L. Henkin, et al. (Eds.), Proceedings of the Tarski Symposium, Proceedings of Symposia in Pure Mathematics, 25. Providence, RI: American Mathematical Society, 1974, pp. 465-479.

# THE AXIOMATIC METHOD IN THE EMPIRICAL SCIENCES[1]

PATRICK SUPPES

**1. Introduction.** My original intention was to give a talk primarily on the theory of models as applied to the theory of measurement in the empirical sciences. Conversations with Tarski earlier in the symposium changed my mind. We agreed that it would be desirable to have a survey, even if superficial, of the place of the axiomatic method and the theory of models in the empirical sciences. And so my present intention is to attempt such a survey. I recognize the difficulties, and consequently, ask in advance for your indulgence. The survey will necessarily be idiosyncratic—restricted to those topics I know something about. I shall begin with measurement, go on to physics and end with natural language. Throughout I shall try to emphasize open problems.

Before I begin, however, I want to record my own intellectual indebtedness to Alfred Tarski, both directly and indirectly. Not long after coming to Stanford in 1950, I began to attend Tarski's seminar in Berkeley. In 1951 J. C. C. McKinsey joined the faculty at Stanford, and in 1952 I taught a course in the philosophy of science at Berkeley, which included as undergraduate students Richard Montague and Dana Scott. The years of close contact with McKinsey and Scott have been of permanent significance to me, and it is fair to say both of them were probably influenced more by Tarski than by anyone else. When McKinsey and I first began to think about working on the axiomatic foundations of physics in 1951, we discussed our plans and line of attack with Tarski on more than one occasion, and his sympathetic advice and support were important to both of us. It is a pleasure to record on this most appropriate occasion my debt to Alfred Tarski and the value I place on our friendship of many years.

---

[1] This article was written while I held a Guggenheim Fellowship.

**2. Measurement.** The theory of measurement provides some of the simplest and clearest examples of the axiomatic method in the empirical sciences. In addition, the axiomatic tradition is very old in the theory of measurement. Large parts of ancient Greek geometry can be regarded as properly belonging to the theory of measurement, and what many would claim to be the first treatise on mathematical physics, Archimedes' treatise *On the equilibrium of planes*, is, in Book 1, primarily an axiomatic analysis of conjoint measurement in the case of balancing weights with unequal distances between the weights and the fulcrum.

From a logical standpoint, measurement is also a reasonably simple area of axiomatic applications, because when the basic set of objects to be measured is finite, then in most cases one can completely axiomatize the theory within ordinary first-order logic. Of course, once the basic set of objects is permitted to be infinite, then to obtain the standard representation theorem in terms of real numbers, an Archimedean or some equivalent axiom is required, which takes us beyond first-order logic.

From a mathematical standpoint there are two standard problems for a given theory of measurement. One is to find a set of axioms for which a representation theorem into the real numbers can be found that preserves structure in the appropriate isomorphic or homomorphic sense. The other is to determine the uniqueness of the representation, uniqueness in most cases up to some group of transformations, although in certain cases the set of transformations preserving the representation does not form a group.

Because of the logical simplicity of many theories of measurement, it is possible to obtain some explicit metamathematical results. An example of such results was reported in an article by Scott and me **[16]**. We proved that the theory of differences in utility preference or psychological intensity, for example of a tone, is not axiomatizable by a universal sentence. More explicitly, let us consider a nonempty finite set $A$ and a quaternary relation $R$ on this set such that $xy\ R\ uv$ if and only if the subjective psychological difference between $x$ and $y$ is equal to or less than that between $u$ and $v$, with due account being taken of the algebraic sign. We seek then for any such relational structure $\mathfrak{A} = \langle A, R\rangle$ a real-valued function $f$ defined on $A$ such that

$$xy\ R\ uv \quad \text{if and only if}\ f(x) - f(y) \leqq f(u) - f(v).$$

What Scott and I showed is that a finite number of universal first-order axioms on the relation $R$ cannot guarantee the existence of such a representation. Necessary and sufficient conditions, obviously not finite and first-order, can be found in an article by Scott **[15]**.

