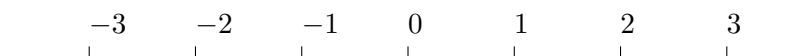


Model Theory as Peacock's Revenge

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If you have studied Calculus from an American university textbook, you have almost certainly seen the diagram



The diagram represents the real numbers (in other words the numbers with decimal expansions), arranged in order from smaller to greater. The more strenuous textbooks add some other numbers to the diagram, for example e a little to the left of 3 and π a little to the right of it. These two numbers are irrational; in other words they can't be written as m/n where m and n are integers. Probably the first irrational number to be discovered was $\sqrt{2}$, the square root of 2, somewhere between 1 and 2 in the diagram. Medieval mathematicians regarded irrational numbers as substandard and used insulting names for them, such as 'surd', which is the Latin for deaf-mute.

A long time ago people realised that negative real numbers don't have real square roots; for example no real number is a solution of the equation $x^2 + 1 = 0$. In 1545 Girolamo Cardano published a formula for calculating solutions of cubic equations. The cubic equation

$$x^3 - 7x + 6 = 0$$

has solutions $x = 2, 1$ and -3 , but Cardano's rule for solving it requires us to use the square root of -60 during the calculation. This is not a special case; Cardano's rule involves square roots of negative numbers whenever the cubic has three distinct real roots. Square roots of negative numbers were even more suspect than irrational numbers, so they became known as 'imaginary'. But Cardano's rule showed that we need them.

By the end of the eighteenth century mathematicians had worked out sensible ways of handling imaginary numbers. They considered complex numbers $a + b\sqrt{-1}$ where a and b are real. They knew how to add, subtract, multiply and divide complex numbers, and how to picture them in the plane. Cotes and Euler had established some laws involving them, for example

$$e^{\pi\sqrt{-1}} = -1.$$

Cauchy was about to extend the integral and differential calculus to complex numbers, and he would show that this extended calculus gives powerful methods for handling the real numbers themselves.

Not everybody was happy with these developments. If -1 doesn't have a square root, then the whole theory of complex numbers is based on a pretence. Some people (for example Augustus De Morgan's father-in-law William Frend) seized the opportunity to point out that you can't take something away from nothing, so negative numbers are a pretence too. How to answer these objections?

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In 1834 George Peacock [10] proposed an answer. Unlike earlier mathematicians, he didn't distinguish between numbers that exist and numbers that don't. Instead he distinguished between symbols on the one hand, and their interpretations on the other. The symbol 1 is just a mark; we usually interpret it as standing for the number one. This is a subtle distinction, since both the symbol and the number are written the same way. But the rest of our story depends on it.

Here is one of Peacock's remarks about his distinction:

To *define*, is to assign beforehand the meaning or conditions of a term or operation; to *interpret*, is to determine the meaning of a term or operation conformably to definitions or to conditions previously given or assigned. It is for this reason, that we *define* operations in arithmetic and arithmetical algebra conformably to their popular meaning, and we *interpret* them in symbolical algebra conformably to the symbolical conditions to which they are subject. ([10] p. 197 footnote)

What is Peacock saying here? What does he mean by 'beforehand' and 'previously'?

Peacock's idea is this. We have the natural numbers $0, 1, 2 \dots$, and we can introduce symbols that stand for natural numbers and operations on natural numbers. Then we can use these symbols to write down laws (Peacock's 'conditions') that are true for the natural numbers. For example $x + y = y + x$; when we add two natural numbers, the order doesn't matter. Another law is

$$x + (y - z) = (x + y) - z.$$

This doesn't always make sense for the natural numbers; for example $1 + (1 - 4)$ is not a natural number. But whenever both sides do make sense, the law holds, so we count it as a law of the natural numbers. This is a case of 'beforehand', because the natural numbers came before the laws; we discovered the laws by studying the natural numbers.

On the other hand, take the laws above and forget that they came from the natural numbers. Instead, regard them as referring to the complex numbers and the operations $+$, $-$ etc. on complex numbers. Then again they come out true. This time we have 'interpreted' the symbols and the laws that go with them. The laws came first (Peacock's 'previously'), then the interpretation. Strictly of course we could choose to use the natural numbers again instead of the complex numbers, and so the natural numbers are also an interpretation. But they have a privileged position as the original meanings of the symbols; in today's language Peacock would have said that they are the 'intended interpretation' of the symbols and the laws.

