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ABRAHAM ROBINSON, 1918 - 1974

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1. Abraham Robinson died in New Haven on April 11, 1974, some six months after the diagnosis of an incurable cancer of the pancreas. In the fall of 1973 he was vigorously and enthusiastically involved at Yale in joint work with Peter Roquette on a new model-theoretic approach to diophantine problems. He finished a draft of this in November, shortly before he underwent surgery. He spoke of his satisfaction in having finished this work, and he bore with unforgettable dignity the loss of his strength and the fading of his bright plans. He was supported until the end by Reneé Robinson, who had shared with him since 1944 a life given to science and art.

There is common consent that Robinson was one of the greatest of mathematical logicians, and Gödel has stressed that Robinson more than any other brought logic closer to mathematics as traditionally understood. His early work on metamathematics of algebra undoubtedly guided Ax and Kochen to the solution of the Artin Conjecture. One can reasonably hope that his memory will be further honored by future applications of his penetrating ideas.

Robinson was a gentleman, unfailingly courteous, with inexhaustible enthusiasm. He took modest pleasure in his many honors. He was much respected for his willingness to listen, and for the sincerity of his advice.

As far as I know, nothing in mathematics was alien to him. Certainly his work in logic reveals an amazing store of general mathematical knowledge. He was always on the lookout for new areas where logic might contribute, and his insights here were very fine. I have a fond memory of the last colloquium he attended at Yale. Lipman Bers had spoken on a method of compactification in the theory of Riemann surfaces. As we walked back to our offices afterwards, Abby told me with a little smile that he would be looking at that more closely in the light of his general method for compactification. At that time he was thinking about nonstandard geometry, so this could have been interesting. But he got no chance to think this matter through.

My intention here is to survey Robinson's life as a logician. Unfortunately, this involves the neglect of a considerable part of his research until the mid 1950's. For one of the strangest aspects of Robinson's life is his dual career as logician and applied mathematician. The distance between these disciplines makes an informed survey by one individual almost impossible. Fortunately, Professor Alex Young, in an obituary for the London Mathematical Society [Y], has provided a fascinating account of the applied mathematician. For this reason, I will make only a few remarks about Robinson the applied mathematician, in the course of an introductory sketch of his entire life.

2. Biographical Outline. Robinson was born on October 6, 1918, in Waldenburg, Germany (now Walbrzych, in Polish Silesia). His parents were Abraham Robinson and Hedwig Lotte. Abraham was the younger of their two children. His brother, Saul, was also richly talented. He was an authority on comparative education, a former Director of Education for UNESCO, and at the time of his sudden death in 1972 was Director for Education of the Max Planck Institute in Berlin.

Abraham Robinson senior died in 1918 before the birth of his son. He was a writer who had studied in Berlin and London where he had earned a doctorate in philosophy. (Earlier, he had studied chemistry in Zurich.) In 1918 he was appointed as the head librarian of the Hebrew National Library in Jerusalem, but he died before assuming the position. He had never seen Palestine.

Robinson's mother was a teacher. Under her guidance he was raised to a deep and informed love of art. In 1944 he married an artist, Reneé Kopel of Vienna. Their last home together, in the woods of Sleeping Giant near New Haven, shows their taste for light and calm.

Not too much is known of Robinson's early years. Reneé Robinson has some diaries and notebooks, containing poems and plays, suggesting a sensitive observant child with an ambition to write.

In 1933 Mrs. Robinson and her two sons emigrated to Palestine. Abraham became a brilliant student, and in 1935, at the age of seventeen, he began to study mathematics at the Hebrew University of Jerusalem. His teacher was the revered Abraham Fraenkel, founder of the outstanding Israel school of mathematical logic. Robinson's first paper, published in 1939 [**1**], on axiomatic set theory, shows clearly Fraenkel's influence.

In 1939 Robinson won a scholarship to the Sorbonne, but the outbreak of war put an end to his few months of study in France. After the Nazi invasion of France in 1940 Robinson was lucky to escape to England, on one of the last boats out of Bordeaux.

And now came a complete transformation of scientific activity. We can see from the first papers [**1**], [**2**], and some unpublished material available at Yale, that around 1940 he was a young mathematical logician with a considerable aptitude for abstract algebra. Now, very rapidly, he became an applied mathematician with an international reputation in aerodynamics.

The sequence of events was thus. On arrival in England, Robinson enrolled in the Free French Air Force, but because of his mathematical qualifications was soon seconded to the Royal Aircraft Establishment at Farnborough. From Professor Young's account [Y], one learns that Robinson applied himself singlemindedly to learn structural mechanics and aerodynamics, with outstanding success. It is worth noting too that he learned to fly at this time, to increase his understanding of the practical problems involved. Most of his work was of high security classification and the little that was published emerged only after the war. Professor Young gives a detailed analysis of Robinson's achievement from the war years till 1968 when he published his last paper on wave propagation [82]. The young logician of 1940 was by 1945 a world authority on delta wings and supersonic flow, and would later co-author a standard book on wing theory [R3].