A rather exhaustive treatment of the axiomatic foundations of many parts of measurement is given in a treatise that has just appeared [7], but a number of fundamental problems remain open. From a conceptual standpoint perhaps the most interesting is the problem of satisfactorily characterizing in an elementary way simultaneously the theory of a procedure of measurement and the theory of its errors. The theory of errors of measurement has occupied some of the ablest mathematicians of the past. I mention, for example, Laplace and Gauss. But the modern axiomatic viewpoint has not yet been applied with any thoroughness to the conceptual foundations of the theory. The first task should probably be to characterize in qualitative terms the various classical probability distributions closely associated with the theory of error. I have done this for the geometric distribution in a recent paper **[17]**, but this is only a first step. The most important case is, of course, that of the multivariate normal distribution, and a thorough investigation of its qualitative axiomatic foundations does not, as far as I know, yet exist.

**3. Forces in classical physics.** It is now 20 years since McKinsey and I began working on the axiomatic foundations of physics. I remember well the argument we had with the editor of the *Journal of rational mechanics*, the irrepressible Clifford Truesdell, who was unhappy with the analysis of force we provided in our original axiomatization **[9]**. Truesdell felt that our characterization of forces as ordered triples of real numbers satisfying the obvious law of addition for such vectors did not provide any deep conceptual insight into the physical nature of forces. In this judgment I think he was right, but it does not mean that our axiomatization was wrong. The first problem in that early work was to get a set of axioms that were sufficient to characterize what is ordinarily regarded in physics as classical particle mechanics and to characterize it in a way that was logically and mathematically acceptable. Roughly speaking, this means that axioms were given that were mathematically self-contained without any assumptions about the mathematical nature of the objects being considered left implicit, as is often the case in the kind of axioms stated by physicists.

What is missing and what is needed in the analysis of forces is a kind of explicit analysis in terms of elementary primitives. Clearly the primitive notions that McKinsey, Sugar and I used were complicated and already put into the axioms a substantial mathematical apparatus. The axioms were not simple in the way that primitive concepts and axioms of geometry are simple. What is needed is an analysis of the concept of force in the style of Tarski's classic article, *What is elementary geometry*? **[21]**.

A recent interesting analysis mainly restricted to the special case of static forces has been given by Krantz **[6]**. His central concept is that of an equilibrium structure $\langle A, E, \oplus, *\rangle$, where $A$ is a nonempty set of configurations (a configuration determines the motion of a given material object), $E$ is the subset of configurations that produce no change in momentum, $\oplus$ is a binary operation on $A$ intuitively corresponding to the concatenation of two configurations, and $*$ is a binary operation from $R^+ \times A$ to $A$, where $R^+$ is the set of positive real numbers. In the case of a force table, the apparatus consisting of a table with a small ring around a center post and strings attached to the ring and running over pulleys clamped to the edge of the table with weights hanging from the strings, the meaning of the operation

$r*$ is clear: All the weights are multiplied by the positive real number $r$. Krantz's first axiom is that $\langle A, \oplus \rangle$ is a commutative semigroup. For $a$, $b$ in $A$ and $r$, $s$ in $R^+$ he postulates as the second axiom:

(i) $r * (s * a) = (rs) * a$,

(ii) $r * (a \oplus b) = (r * a) \oplus (r * b)$,

(iii) $(r + s) * a = (r * a) \oplus (s * a)$,

(iv) $1 * a = a$.

The remaining two axioms are axioms of superposition and equilibration. It is clear from the axioms I have stated that Krantz's formulation is quite close to a vector-space approach. His analysis is certainly a conceptual improvement over that offered in [**9**].