The advantage of the complex numbers over the natural numbers (said Peacock) is that the operations make sense more generally. In the natural numbers we can consider $\sqrt{4}$ but not $\sqrt{3}$. In the real numbers $\sqrt{3}$ exists but $\sqrt{-3}$ doesn't. In the complex numbers $\sqrt{-3}$ exists. Another way of putting this is to say that as we proceed from the natural numbers to the integers, the rational numbers, the real numbers and the complex numbers, more and more equations become solvable. The complex numbers are a natural stopping place, because in them every algebraic equation with at least one variable has a solution, as Gauss had proved in 1799. But they are not necessarily the end of the series, because

There is no necessary limit to the multiplication of such signs.
([10] p. 201)

For example we might add exponentiation, 2^x . Are there equations in this operation that have no solution in the complex numbers but could have solutions added to the complex numbers?

Peacock also observed that we can do a third thing: forget the natural numbers, don't provide a new interpretation, but simply calculate with the

symbols according to the laws. He called this ‘symbolical algebra’, as opposed to the ‘arithmetical algebra’ that studies the natural numbers. He related symbolical algebra to interpretations by observing that if we prove something in symbolical algebra, what we prove must be true in any interpretation of the symbols and laws. If our interpretation makes the laws true, then it also makes true anything else that follows from the laws.

Peacock was ahead of his time. His immediate successors developed his symbolical algebra, but they sidestepped two other parts of his picture. First they ignored the interpretations; if we know how to calculate consequences of our laws by symbolical algebra, what does it add to say that the laws can be interpreted? (George Boole in 1847 was an honourable exception. He refined Peacock’s notion to introduce the concept of a ‘system of interpretation’; we will meet it again below.) Second they argued that there are worthwhile systems of symbols and laws that don’t come from the natural numbers; for example in 1844 William Hamilton invented quaternions, in which the law $xy = yx$ is simply false. Actually Peacock himself should have noticed that the law $\sqrt{x^2} = x$ is true for the natural numbers (wherever it makes sense) but false for the real numbers; take $x = -1$.

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Peacock’s notion of interpretations never really died out, but it was kept alive by a different kind of question. In the first half of the nineteenth century the geometers were entering their own crisis of split meanings. Geometry had previously been about the line, the flat plane and three-dimensional space. Now, thanks to Gauss and Riemann, one had a geometry of the surface of the sphere or a cylinder, and some strange imaginary spaces of ‘negative curvature’, and any number of other possibilities. So it was natural to ask which systems of geometric propositions imply which other geometric propositions. The old question whether Euclid’s parallel postulate was derivable from his other postulates was just one example of this kind of question. A methodology developed for answering such questions. Suppose we have a system T of propositions that hold for a certain space, and we want to know whether these propositions imply some other proposition ϕ . One way of showing that they don’t is to provide a Peacock-style reinterpretation of the system of propositions T , and show that under this reinterpretation the proposition ϕ is false. By the 1920s these reinterpretations had become known as ‘pseudospaces’ or ‘pseudomodels’. To

do their job they didn't even need to be spatial; they could be abstract or numerical.

By this date George Peacock was long since forgotten. Nobody discussed his views on the privileged position of arithmetic, or his idea of adding new elements to a structure to make more equations have solutions. Peacock himself left his post at Cambridge University to become Dean of Ely Cathedral in 1839—apparently he hadn't been a popular lecturer—and he died in 1858 leaving no offspring. But his fundamental distinction between symbols and interpretations grew steadily in importance. One reason for this was that mathematicians had begun to consider classes of 'structures'. A structure consists of a set of objects and certain labelled operations and relations on them; typical examples are groups and fields.

Essentially a structure is a system of interpretation in Boole's sense. The set of objects of the structure provides the range of allowed interpretations for the variables. Boole suggested calling this set the 'universe', borrowing a term from the logic of his friend Augustus De Morgan. This name has survived, though the universe of a structure is also known today as its 'domain'. We speak of the elements of the universe as 'elements of the structure', and the 'cardinality' of the structure is the number of elements in its universe. The operations of the structure are (in Peacock's sense) interpretations of their labels. An 'isomorphism' from a structure A to a structure B is a bijective function f from the universe of A to the universe of B , so that if a relation of A holds between elements a_1, \dots, a_n of A , then the relation with the same label holds in B between the corresponding elements $f(a_1), \dots, f(a_n)$, and vice versa from B to A . (For this definition, relations include equations.)