In 1945 he served as a Scientific Officer in occupied Germany. In 1946 he was appointed Senior Lecturer in Mathematics at the newly formed College of Aeronautics at Cranfield. There he developed his ideas on wing theory and supersonic flow, while beginning his amazing journey back to mathematical logic. In this period he obtained the degree of M. Sc. from Jerusalem, and in 1949, he completed his Ph.D. thesis on *The metamathematics of algebraic systems*, under the direction of Professor P. Dienes of Birkbeck College, London University. This became his first book *On the metamathematics of algebra.*

From this point on, Robinson was a world authority in mathematical logic, and particularly in the theory of models. Yet he continued to work at high level in applied mathematics. From 1951 until 1957 he was first Associate Professor and later Professor of Applied Mathematics at the University of Toronto. From there issued a unique series of contributions to logic and wing theory, including two books on the former and one on the latter.

Thereafter he published only two papers related to aerodynamics, although Young makes clear that he maintained a strong interest in the area.

In the last twenty years of his life he gave logic many new ideas of compelling simplicity, that were to prove relevant in many areas of mathematics. Let me just mention some of these ideas now, and postpone a discussion till later. They are

(i) Model completeness in algebra, with a simplified solution of Hilbert's 17th Problem, and the invention of differentially closed fields;

(ii) Nonstandard analysis, with applications to Hilbert space, number theory, potential theory, complex analysis, economics, and quantum field theory;

(iii) Forcing in model theory, with applications to algebra.

He wrote also on the philosophy of mathematics, mathematical education in schools, automata theory, differential equations and summability. He could talk entertainingly about anything, and he was a courteous listener.

He and Reneé loved travel, and had seen much of the world. At the end he regretted not seeing Australia (at the last moment he had to decline the invitation to the meeting there in January 1974).

He delivered lectures in German, Hebrew, French, Italian and Portuguese. But he didn't speak Russian, and it is sad that when he and his great Soviet counterpart, Mal'cev, met in Moscow, communication was very limited. Mrs. Robinson has told me that Abby learned a little Welsh, but I confess that I can scarcely imagine that lilting language in Abby's inimitable accent.

From 1957 until 1962 he was Professor of Mathematics (and Chairman of the Mathematics Department) at the Hebrew University, and from 1962 until 1967 he was Professor of Mathematics and Philosophy at U.C.L.A. From 1967 until his death he was Professor of Mathematics at Yale, and Sterling Professor since 1971.

He was in constant demand as a speaker at meetings, and he could be relied on to produce new ideas each time. In fact he told one of his students that it was good for him to get an invitation, as it forced him to do something new. One should for this reason regret that he did not live to accept his invitation to give a one hour address at the International Congress in Vancouver, 1974. This was an honor he greatly appreciated, and as he prepared for it in late 1973 he had many nice possibilities for his theme.

He received many honors, culminating in (posthumous) membership of the National Academy of Sciences in April 1974. In 1972 he was elected a Fellow of the American Academy of Arts and Sciences. From 1968 to 1970 he was President of the Association for Symbolic Logic, and on retiring in 1970 he delivered a challenging series of problems to the world's logicians. In 1973 he was the second recipient of the Brouwer Medal of the Dutch Mathematical Society.

Robinson was a strong, disciplined, wise man. Until his last months, he enjoyed good health. He loved the social life of mathematics, but when writing something up he insisted on a regular daily production.

Only a strong person could have produced what he did, eight books and over 110 published papers. He taught a seminar at Yale until less than two months before his death. He finished a paper on Russell's theory of descriptions. He received his many visitors cheerfully.

He is buried in Jerusalem.

3. Robinson's work in logic. To understand the unique character of Robinson's work in logic, it is useful to reflect on what was known in logic around 1948. I will take account only of those ideas which seem relevant to Robinson's later work.

3.1. There were few results known, but those were of high quality. The language of first order logic had evolved to a form that was generally used, and the fundamental results of its syntax and semantics were known. In 1930 Gödel [**G1**] had proved the Completeness Theorem for countable logics, and from this follows the Compactness Theorem, that a set of sentences has a model if and only if each of its finite subsets has. In 1936 Mal′cev [**M1**] extended the result to the uncountable case, but his work remained unknown in the West, until its rediscovery by Henkin [**H**] and Robinson [**R1**].

The most dramatic results in all of logic were (and are) Gödel's Incompleteness Theorems of 1931 [**G2**]. But prior to 1948 these results had little impact on algebra and number theory. In 1948 Markov [**Mar**] and Post [**P**] obtained the unsolvability of the work problem for semigroups. The two outstanding unsolvability results, the work problem for groups [**N3**], and Hilbert's 10th Problem [**Mat**], were still ahead.