The outstanding work of Walter Noll [**11**] should also be mentioned, but his axioms on the concept of forces acting on bodies require a highly sophisticated and developed mathematical framework. We are still left with the problem of giving, even for highly simplified situations, an elementary axiomatization of the concept of force. Providing such an axiomatization seems to me one of the more interesting problems that remains open in the axiomatic foundations of classical mechanics. (I emphasize that I speak here of the *axiomatic* foundations; it is well known that a large number of mathematical problems remain open in classical physics. A good sense of the mathematical depth of a problem like that of stability is to be found in the treatise on the foundations of mechanics by Abraham [**1**].)

**4. Relativity.** Given the unbelievably large number of papers that have been written on the axiomatic foundations of Euclidean geometry and closely related projective and affine geometries, it is hard to understand why a geometrical characterization of forces has not been the focus of more studies. It is even harder to understand why the geometrical characterization of the space-time manifold of special relativity, which is so close in character to that of Euclidean geometry, has not been the subject of intense axiomatic investigation.

Although there have been a number of axiomatic investigations of the manifolds assumed in general relativity, I shall restrict my discussion to special relativity, because the axiomatic problems are closer to foundational investigations and contact with the logical theory of models is fairly direct. In the case of general relativity, a much richer mathematical apparatus is assumed, and consequently such connections are more remote and indirect. Some excellent recent studies of the axiomatic foundations of general relativity are to be found in [**4**] and [**8**].

The most extensive, and in many ways the deepest, qualitative investigations of the axiomatic foundations of special relativity are still to be found in the work of Alfred A. Robb. He published his first results in 1911 and authored a book on the theory of time and space in 1914 (a second edition appeared in 1936 [**13**]). From a foundational standpoint Robb's axiomatic work is especially interesting, because he uses the single binary relation of *being after* between two space-time points. The adequacy of this single primitive concept is surprising in light of Tarski's general mathematical results of many years ago that no nontrivial binary relations can be defined in Euclidean geometry [22]. The intuitive idea of Robb's approach is simple and natural. One space-time point is after another if the first is in the forward light cone of the second. In other terms, this means that the two points are connected by a possible inertial path of a particle. Robb worked out in complete detail the geometry of this partial order relation. Of course, two points that are spatially connected in the usual terminology of special relativity cannot be related by the *after* relation. Robb developed the whole theory much in the spirit of Forder's book on the foundations of Euclidean geometry [3]. I do not mean to suggest that Robb was influenced necessarily by Forder, but they both wrote in the same geometrical spirit and shared the same defects of exposition and formulation.

Robb's development of the theory is in several respects unsatisfactory. First, the axioms are of a complex character. It seems evident that Robb formulated axioms as he needed them in the deductive development of the theory, but he did not then attempt a serious simplification and reduction of their number. Second, no explicit representation theorem is proved, even though it is evident that this can be done from the results that Robb does prove, and *a fortiori*, no uniqueness theorem about the representation is proved. The uniqueness theorem was proved in independent and separate fashion much later by Zeeman [25] in a beautiful paper that shows Robb's primitive is adequate for the derivation of the Lorentz transformations. If Klein's Erlanger program had been carried through for special relativity, Zeeman's results would have been proved essentially at the same time as Robb's. It is a reflection of the primitive state of axiomatic discussions of special relativity that such a long gap exists between the two; in fact, Zeeman's results were proved, I think, independent of knowledge of Robb's earlier work.

A good discussion of Robb's axiomatization may be found in [2] and an alternative approach in terms of a symmetric binary relation of signaling in Latzer [8]. However, we still need the kind of foundational and metamathematical investigation of the geometry of special relativity exemplified so well for Euclidean geometry in Tarski's classic article. We do not have a first-order formulation of axioms for the geometry of special relativity, and there seem to be no metamathematical results of any sort as yet in the literature.

**5. Classical quantum mechanics.** A surface view of quantum mechanics, even classical quantum mechanics, seems to present a bewildering array of problems, special solutions and techniques that have little hope of being put into any orderly fashion from the standpoint of axiomatic foundations. Where detailed mathematical studies have been made of special parts of quantum mechanics, it would appear that the apparatus of modern functional analysis is sufficiently essential and powerful to make it impossible to look at any significant problems in quantum mechanics from a genuinely foundational standpoint, e.g., from the standpoint that metamathematical questions of any kind can be asked.