Though the concept itself is older, strictly the name 'structure' is twentieth-century. Structures were known in the late nineteenth century as 'systems', or more pedantically 'systems of objects' (in German *Systeme von Dingen*) to distinguish them from the 'systems of axioms' which were the descendants of Peacock's 'conditions'.

Peacock hadn't thought in terms of systems/structures. For him the world of mathematics consisted of numbers, and the processes that he described were about how to add new kinds of number. But in the new framework the natural numbers were a structure, the integers another, the rational numbers another, the reals another and the complex numbers another again. I will refer to this sequence as 'Peacock's hierarchy'. Strictly, to make the real numbers into a structure we have to decide what operations we include; the set of symbols for these operations is then called (in modern terminology) the 'signature' of the structure. For example the natural

numbers might have a signature consisting of the symbols $+$, \times , 0 and 1 ; passing to the integers we can add $-$ to the signature, and so on.

In terms of structures one could also separate two parts of Peacock's picture. We 'expand' a structure by keeping its elements the same but adding new operations or relations. We 'extend' a structure by keeping the signature the same but adding new elements. The resulting new structure is called an 'extension' of the old one and the old one is called a 'substructure' of the new. We write $A \subseteq B$ to express that the structure B is an extension of the structure A . Passing from the natural numbers to the complex numbers involved both expansions and extensions.

Peacock's fundamental distinction between symbols and their interpretations got further reinforcement from the realisation in the late nineteenth century that 'conditions' (or propositions or axioms) don't have to be equations. Any true sentence about the natural numbers, even Fermat's Last Theorem, can be regarded as an axiom if we can write it precisely using only the allowed formal symbols. In the late 1910s David Hilbert's lectures at Göttingen gave popularity to the idea that any sentence of first-order logic could be taken as an axiom, at least if it was consistent. Besides the equality, variables and function and constant symbols that Peacock allowed, first-order languages permit symbols for relations, together with

\neg 'not', \wedge 'and', \rightarrow 'if ... then', \forall 'for all', \exists 'there exists'.

Grumpy old men like Gottlob Frege made peevish remarks about idle generalisations and misuse of the word 'axiom', pretending not to realise that the new freedoms were bound to stimulate new methods for handling them.

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If we allow our axioms to be any first-order sentences, then one of Peacock's claims goes out of the window. It's by no means true that all the axioms that hold for the natural numbers hold for the larger number systems, even if we restrict the operations to plus and times. For example the first-order sentence

$\forall x (x^2 \neq 2)$ ('For all x , x^2 is not equal to 2')

says that whatever number we take, its square is not 2. This is true in the natural numbers; also it's true in the integers and in the rational numbers.

But it's false in the real numbers and the complex numbers. Likewise the sentence

$$\forall x (x^2 + 1 \neq 0)$$

expresses that -1 has no square root; this is false for all Peacock's number systems except the largest, the complex numbers.

There is a pattern here. The two sentences above both have the form

$$\forall x_1 \dots \forall x_n \phi(x_1, \dots, x_n)$$

where the formula $\phi(x_1, \dots, x_n)$ is 'quantifier-free', i.e. contains neither \forall nor \exists (and of course in the two examples above, $n = 1$). A sentence of this form is said to be 'universal'. If you have three structures $A \subseteq B \subseteq C$, and a universal sentence is true in B , then it must be true in A too, but it need not be true in C . Dually, an 'existential' sentence is one of the form above but with \exists in place of \forall . An example is

$$\exists x (2 \neq 0 \rightarrow 2 \times x = 1)$$

which says that there is an element x such that if 2 is not zero then 2 times x is 1. (We said it this way in order to bring the 'there exists' to the front.) The sentence is false in the natural numbers and false in the integers, but true in the rationals and so on upwards. In fact if $A \subseteq B \subseteq C$ are structures and an existential sentence is true in B , then it must be true in C too but need not be true in A .

The process that Peacock described, adding new elements so that operations make sense in more places, adds to the existential sentences that are true, and takes none away. (This is true for extensions for the reason given in the previous paragraph. It's also true for expansions, since passing to an expansion never makes a true sentence false, though it gives meanings to more sentences and so may generate new true sentences.) So we can reasonably ask: Is this a general procedure? Can we start with any kind of structure and add new elements so as to maximise the number of existential sentences that are true? A number of people (including Henri Poincaré) speculated about this question in general terms. The group theorist W. R. Scott showed in 1951 that if we start from an arbitrary group instead of the natural numbers, and form extensions so as to increase the number of true existential sentences not using \neq or \rightarrow , we reach a natural stopping point called an 'algebraically closed group', which stands in roughly the same relationship to the original group as the complex numbers do to the natural numbers.