Skolem [**S**] had found nonstandard models of number theory, by a construction which later emerged as the ultrapower construction. A simple proof was overlooked until 1948, when Henkin [**H**], fresh from his new proof of the Gödel Completeness Theorem, could get a two line proof by the device of adjoining new constants to the language and using compactness. The method is already explicit in Mal′cev in 1936 [**M**], which I believe to be the first occurrence of the diagram method that Robinson employed systematically.

3.2. Logic was not then in a position to give much help to algebra, but two very important results were known. These were Tarski's work on real closed fields [**T**] and Mal′cev's method for local theorems in group theory [**M2**]. The former was of basic interest to Robinson throughout his career as a logician.

In the 1930's Tarski had obtained a quantifier elimination procedure for the fields **C** and **R**. (These results were not published until 1948.) Tarski's method works for algebraically closed fields and real closed fields respectively. There are interesting consequences.

THEOREM. (i) *Any two algebraically closed fields of the same characteristic satisfy exactly the same sentences of the language of field theory.*

(ii) *Any two real closed fields satisfy exactly the same sentences of the language of ordered fields.*

This enables one to *transfer* cheaply results from **C** and **R** to other fields. Thus, it was then known that any finite dimensional (not necessarily associative) division algebra over **R** has dimension a power of 2. By transfer, one gets the result for an arbitrary real closed field. When the optimal result was proved by Bott and Milnor, that the dimension is 1, 2, 4 or 8, this transferred, although the original proof did not.

3.3. One rather basic fact is that in Henkin [**H**] the use of nonstandard models of second order logic is firmly established. One did not have a compactness theorem for second order logic with the standard semantics, but one could get a "fake" compactness theorem by allowing nonstandard models in which set quantification meant not quantification over *all* subsets of a set, but quantification over some prescribed subcollection of subsets of a set.

The point I am making is that in the Skolem and Henkin works one already had *all* the metamathematical concepts needed to establish nonstandard analysis. But more than a decade would pass before Robinson established the subject and found interesting applications. A similar situation will be seen in connection with forcing.

3.4. *Model completeness.* Robinson's early work between 1949 and 1955 centers round the method now known as Robinson's Test [**R5**].

He obtained new proofs of Tarski's theorems on algebraically closed and real closed fields. The proofs are less constructive than Tarski's. They use on the one hand the Compactness Theorem, and on the other basic algebraic facts concerning algebraic closures and real closures. Robinson's proofs seem easier to generalize than Tarski's (this will be confirmed by the history of the Ax-Kochen-Ersov Theorems).

The main idea Robinson detected to be common to algebraically closed and real closed fields is that of a closure operation on a class of models. He generalized this to arbitrary classes of structures, in his notion of model completion. But much of the success of his analysis here comes from the metamathematical reformulation. Instead of looking for theorems about existence and uniqueness of closures, he concentrated on a closure operation on sets of axioms. (Only in the 1970's was Shelah **[Sh1]** able to find general theorems about uniqueness of closures.)

Suppose $\mathfrak{M}$ and $\mathfrak{N}$ are $\mathfrak{L}$-structures, and $\mathfrak{M} \subseteq \mathfrak{N}$. We define

$$\mathfrak{M} \prec \mathfrak{N}$$

to mean that if $\Phi(v_0, \ldots, v_{n-1})$ is any $\mathfrak{L}$-formula, and $\mathfrak{M}_0, \ldots, \mathfrak{M}_{n-1} \in \mathfrak{M}$, then $\mathfrak{M} \models \Phi(m_0, \ldots, m_{n-1}) \Rightarrow \mathfrak{N} \models \Phi(m_0, \ldots, m_{n-1})$.

Tarski had in fact showed that if $\mathfrak{M}$, $\mathfrak{N}$ are real closed fields and $\mathfrak{M} \subseteq \mathfrak{N}$ then $\mathfrak{M} \prec \mathfrak{N}$. This is because any Φ is, by Tarski, equivalent in real closed fields to a quantifier-free formula, and then the proof is immediate. Tarski's proof used Sturm's Theorem essentially, to eliminate the quantifiers. Robinson's proof did not use Sturm directly, but instead relied on the results of Artin-Schreier [A-S] on existence and uniqueness of real closures for ordered fields (of course, Artin-Schreier used Sturm).

The key to the success of Robinson's method is Robinson's Test. To explain this we need a metamathematical notion, $\prec_1$, which is to be thought of as a generalization of "is relatively algebraically closed in". ($\prec$ was known in the 1930's to Tarski, but the algebraically more natural $\prec_1$ was singled out by Robinson.) We define $\mathfrak{M} \prec_1 \mathfrak{N}$ in the same way as we defined $\mathfrak{M} \prec \mathfrak{N}$, with the restriction that now we consider only existential formulas Φ, i.e. formulas $(\exists t_1) \cdots (\exists t_k)\Psi$ where Ψ is quantifier free.

