms for the restricted case. Then we readily find a recursive model complete set of axioms for the unrestricted case, and use work of Wilkie/Ressayre to show this complete, giving decidability.

Real exponential-algebraic numbers

There is an idea implicit in our thinking [3] concerning the notion of algebraic in a real exponential setting. Since we do not make it explicit there, and no one has done so in print subsequently, it makes sense to make a digression on this matter.

Hardy had shown that exponential polynomials in one variable have only finitely many real roots. Note that this is a real phenomenon, failing in the complex situation. This might tempt one to define exponential-algebraic reals as being roots of such exponential polynomials over Q. But this appears not to be the right notion, because we are not able to show that the points of transverse intersection of two 2-variable exponential polynomials (Hovanskii proved that there are only finitely many such points) have their coordinates exponential algebraic in the proposed sense. On reflection the right notion is to define exponentially algebraic tuples to be tuples solving a Hovanskii system,and then define exponentially algebraic elements to be coordinates of such tuples. Using the fine detail of Wilkie's 1991 work,one can show that the exponentially algebraic elements form the prime model of the theory of real exponentiation. Moreover, Schanuel's Conjecture implies that this prime model is a computable exponential field. And finally, using Schanuel's Conjecture one shows that pi is not in this field! One certainly does not know how to prove this unconditionally.

The p-adic case

We have not got nearly so far in our analysis of the p-adic exponential. In 1990 I gave a model-completeness proof using p-adic trigonometric functions as extra primitives. But one does not know axioms, nor has one identified any analogue of the etale phenomenon. I was able to give effective upper bounds for the number of roots of unary polynomials, and one knows from Denef-van den Dries that there is a theorem of Hovanskii type, but no effective bounds are known. Maybe one should turn to the rigid case at this point.

Zilber's ideas on the complex exponential

My final topic, unfortunately to be described only briefly, is Zilber's programme [157, 158, 159, 160] concerning the complex exponential. This begins with the observation that Schanuel's Conjecture for the complexes is of the following form:

$$\delta(\bar{x}) \geq 0,$$

where

$$\delta(\bar{x}) = t.\,d.\,\mathbb{Q}\,(x_1, \ldots, x_n, \exp(x_1), \ldots, \exp(x_n)) - l.\,d.\; < x_1, \ldots, x_n >,$$

t.d. is the *transcendence degree* and *l.d.* is the *linear dimension.*

One had seen things like this quite often since the mid 1980's, in connection with stable and simple theories originally. Hrushovski [10, 12, 17]had used such "predimensions" δ to construct exotic models by an ingenious variation on the classical Fraisse method. The method is really a natural extension of various forcing methods used by the Robinson school in the 1970's. Using the predimension on small (usually finite) relational structures one develops a notion of strong morphism. This is not quite a first-order notion, but it has good limit properties. In fact it has, like many other forcing notions, a simple definition in a fragment of infinitary logic (Keisler's book on this,and related later papers [130, 131] will give a good idea of what I mean). But really all that is involved is a poset condition, and one gets generics, etc, more or less routinely. There are differences, of course. For example, the method does not construct existentially closed models in the old Robinsonian sense, but rather an analogue relative to the category of strong embeddings. It turns out that the role of universal-existential formulas is take over by Boolean combinations of existential formulas.

There are a number of very striking examples where the method leads to omega-stable elementary classes, notably in the area of generic curves and generic functions, which even have analytic models [66].

Zilber considered certain fields with exponential (just the obvious axioms) and considered the class of Hrushovski generics for the Schanuel predimension. In fact, for technical reasons he considers various sorted variants of exponential fields, but this is not really the point.

The essential point is that he considers models of the Schanuel infinitary axiom,which is in effect an omitting-types axiom,together with an infinitary axiom forcing the kernal of exp to be standard(this is the only way to avoid Gödelian phenomena) and then constructs existentially closed models by the Hrushovski method. One then identifies $L_{\omega_1,\omega}$ axioms, formally related to those for ACFA,specifying which systems of equations are solvable.Essentially the axioms,very close to first-order,say that systems of exponential equations have solutions unless they formally conflict with the functional equation for exp.

The final restriction is to impose an $L_{\omega_1,\omega}(\mathbb{Q})$ condition,corresponding to the fundamental analytic property of the complexes,that zero-dimensional analytic varieties in affine space are countable.This condition has the effect of forcing a tight structure theory for prime models in the now restricted category of models.What is most striking is that this is model theory of Shelah type for abstract elementary classes [135, 127](with origins in work of Keisler from around 1970).Such infinitary model theory has had little impact till now on algebra or geometry,though of course Shelah's work on prime models for stable theories was very important for differentially closed fields.

What is most interesting is not that one gets the existence of models in all cardinalities, but that one has a prime model technology, and in particular one has uniqueness of prime models over independent sets. Further, one has a minimality phenomenon for the class of prime models over independent sets, showing in particular that definable sets are either countable or of maximal cardinality.

Zilber speculates that in particular one gets the complex structure in this way,as the canonical model in power continuum. Of course this will be hard to prove, as it implies the Schanuel Conjecture(we are forcing the Schanuel condition to hold).In fact,it implies much more that one would love to have,for example the so-called Conjecture on Intersection with Tori (and it all relates to Ax's work).It is an inspiring picture,and it is also salutary from a more philosophical view,showing a way through a Gödelian world towards deep results in definability.

Zilber's (latest) conjecture implies that the set of reals is not definable in the complex exponential field.This is not known unconditionally.It also implies that the complex exponential field has automorphisms other than the identity or complex conjugation,an issue raised long ago by Schanuel and Mycielski.

A Final remark about Axioms

It is instructive to see how the shape of the core axiom systems has changed over 40 years.Robinson's axioms were always formulated in terms of solving equations in a single variable,and this kind of formulation prevailed through the AKE revolution.For regularly closed fields,one was obliged to use axioms about higher-dimensional varieties, though Geyer allowed one to restrict to planar curves. At various times in the 70's I looked at extensions of the theory of differentially closed fields to other theories of fields with derivation, and I found it natural to formulate corresponding axioms in terms of varieties of higher dimension and their "transforms" under the derivation. A more elegant version of this was presented by Pierce and Pillay in [142].

The axioms for ACFA are of this form too,giving consistency conditions for the multidimensional graph of the automorphism *sigma* to meet a subvariety of the product of a variety and its "transform". And Zilber's axioms are of this form too, for exp, but with a mildly infinitary flavour.

There are other cases where one has given detailed metamathematical analyses without using such formulations,but where it may well be valuable to continue to search for such axioms. Obvious examples are the real exponential field,and the Witt Frobenius [92].

Finally,I did not have time to consider important examples such as the ring of algebraic integers,where one has a local/global principle as axiom [23, 146],or the ring of adeles, which I hope to treat with the students on my project.

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