Then in 1962 Michael Rabin showed how to carry out the same idea, starting from any structure A at all. We take a set T of universal sentences true in A . The construction makes repeated extensions, so as to reach a structure B in which the sentences of T are all still true, but if C is any extension of B in which the sentences in T are all true, then the existential sentences true in C are just those true in B . Structures with this property of B , given a particular set T of universal sentences, are said to be ‘existentially closed’ for T . An important result proved by Rabin was that anything we can say about elements of B using an existential formula (the same as an existential sentence, but allowing free variables) can also be said using a set of universal formulas. More precisely, one can prove from T the equivalence of the existential formula and the conjunction of a set of universal formulas. In general this set might be infinite.

Now in 1931 Kurt Gödel proved a famous theorem about CFA, the set of first-order sentences true in the natural numbers using just plus and times as operations. (CFA stands for ‘complete first-order arithmetic’.) In modern terms, he showed that no computer could ever be programmed to list all and only the sentences in CFA. So CFA is enormously complicated. By contrast Alfred Tarski showed in 1949 that the corresponding set for the complex numbers is very simple; he described a simple set of sentences whose first-order logical consequences are exactly the first-order sentences true in the field of complex numbers. Tarski and his students looked at the other structures in Peacock’s hierarchy. Tarski showed that the real numbers have a well-behaved set of true sentences, like the complex numbers. His student Julia Robinson showed that the natural numbers can be defined inside the rational numbers by first-order formulas, and it follows that the set of true sentences in the rationals is as badly behaved as CFA for the natural numbers.

Rabin’s result showed why the complex numbers are well behaved in this sense. In them we can say with an existential sentence that a certain set of equations and inequations (i.e. statements that certain numbers are not equal) has a solution; this is a statement about the coefficients of the equations. By Rabin’s result, this statement about the coefficients can be rewritten with universal formulas. In fact, because of certain pleasant properties of fields, it can be rewritten as a formula with no quantifiers \forall or \exists at all. This allows us to ‘eliminate quantifiers’ from any first-order formulas about the complex numbers. A closer analysis shows that through this reduction we can deduce all true first-order sentences about the complex

numbers from a set of sentences of the form

$$\forall x \forall y \forall z \dots \exists u \exists v \exists w \dots \phi(x, y, z, \dots, u, v, w, \dots)$$

where again $\phi(x, y, z, \dots, u, v, w, \dots)$ is quantifier-free, and all the \forall 's are to the left of all the \exists 's. Sentences of this form are said to be 'universal-existential'. An example is the sentence

$$\forall x \forall y \exists u (u^2 + x.u + y = 0).$$

This sentence says that every nontrivial quadratic equation has a solution; it is true for the complex numbers by the theorem of Gauss that we mentioned earlier.

There is a similar quantifier elimination for the real numbers; in fact Tarski discovered it before the rather easier case of the complex numbers. How does one find a quantifier-free formula $\phi(x)$ that expresses the same as $\exists y (y^2 = x)$ in the field of real numbers? Tarski's solution was (essentially) to pass to an expansion which has a symbol \leq for the usual order relation on the real numbers. Then the required formula is $0 \leq x$.

Tarski's approach had nothing to do with existential closure. But it was reworked by Abraham Robinson, who showed that the field of real numbers is existentially closed for T , where T is a set of sentences expressing that the structure is a field in which -1 is not a sum of squares. (The axioms for fields are universal-existential because of the axiom saying 'Every non-zero element has a multiplicative inverse'. But the notion of an existentially closed structure generalises from universal to universal-existential sentences, and Rabin's results still hold in this more general case.)

How do things fare if we move further up the Peacock hierarchy, for example adding exponentiation? There has been a lot of work on this recently. The picture seems to be that the complex numbers are the best-behaved level. There are quantifier elimination theorems as we go up higher, but they become very complicated. Tarski himself asked around 1950 whether there is a quantifier elimination theorem for the field of real numbers with exponentiation. It was more than forty years before Alex Wilkie gave a positive answer.