Obviously $\mathfrak{M} \prec \mathfrak{N} \Rightarrow \mathfrak{M} \prec_1 \mathfrak{N}$, and the converse is false. But Robinson's Test gives a beautiful global connection between the notions.

THEOREM (ROBINSON'S TEST). *Suppose Σ is a set of $\mathfrak{L}$-sentences. The following are equivalent*:

(I) *For all models $\mathfrak{M}$, $\mathfrak{N}$ of Σ, $\mathfrak{M} \subseteq \mathfrak{N} \Rightarrow \mathfrak{M} \prec \mathfrak{N}$;*

(II) *For all models $\mathfrak{M}$, $\mathfrak{N}$ of Σ, $\mathfrak{M} \subseteq \mathfrak{N} \Rightarrow \mathfrak{M} \prec_1 \mathfrak{N}$.*

Robinson saw that II could be established for many natural sets of axioms Σ, and he did so in his first publications in model theory.

The concepts invariably used in these early applications were:

(i) Diagram of a model;

(ii) Simple extensions of a model;

(iii) Some algebraic facts about closures.

(i) occurs already in Mal'cev's 1941 paper. It is essentially a generalization of the notion of the multiplication table of a group. If we want a model of Σ in which a given $\mathfrak{N}$ is embedded, we have to find a model of $\Sigma \cup$ Diagram($\mathfrak{N}$). The latter is got by adding to $\mathfrak{L}$ constants $\bar{n}$ for each n in $\mathfrak{N}$, and forming all atomic (resp. negated atomic)

$$R(\bar{n}_1, \ldots, \bar{n}_j) \quad (\text{resp. } \neg R(\bar{n}_1, \ldots, \bar{n}_j))$$

for each $n_1, \ldots, n_j$ such that

$$\mathfrak{N} \models R(n_1, \ldots, n_j) \quad (\text{resp. } \mathfrak{N} \models \neg R(n_1, \ldots, n_j)).$$

Robinson, probably more than any other model theorist, made systematic use of this device.

(ii) arises thus. Suppose $\mathfrak{M} \subsetneq \mathfrak{N}$, and $x \in \mathfrak{N}$. In many cases we have a notion of the substructure of $\mathfrak{N}$ generated by $\mathfrak{M}$ and x, written $\mathfrak{M}(x)$. This is called a simple extension of $\mathfrak{M}$.

Let us look briefly at Robinson's proof for real closed fields. Let Σ be the first order axioms for real closed fields. We have to show that if $\mathfrak{M}, \mathfrak{N} \models \Sigma$ and $\mathfrak{M} \subseteq \mathfrak{N}$ then $\mathfrak{M} \prec \mathfrak{N}$. It suffices, by the above theorem, to show that $\mathfrak{M} \prec_1 \mathfrak{N}$. If not, we can get a counterexample with $\mathfrak{N}$ of finite transcendence degree over $\mathfrak{M}$. Since $\prec_1$ is transitive, we can assume that the transcendence degree is 1. So $\mathfrak{N}$ is the real closure of $\mathfrak{M}(x)$ for some x in $\mathfrak{N}$. But (Artin-Schreier) the real closure of $\mathfrak{M}(x)$ is determined up to isomorphism over $\mathfrak{M}$ by the cut x makes in $\mathfrak{M}$. Let $\Phi(\vec{m})$ be an existential sentence false in $\mathfrak{M}$, true in $\mathfrak{N}$. Then

$$\text{Diagram}(\mathfrak{M}(x)) \cup \Sigma \vdash \Phi(\vec{m}).$$

By Compactness, a finite part of the diagram suffices. But any finite part only places x in an interval of $\mathfrak{M}$, and so has a solution in $\mathfrak{M}$. So,

$$\text{Diagram}(\mathfrak{M}) \cup \Sigma \vdash \Phi(\vec{m}).$$

Therefore $\mathfrak{M} \models \Phi(\vec{m})$, a contradiction.

In his books **[R1]**, **[R5]**, **[R6]** he applies this method to various kinds of abelian groups. But in those early days there were no really striking algebraic applications of the method.

I think that the main importance of his ideas here was that they guided others to some very deep applications of logic to algebra. It is natural to try to extend the method from **R** to $\mathbf{Q}_p$, the field of p-adic numbers. To do this one has first to systematize a theory of closures for valued fields. This already existed in special cases **[K]**. Ax-Kochen **[A-K1]**–**[A-K3]** and Ersov **[E1]** gave metamathematical analysis of completeness and model completeness for valued fields (using either Robinson's method or an ultraproduct variant), and from this Ax and Kochen "solved" the Artin Conjecture about the fields $\mathbf{Q}_p$. One should note that it is much harder to approach these problems by the method of quantifier elimination, although this was done subsequently (Cohen **[C]**).