We mentioned some 'pleasant properties' of fields. One of them is worth spelling out. Suppose \mathbf{K} is a class of structures. We say that \mathbf{K} has the 'amalgamation property' if whenever A, B and C are structures in \mathbf{K} with $A \subseteq B$ and $A \subseteq C$, there are structures D' and D in \mathbf{K} such that $A \subseteq B \subseteq D'$ and $A \subseteq C \subseteq D$, and there is an isomorphism from D' to D which is the identity on A . This is true for fields: if A is a subfield of B and C , then

there is a field extension of D with a field embedding $f : C \rightarrow D$ that is the identity on A . (So C' is the image of f in D .)

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But we have run ahead of the story. When Peacock wrote, his comments were part of a report on ‘certain branches of analysis’. What he said was about the whole organisation of mathematics, but there was no mathematical subject called the Organisation of Mathematics. By the time Rabin came to do his work, there was a branch of mathematics called Model Theory, and Rabin operated within this theory. True, model theory was invented as a kind of receptacle for results about interpretations of formulas, and it would never have been invented if the results hadn’t been there to collect. But the invention of a new discipline is a tale of its own, and we should tell it if only briefly.

Recall that according to Peacock there are two directions of thought connected with interpretations. In the first direction, we start with a particular structure A , we provide symbols for talking about A , and we assemble the facts about A that can be expressed with these symbols. In the second direction we start with a set T of symbolic sentences, and we consider what interpretations of the symbols will make the statements in T all true. Note that there is an asymmetry here: in Direction One we have a single structure and we go to many formal sentences, while in Direction Two we start with many formal sentences and we go to many structures.

It was more than a hundred years before anybody seriously challenged this asymmetry. You will find the asymmetry built into Tarski’s account in his textbook [13] from 1936, though with a very brief apology (on the turn of pages 119 to 120 in the 1994 edition). Historically this is odd, because three decades earlier a group of American mathematicians had already set to work on the proper way to use Direction One. Namely, they started with a class \mathbf{K} of related structures, for example the boolean algebras, and they wrote down axioms that are true in every structure in \mathbf{K} , and which define \mathbf{K} in the sense that every structure in which the axioms are all true is in fact in \mathbf{K} . The best known of these Americans are Edward Huntington (who handled the boolean algebras in 1904), Oswald Veblen and Charles Langford.

Tarski’s book was one of the last publications to be written in the old style. In the 1940s Anatolii Mal’tsev in Russia, Tarski in America and Abraham Robinson in Britain all switched to a new framework. On the one hand

we have classes \mathbf{K} of structures, on the other hand we have sets T of formal sentences. One can study the relation 'Every sentence in T is true in every structure in \mathbf{K} '. Through this relation, facts about first-order sentences can be translated into facts about structures. For example an early result of Mal'tsev in this vein was about groups that have faithful n -dimensional matrix representations, where n is a fixed positive integer. Mal'tsev proved in 1940 that if every finitely generated subgroup of a group G has this property then so does G . His proof showed how to deduce this from a more general result called the Compactness Theorem, which we will return to. Another early result in the field, also derived from the Compactness Theorem but closer to Peacock's interests, was proved independently by Tarski and Jerzy Łoś: Suppose ϕ is a first-order sentence, and suppose that for all pairs of structures $A \subseteq B$, if ϕ is true in B then it is also true in A . Then ϕ is logically equivalent to a universal sentence. This is a partial converse to a property of universal sentences that we mentioned earlier.

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To celebrate the new subject some terminology was fixed in the 1950s. A system (of things) now became a 'structure', and a set of formal sentences became a 'theory'. When a formal sentence ϕ is true in a structure A , one expressed this by saying that A is a 'model' of ϕ , not an 'interpretation' of ϕ as Peacock would have said. A structure A was said to be a 'model' of a theory T if it was a model of every sentence in T . The subject was called 'model theory'. (Rudolf Carnap had earlier suggested the name 'theory of systems', but this suggestion fell by the way.)

Peacock's own word 'interpretation' got a new meaning. In the first half of the twentieth century it became common to think of mathematics as happening inside set theory. So a structure A is a set-theoretic object, and when we use A to give meaning to the symbols of a formal language L , what we are really doing is translating from the language L into the language of set theory. Hence translations between formal languages became known as 'interpretations'. But now suppose we have a translation from language L to language L' , and we have a structure A whose language is L' . Then we can use the translation to define a structure A' whose language is L . Namely, the elements of A' are the same as those of A , and a relation is defined to hold in A' if its translation holds in A (and likewise for operations). In this case we say that A' is 'interpreted in' A . A slight generalisation allows us

to take the elements of A' to be equivalence classes of n -tuples of elements of A . For example, given the natural numbers \mathbb{N} we can define the integers \mathbb{Z} by an interpretation: the elements of \mathbb{Z} are the equivalence classes of ordered pairs (m, n) of natural numbers, where (m, n) is equivalent to (p, q) if and only if $m + q = n + p$, and the arithmetical operations on \mathbb{Z} are defined in terms of those of \mathbb{N} so that the equivalence class (m, n) behaves as we expect the integer $m - n$ to behave. Julia Robinson's definition of the natural numbers inside the field of rational numbers was a more complicated example of an interpretation in the new sense.

The name 'model theory' caught on and the subject spread fast. In fact it spread so fast that many people pronounced in the mid 1960s that the subject had exhausted itself. I remember being advised, as a DPhil student in 1966, to choose another area because model theory was dead. Ironically 1965 marked one of the decisive moments in the history of the subject, namely the invention of stability theory by Michael Morley. But again I run ahead of myself

With the new terminology in hand, we can state the Compactness Theorem. It says that if T is a set of first-order sentences, and every finite subset of T has a model, then T has a model. Gödel had proved this in his PhD thesis of 1930, but only for countable sets of sentences; independently Mal'tsev proved it in 1936 for arbitrary first-order languages. (This independence is not on public record yet; I recently learned from Lev Beklemishev that the Mal'tsev archives make it clear. Some years later, Abraham Robinson and Leon Henkin independently reached Mal'tsev's result by generalising Gödel's.)

By the 1950s it had become the fashion in pure mathematics, whenever one introduced a new kind of structure, to define a class of mappings between the structures. These mappings would be the ones that preserved features important in the study of the structures. Model theorists chose a class of mappings which they called 'elementary embeddings'. For simplicity I define only a special case here. Suppose A and B are structures with $A \subseteq B$. We say that B is an 'elementary extension' of A , and A is an 'elementary substructure' of B , if after adding symbols to name all the elements of A , every first-order sentence true in A is also true in B . For example if A and B are fields and B is an elementary extension of A , and a certain element a of A has no square root in A , then a has no square root in B either. So an important fact about Peacock's hierarchy was that the extensions involved were *not* elementary.

Some important general theorems were proved about elementary embeddings. One fact was this. Suppose A is an infinite structure (recall that

this means the universe of A is an infinite set), and suppose the first-order language for the signature of A has λ sentences. Then for every cardinality κ which is at least the cardinality of A and at least λ , A has an elementary extension of cardinality κ ; and for every cardinality κ which is at most the cardinality of A and at least λ , A has an elementary substructure of cardinality κ . This is a part of the grand theorem known by the grand name of ‘Upward and Downward Löwenheim-Skolem-Tarski theorem’.

The notion of elementary embeddings was important for Abraham Robinson. He said that a theory T is ‘model-complete’ if whenever A and B are models of T with $A \subseteq B$, B is an elementary extension of A . Model-complete theories always have a kind of quantifier elimination: every formula is equivalent to an existential formula. So we can eliminate the quantifier \forall ; we can eliminate \exists as well, but not both quantifiers at once.

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Around 1970 a certain description of problem solving became popular among cognitive scientists. This is not model theory, but it often happens that the same abstract ideas appear in several disciplines at about the same time; maybe there is some intangible cross-fertilisation. The picture was as follows. We have a collection S of ‘states’, which is called the ‘problem space’. Some of the states are picked out as ‘goals’; if we are in one of these then we have solved the problem. We are given ways of passing from one state to another, and one state is picked out as the start state. We solve the problem by starting from the start state and passing along the allowed routes until we reach a goal state.

For a model theorist a central problem is to construct a model of a given theory T . One way of doing it is (remember Peacock) to start with a small structure and form extensions, until we have all the elements we need. So take the ‘goals’ to be the models of T , and the ‘states’ to be the substructures of models of T . There is a technical device that allows us to expand the language of any first-order theory T in a way that makes T model-complete; it turns out to be convenient to do this here, so that whenever A and B are ‘goals’ with $A \subseteq B$, B is an elementary extension of A . Because of an amalgamation property of elementary embeddings, we can simplify the picture without losing any essential information, by imagining that all the ‘goals’ are substructures of a single very large model of T , known as the ‘big model’ or the ‘monster model’. This setting came into model theory in the 1970s, though it was a natural consequence of Morley’s 1965 paper.