3.5. In his address to the 1950 International Congress he gives some general philosophical considerations on the use of symbolic logic for algebra, and gives some applications, essentially of the Compactness Theorem. None of these individually are of special importance. The main feature is that logic immediately brings order to a collection of isolated results. Here one sees Transfer Theorems of the form of a Lefschetz Principle, that if a first order sentence Φ holds in all fields of characteristic 0 then it holds in all fields of characteristic p for all but finitely many primes p. This is of course the style of the very important Ax-Kochen Theorem. I believe that Robinson's early work pointed clearly the way to such results.

An interesting aside in the 1950 paper concerns proving a version of Hilbert's Irreducibility Theorem, just from Compactness and some elementary lemmas. No proof is given at this point, but in 1955 Gilmore and Robinson [33] gave a striking model-theoretic reformulation of the Hilbertian property of fields. In a recent Memorial Volume [**R**], Roquette gives a very attractive account of the method and its wide applicability. The Gilmore-Robinson paper is essentially a nonstandard formulation of a classical concept in diophantine geometry, and, as Roquette points of [**R**], "nonstandard methods seem to be suited to simulate "geometric" situations in number fields". This is exactly what happens in the Robinson-Roquette paper [**113**] on the Siegel-Mahler theorem.

3.6. *Nullstellensatz and ideals.* Two other basic ideas in his mind in those early days concerned generalizations of the Hilbert Nullstellensatz and of the very notion of ideal. The former would become a familiar theme in metamathematical analyses of algebraic systems, especially fields. The latter has not till now led to many interesting results, although he developed a natural theory of metamathematical ideals in his book [**R2**] and in later publications.

The form of the generalized Nullstellensatz is:

> Suppose $\mathfrak{M}$ is a substructure of a model of T. If Σ is a system of atomic and negated atomic formulas over $\mathfrak{M}$, and Σ is solvable in some model of T containing $\mathfrak{M}$ then Σ is solvable in all models of T containing $\mathfrak{M}$.

(N) holds for the theory T of algebraically closed fields, and contains the weak Nullstellensatz stated in Lang [**L1**]. Note that for fields any system Σ as above is equivalent (via à vis solvability) to a system of equations.

(N) does not follow just from model completeness of T. It needs quantifier elimination for T. As it turns out this is equivalent to the amalgamation property for $T_\forall$, the set of universal consequences of T.

(N) holds for the theory of real closed fields, and gives the real Nullstellensatz of Lang [**L2**].

Note that by Robinson's Test (N) will imply model completeness. In this way, using a pre-existing Nullstellensatz, Ersov [**E2**] obtained a model completeness result for separably closed fields.

Ideals. The idea behind his metamathematical theory of ideals is very simple. Suppose the ring R is given: Form Diagram (R). Adjoin a function symbol φ, together with axioms saying that φ is a ring homomorphism.

Let $X \subseteq R$. Consider the set Δ, where Δ is

$$\begin{aligned}&\text{Axioms of Ring Theory} \cup \text{Diagram}(R)\\ &\cup \text{“}\varphi \text{ is a homomorphism”}\\ &\cup \{\varphi(\overline{x}) = 0; x \in X\}.\end{aligned}$$

Let $X^* = \{y: \Delta \vdash \varphi(\bar{y}) = 0\}$. Then X^* is an ideal.

Instead of using a symbol for homomorphism, we might consider an ordering $<$, and the constraints $\bar{x} > 0$, $x \in X$. In this way we get a new kind of "ideal", which Robinson used (not essentially) in his analysis of Hilbert's 17th Problem.

Clearly the possibilities are unlimited, and the notion is connected with the notion of n-type used by Morley [**Mor**] and all subsequent workers. But I do not know of any really essential algebraic uses of the idea.

3.7. *Hilbert's* 17th *problem*. The first major success of Robinson's methods came in connection with Hilbert's 17th Problem. This had been solved by Artin in 1926 [**A**], building on the theory of real closed fields invented by Artin and Schreier. The problem is:

Suppose $f \in \mathbf{Q}(x_1, \ldots, x_n)$ and f is positive definite. Is f a sum of squares in $\mathbf{Q}(x_1, \ldots, x_n)$?

One has the same problem for $\mathbf{R}$.