There are a number of natural questions we can ask in this setting. For example, suppose we move through the problem space by adding elements one at a time (and then closing off under the functions of the big model so that we get a structure). When A is a ‘state’ (i.e. a substructure of the big model), two elements b, c of the big model are said to have ‘the same type over A ’ if the result $A(b)$ of adding b to A is indistinguishable from the result $A(c)$ of adding c . One can make this precise by saying that there is an isomorphism from $A(b)$ to $A(c)$ that is the identity on A and takes b to c . An equivalence class of elements under this relation is called a ‘type over A ’. We write $S_1(A)$ for the set of types over A . And now we can ask: is there some bound on the number of elements in $S_1(A)$? For example, must it have the same size as A itself? It must have at least the size of A , because we can form an extension $A(a)$ whenever a is an element that was already in A .

We can also ask how large the class of goals is. In the examples that were first studied, the goals are the elementary substructures of the big model, and by the Löwenheim-Skolem theorem there is at least one of these in every cardinality between the cardinality of the big model and that of the language, including the cardinalities at both ends. Under these assumptions there will be lots of goals. The strongest restriction we can hope for is that for each cardinality, all of the goals with that cardinality are isomorphic to each other. In this case we say that the theory is ‘categorical in power’. We say that the theory is ‘categorical in cardinality κ ’ if all the goals with cardinality κ are isomorphic to each other.

For example if the big model is an algebraically closed field, then its elementary substructures are its subfields that are algebraically closed. An old theorem of Steinitz says that two algebraically closed fields of the same characteristic are isomorphic if and only if they have the same transcendence degree. It follows that the theory is categorical in every uncountable cardinality but not categorical in the cardinality ω of the set of natural numbers. A theory of this kind is said to be ‘uncountably categorical’, and its models are also said to be ‘uncountably categorical’.

Now, thanks largely to the work of Morley and its development by a number of people (notably Bill Marsh, Alistair Lachlan, John Baldwin, Saharon Shelah, Daniel Lascar and Bruno Poizat), a whole string of unexpected connections between such questions came to light. If a first-order theory is uncountably categorical, then for each infinite substructure A of the big model, $S_1(A)$ always has the same cardinality as A . If for each infinite substructure A of the big model, $S_1(A)$ always has the same cardinality as A , then various strong amalgamation properties hold, and various fea-

tures of models must be definable by first-order formulas. The details are complicated and often involve heavy combinatorics, but the interrelations that came to light were a positive hanging garden of Babylon. This subject became known as ‘stability theory’, for the not very convincing reason that in good cases, cardinalities stay stable as we pass from A to $S_1(A)$.

The first cases to be studied, for example that of algebraically closed fields of a fixed characteristic, were in first-order languages with a countable set of symbols. Morley’s Theorem in his 1965 paper had said that if a first-order theory in a countable language is categorical in some uncountable cardinality then it is uncountably categorical. Special techniques were needed for extending the structure theory of uncountably categorical structures to uncountable first-order languages, but it was achieved by Steve Buechler, Ehud Hrushovski and Chris Laskowski. Shelah studied broader generalisations, some of them quite remote from first-order languages. In one of his generalisations the class of structures is ‘excellent’; this is a technical condition which involves amalgamation properties in several dimensions. He showed that an excellent class satisfies Morley’s Theorem. ‘Abstract elementary classes’ are another of Shelah’s generalisations; in them even the notion of a language goes missing. Only mathematical mountaineers survive at this altitude.

§§§

We began with natural numbers, and we generalised them to integers, rational numbers, real numbers, complex numbers. For Peacock the natural numbers are the heart of mathematics, and these other generalisations are justified by their relationship to the natural numbers.

But then we generalised still further, and studied properties of models of arbitrary first-order theories. Shelah generalised further yet again, to classes of classes of first-order theories — a subject he very reasonably called ‘classification theory’, though its methods coincide with those of stability theory. In his broadest generalisations, even the restriction to first-order theories vanishes. We seem light years away from the constrained nineteenth-century world of Peacock, where everything revolves around the natural numbers.

Not so, said Boris Zilber. If we ask the right model-theoretic questions, we get straight back — not to the natural numbers, but at least to algebraically closed fields; the complex numbers are an algebraically closed

field. Zilber's claim is paradoxical. Fields are a rather specific algebraic notion, and there seems no reason why they should emerge from general model-theoretic considerations.

Zilber's proposal was to examine uncountably categorical first-order theories. In the 1980s he conjectured that every infinite uncountably categorical structure is equivalent under interpretation to one of the following structures, all of them important in classical mathematics: an infinite set, an infinite projective or affine space over a finite field (these are close relatives of vector spaces over finite fields) or an algebraically closed field. He also suggested, and in some cases proved, how we can tell which of these possibilities applies. The conjecture was false, as Ehud Hrushovski proved. But one could identify the reason for its failure: an algebraically defined topology called the Zariski topology is definable over algebraically fields but can't be reconstructed from pure model theory. Together Hrushovski and Zilber gave an axiomatisation of Zariski topologies and showed that Zilber's conjecture is completely true for 'Zariski geometries' (structures obeying their axioms).

This was less than a derivation of the notion of algebraically closed fields from purely model-theoretic notions, but it was more than anybody had expected before Zilber's conjecture. It was also good mathematics; Hrushovski applied his and Zilber's result to find a complete and uniform solution of the Mordell-Lang conjecture on function fields, a classical problem in diophantine geometry that had previously been solved only in some cases. More recently Anand Pillay and Martin Ziegler found a way to eliminate the Zariski geometries from Hrushovski's argument by using another idea from classical mathematics; but it was the Zariski geometries that were the key to the original solution of the problem.

Peacock had noted that we can pass to further extensions of the natural numbers by considering further operations. What happens if we consider exponentiation? There are some classical problems about the exponentiation function on the complex numbers, some of them reckoned very difficult. One of these is Schanuel's conjecture, which says that if complex numbers a_1, \dots, a_n are linearly independent over the field \mathbb{Q} of rationals, then the field $\mathbb{Q}(a_1, \dots, a_n, e^{a_1}, \dots, e^{a_n})$ has transcendence degree at least n over \mathbb{Q} . Within the last few years Zilber made a further breakthrough of a rather gnomonic kind. He wrote down a set of axioms for exponentiation functions on algebraically closed fields, including Schanuel's conjecture as a set of axioms. He showed that the resulting class of fields with exponentiation functions is an excellent class in Shelah's sense, and it is categorical in the first uncountable cardinal ω_1 . Therefore by Shelah's result quoted

earlier, this class is categorical in the cardinality of the field \mathbb{C} of complex numbers; so there is up to isomorphism just one way of adding an exponentiation function e^x to \mathbb{C} so as to get a structure in this class. People's hunches differ, but it looks a very reasonable conjecture that the usual exponentiation function e^x on \mathbb{C} (got by generalising from exponentiation n^x on the natural numbers) is just such a function. This conjecture implies Schanuel's conjecture.

In a hundred years or so we may know whether this conjecture of Zilber has paid off and led to a proof of Schanuel's conjecture. That would certainly give a new use for Peacock's description of arithmetic as 'the science of suggestion' ([10] p. 199).

§§§

George Peacock planted a seed, and today the tree that bloomed has become a thriving branch of mathematics. I have distorted it a bit by concentrating on Peacock's theme and its transformations. Model theory is much wider than this; like Walt Whitman it is large and contains multitudes. But I know I am not alone in feeling that these close interactions with classical mathematical structures provide a backbone to the subject.

In fact there are further connections between model theory and the field of real numbers. One is Abraham Robinson's Nonstandard Analysis, which constructs infinitesimals by passing to a large elementary extension of the real number field, so that we can calculate dy/dx by taking dy and dx to be infinitesimals very much as Leibniz did (Bell [1]). Another is the theory of o-minimal structures, founded by Anand Pillay and Charles Steinhorn after suggestions by Lou van den Dries, which generalises Tarski's quantifier elimination on the field of real numbers roughly as stability theory generalised properties of the complex number field, and has proved useful for solving classical problems (Van den Dries [3]).

There are several textbooks available, for example Manzano [8], Hodges [5], Marker [9], Pillay [12] in rough order of progression from elementary to research level. The most recent developments are still available only in research papers — for example Zilber [15] for his work on exponentiation in the complex numbers, Grossberg and Hart [4] for the background on excellent classes, and Wilkie [14] for his work on exponentiation in the real numbers. Techniques from model theory have spilled over into many other areas, for example complexity theory (Immerman [7]), software system design (Börger and Stärk [2]) and natural language semantics (Peters

and Westerståhl [11]). The definitive history has still to be written, but there are some notes on my website at [6].

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George Peacock
1791–1858



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