In general, let K be a field with a unique field ordering, such that K is dense in its real closure. Then $K(x_1, \ldots, x_n)$ is orderable. Indeed, a beautiful lemma of Artin says that if $g \in K(x_1, \ldots, x_n)$ and if g is not a sum of squares in $K(x_1, \ldots, x_n)$ then there is an ordering $<$ of $K(x_1, \ldots, x_n)$ in which $g < 0$. This ordering restricts to the unique order on K. So we have

$$\begin{array}{ccc} K & \longrightarrow & K(x_1, \ldots, x_n) \\ \downarrow & & \downarrow \\ \widetilde{K} & \longrightarrow & \widetilde{K(x_1, \ldots, x_n)} \end{array}$$

where $\sim$ denotes real closure. After this Artin needed a complicated specialization argument. But if one uses model-completeness one is essentially there. For we have

$$\widetilde{K} < \widetilde{K(x_1, \ldots, x_n)}$$

But $\widetilde{K(x_1, \ldots, x_n)} \vDash (\exists y_1) \cdots (\exists y_n)(g(y_1, \ldots, y_n) < 0)$. So

$$\widetilde{K} \vDash (\exists y_1) \cdots (\exists y_n)(g(y_1, \ldots, y_n) < 0).$$

So

$$K \vDash (\exists y_1) \cdots (\exists y_n)(g(y_1, \ldots, y_n) < 0),$$

by density.

So g is not positive definite.

This style of argument would later be used essentially by Kochen [**Ko**] to obtain a p-adic analogue of the solution of Hilbert's 17th Problem, after Ax-

Kochen-Ersov found the model-completeness result for p-adic fields. Further uses are listed in my survey article [**Macl**].

3.8. *Bounds in the theory of polynomial ideals.* A related consideration in the mid 1950's concerned bounds in problems about polynomial ideals. For example, we know that if K is algebraically closed, and $f, g_1, \ldots, g_n \in K[x_1, \ldots, x_m]$, and f vanishes at all common zeros of $f, g_1, \ldots, g_n$ in K then some power of f is in the ideal generated by $g_1, \ldots, g_n$. So there exist $k, h_1, \ldots, h_n$ such that $f^k = \sum_{j=1}^{n} h_j g_j$.

The problem is the existence of bounds on k and $\sup_{1 \leqslant j \leqslant n} \text{degree}(h_i)$ in terms of $\sup(\text{degree}(f), \text{degree}(g_j), n)$.

From compactness one gets such a bound which is general recursive. See [**34**], [**35**]. Better bounds can be found by the methods of Hermann [**He**] or Kreisel [**Kr**], but both are harder to obtain.

The same sort of considerations apply to bounds in Hilbert's 17th Problem, and membership problems in the theory of ideals [**R2**]. Surprisingly, the apparently simple membership problem of testing whether $f \in [g_1, \ldots, g_n]$ was not solved by model theory until much later, by Robinson in [**101**]. (The proof uses a very neat compactness trick.) Many challenging problems remain open in this area, notably the Ritt Problem in differential algebra [**104**].

3.9. In the mid 1950's he obtained a number of theoretical results, notably the important Joint Consistency Lemma which is equivalent to Beth's Theorem [**36**]. He also obtained a useful criterion for elimination of quantifiers.

In a paper published in 1959 [**50**] he solved a problem from Tarski's classic paper, by obtaining a completeness theorem for the theory of real closed fields with distinguished dense proper real closed subfield. For this he used Robinson's Test and the proof is quite hard. It can be simplified by the use of ultraproducts, then just coming into fashion.

3.10. *Model completion.* A unifying idea in Robinson's work is that of the model completion T^* of a theory T. We say that T^* is a model companion of T if any model of T is embeddable in a model of T^*, and vice versa, and T^* is model complete. A nice result of Robinson [**88**], proved by a union of chains argument, is that T^* is unique, if it exists. (Not till the late 1960's did people look at cases where T^* does not exist.) If T has the amalgamation property (or, equivalently, T^* has elimination of quantifiers) we call T^* the model completion of T.

The possibilities may be seen in the following table:

T	T^*	Model Completion?
Theory of fields	Theory of algebraically closed fields	Yes
Theory of ordered fields	Theory of real closed ordered fields	Yes
Theory of formally real fields	Theory of real closed fields	No
Theory of groups	Does not exist [ ]	–

The idea is to provide an "algebraic closure" for *theories*. As remarked before, there are no elementary general theorems on algebraic closures of *models*. One should also point out the curious lack in the mid 1950's, at least among model theorists, of a definition of algebraically closed structure. (For groups, a definition had been given in [**Sc**].)

3.11. *Differentially closed fields.* In terms of applications to algebra, 1959 was important for the beginnings of the model-theoretic study of differential fields [**51**].

Robinson looked at T, the theory of differential fields of characteristic 0, and, by straightforward use of pre-existing elimination theory due to Seidenberg [**Se**], he proved that T has a model completion T^*. He called the models of T^* differentially closed fields of characteristic 0. He pointed out an interesting gap in the analogy with algebraically closed fields, namely that one had no known notion of the differential closure of a differential field. Nor did one have really intelligible axioms for differentially closed fields. Moreover, one had no corresponding result in characteristic p.

Later workers resolved most of these problems, using more recent methods initiated by Morley [**Mor**]. In characteristic p, Wood [**W1**] showed that there is a model companion which is not a model completion. In her 1968 thesis [**B**], Blum showed that the theory of differentially closed fields of characteristic 0 is ω-stable, and found a nice set of axioms. From ω-stability there follows the existence of a prime differentially closed extension $\tilde{F}$ of any differential field F of characteristic 0. ($\tilde{F}$ is prime in this sense if for any other differentially closed K with $F \to K$, this map factors through $\tilde{F}$.) By a 1972 result of Shelah [**Sh1**] $\tilde{F}$ is unique, so deserves to be called the differential closure of F. By a result of Kolchin, Rosenlicht and Shelah [**Sh2**], $\tilde{F}$ is not minimal. Except for minimality, corresponding but more delicate results were found in characteristic p by Shelah (existence and uniqueness of $\tilde{F}$ [**Sh2**]), and Wood (existence of $\tilde{F}$ [**W2**]).

This is an example where logic aids algebra to establish needed concepts. So far there are no applications of the model theory of differential fields, except a mild Nullstellensatz [**Sa**].

3.12. *Nonstandard analysis.* In 1960 there appeared his first paper on nonstandard analysis [**60**]. This was followed by a series of papers, a section in his *Model theory*, and the book *Nonstandard analysis*. He was quickly followed into the new area by a group of analysts, notably Luxemburg. In the 16 years since it appeared, the method has interacted successfully with analysis, algebra, number theory, topology, mathematical economics, theory of Brownian motion, and quantum field theory. (See [**IJ**].) Already, the method is offered in some universities (M.I.T., Wisconsin) as an alternative to the (ε, δ)-method of Weierstrass.

Of all his achievements, this is the one best known outside logic. To a mathematician it offers an elegant calculus, suggesting new ideas, and it applied to all branches of mathematics. To a philosopher, it provides a firm foundation for the use of infinitesimals as entities in calculus. In the 300 years since Leibnitz and Newton this had not been done. Now, by model theory, we have this, and a vast generalization.

Many proofs and constructions become simpler, in the sense that complicated limiting processes can be replaced by more intuitive nonstandard-finite discrete processes. A good example is the recent work of Loeb on potential theory **[Lo]**. In a few cases new results were found, notably the Bernstein-Robinson theorem on polynomially compact operators **[72]**. (In this case, of course, the result has been superceded by conventional techniques.)

Of course, nonstandard analysis depends only on the existence of ultrafilters, and so anything proved using it can be proved without it. But the proofs might become unintelligible. Gödel **[G3]** remarked that simplification facilitates discovery, and for this reason we may expect a bright future for nonstandard analysis. There is, I believe, a general feeling that we ought to have infinitesimals as entities, just as we have the complex numbers.

Probably only someone of Robinson's immense erudition could have conceived the utility of establishing nonstandard analysis (the logical tools had been around for a while). He rapidly made clear that nonstandard analysis will simplify definitions and results from elementary calculus to advanced complex or functional analysis.

There are nowadays other methods of nonstandard analysis, also with much to contribute. Scott and Takeuti **[Sc]**, **[Ti]** have made clear the power of Boolean models in analysis, and Mulvey **[Mul]** has done the same for sheaf-theoretic models in algebra. Robinson himself, in his last years, gave considerable thought to generic arithmetic as a new nonstandard method.

3.13. *Number theory.* From 1967 until his death, he was very active in introducing nonstandard methods to number theory. His first efforts are concerned with the interpretation of the Krull topology and profinite groups, and are altogether natural **[77]**. Gradually he developed a nonstandard treatment of the class field theory, essentially using nonstandard approximation theory.

In **[103]** a new development takes place. The idea is that if a curve $\mathcal{C}$ over a field K has infinitely many points then in an enlargement K^* $\mathcal{C}$ has a nonstandard point, which is actually a generic point of the curve. Thus we may associate to $\mathcal{C}$ a function field $K(\vec{x})$ over K, with $K(\vec{x}) \subseteq K^*$. In this way Robinson gives a concrete connection between algebraic number fields and algebraic function fields. This is of course extremely suggestive, and in his final major work **[113]** the idea is used to give a nonstandard approach to the Siegel-Mahler Theorem. There is a direct connection back to the 1955 paper with Gilmore. It is to be hoped the next few years will see a systematic development of this beautiful idea of geometrizing number fields by model theory.

3.14. *Forcing.* In 1969 **[93]** Robinson adapted P. J. Cohen's technique of forcing for use in model theory. He was certainly not the first to try this, but it seems to have been a common experience to reach the conclusion that nothing nontrivial was to be expected. The exception, until 1969, was Reyes

[Re] who anticipated Robinson's discovery of infinite forcing (but in a version prima facie very different from Robinson's, and less easily used).

What Robinson did was to associate to a first order theory T various notions of forcing. The forcing conditions were fragments of diagrams of models of T. If we allow only finite fragments we get finite forcing, and if we allow arbitrary fragments we get infinite forcing. The notion

$$p \Vdash \Phi \quad (p \text{ forces } \Phi)$$

is defined, following Cohen, in the obvious way. One gets two notions to study:

(a) Generic structures, where forcing and satisfaction coincide;

(b) The theory of the generic structures.

There is a satisfactory version for uncountable languages, but let us confine ourselves to countable languages where everything is smoother. For either notion of forcing, generic structures exist, and though they may not be models of T are models of $T_{\forall\exists}$, the universal-existential sentences of T. Moreover, the theory of the generic models (known as the forcing companion of T) has the same universal part as T. Let T^f, T^F be the forcing companions for finite and infinite forcing respectively. It turned out later that, for some important T, $T^f \neq T^F$ [Mac2], but Barwise-Robinson [88] established the following satisfactory connection between forcing and model-completion:

THEOREM. *If T has a model companion T^* then $T^* = T^f = T^F$.*

This gives several interesting ways of making total the map $T \mapsto T^*$.

These investigations coincide with a very overdue definition of existentially closed structures [E-S] by members of Robinson's group at Yale. An $\mathfrak{L}$-structure $\mathfrak{M}$ is T-e.c. (T-existentially closed) if $\mathfrak{M} \models T_\forall$ and whenever $\mathfrak{M} \subseteq \mathfrak{N} \models T$ then $\mathfrak{M} \prec_1 \mathfrak{N}$. These exist in profusion, and are well-known classically in special cases. For example, in field theory they are the algebraically closed fields.

The connection with forcing is that generic structures are T-e.c. Moreover, T has a model companion if and only if the class of T-e.c. structures is an EC_Δ [E-S]. Eklof and Sabbagh [E-S] showed that group theory has no model companion. Later associates of Robinson used forcing to show the complexity of T-e.c. structures, when T is the theory of groups or skew fields [Mac2], [H-W]. But in both cases positive results could be got too. All told, this development created a surge of interest in model-theoretic algebra, and tied model theory more closely to recursion theory. Robinson directed an impressive series of doctorates at Yale. Some of the results are conveniently found in [H-W]. There was an element of serendipity here, as the results of P. M. Cohn [Co], Matejasevic [Ma], and Robinson [93] (all presented at the 1970 Nice Congress), fitted nicely together.

One subject from which Robinson expected more was generic arithmetic [108]. From conversations I know that he expected this to have applications to number theory. The generic structures do not satisfy full induction, but they are probably adequate for all "natural" number theory, and they have a natural form of algebraic geometry.

One can see much in common between Robinson's roles as creator of nonstandard analysis and creator of model theoretic forcing. In both cases the key idea was already around, but no one else had been able to see any use for the method. Robinson had an uncanny instinct for finding the applications.

3.15. *Topological model theory.* One of his projects in the last years was to find a formalism for the metamathematical analysis of topological algebras. He published only one paper on this ([**105**], for his admired friend Mostowski). The paper seems to me of an exploratory character, with many provocative ideas dimly emerging. The results concern definability and "generic" considerations in algebras, and may well have links to forcing. Other younger researchers (Bankston, Garavaglia, McKee, Makowsky, Sgro, Ziegler) have gone in a different direction. The future is not clear, but progress will probably be best guaranteed by following Robinson's practice of reflecting on the interesting living examples of topological algebras.

3.16. *Metamathematical problems.* Robinson, in his retiring address as President of the Association of Symbolic Logic, left a list of metamathematical problems which should guide future progress in model theory. There are of course the classical unsolved problems on decidable fields (he deeply admired both the positive results of Ax-Kochen-Ersov, and the negative result of Davis-Putnam-Robinson-Matejasevic). There are suggestions for topological model theory. There are speculations on the numbers/functions mystery in number theory, to which he may have left us a key. There are intriguing remarks on using Baire category rather than compactness in model theory (this is all linked with forcing and topological model theory). And, typically, Abby was not afraid to go on record as taking seriously absolutely undecidable problems.

4. Epilogue. My intention has been to survey Robinson's work in logic, and to show the extraordinary potential of the simple ideas he first put forward around 1950.

I want to close with a quotation from an influential French general: "The unity which it [axiomatic method] gives to mathematics is not the armor of formal logic, the unity of a lifeless skeleton: it is the nutritive fluid of an organism at the height of its development, the supple and fertile research instrument to which all great mathematical thinkers since Gauss have contributed, all those who in the words of Lejeune-Dirichlet, have always labored to substitute 'ideas for calculations'."

The armored enemy is illusory. The instrument is now finer, after Abraham Robinson.

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Abraham Robinson

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Other Relevant Material

Obituary

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