I do not think this is a true picture. I think it is possible to abstract various pieces of the substructure of quantum mechanics and to ask axiomatic questions of interest about these substructures. Indeed, most of the present audience will immediately think of the applications of multivalued logics to quantum mechanics and will perhaps be familiar with the early discussions of Reichenbach [**12**], the more recent work of Kochen and Specker [**5**] and the more mathematically oriented work of Varadarajan [**23**], [**24**]. Also, I have tried to say something myself about the need for a nonclassical logic of quantum mechanics [**20**].

I would like to state what seem to be some interesting open problems that are amenable to logical attack and also to relate some qualitative results to be found in [**7**]. In order to exclude joint probability distributions of conjugate observables, we can in the spirit of ordinary $\sigma$-algebras characterize quantum-mechanical $\sigma$-algebras by the requirement that such an algebra is a nonempty family of sets closed under complementation and denumerable unions of pairwise disjoint sets.

To look at the logic of the situation we should consider only algebras of sets that are closed under complementation and (finite) union of disjoint sets. A question still open in the literature is the characterization of the sentential logic whose valid formulas are just those valid in all such quantum-mechanical algebras.

We can also proceed in a more abstract fashion and consider orthocomplemented partial orderings. The axioms are embodied in the following definition. In this definition it is understood that $A$ is a nonempty set, $\leqq$ is a binary relation on $A$, this relation being the abstract analogue of set inclusion. The unary operation $'$ is the abstract analogue of set complementation, and 1 is an element of $A$, where this element is the abstract analogue of the sample space $X$ in a quantum-mechanical algebra of sets.

DEFINITION. *A structure $\mathfrak{A} = \langle A, \leqq, ', 1\rangle$ is a quantum-mechanical algebra if and only if the following axioms are satisfied for every $a$, $b$ and $c$ in $A$:*

(1) $a \leqq a$;

(2) *if* $a \leqq b$ *and* $b \leqq a$ *then* $a = b$;

(3) *if* $a \leqq b$ *and* $b \leqq c$ *then* $a \leqq c$;

(4) *if* $a \leqq b$ *then* $b' \leqq a'$;

(5) $(a')' = a$;

(6) $a \leqq 1$;

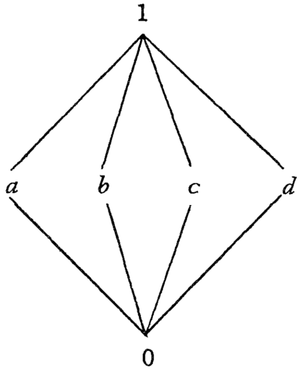
(7) *if* $a \leqq b$ *and* $a' \leqq b$ *then* $b = 1$;

(8) *if* $a \leqq b'$ *then there is a* $c$ *in* $A$ *such that* $a \leqq c$, $b \leqq c$, *and for all* $d$ *in* $A$ *if* $a \leqq d$ *and* $b \leqq d$ *then* $c \leqq d$;

(9) *if* $a \leqq b$ *then there is a* $c$ *in* $A$ *such that* $c \leqq a'$, $c \leqq b$ *and for every* $d$ *in* $A$ *if* $a \leqq d$ *and* $c \leqq d$ *then* $b \leqq d$.

This definition may easily be extended to a $\sigma$-quantum-mechanical algebra, and it is also easy to prove that every quantum-mechanical algebra of sets as defined above is a quantum-mechanical algebra in the sense of this definition.

On the other hand, Bjarni Jónsson gave me the following simple counterexample to show that not every quantum-mechanical algebra is isomorphic to a quantum-mechanical algebra of sets (complicated examples in terms of subspaces of Hilbert space are known, but the virtue of this example is its elementary character). The counterexample is the following lattice.




$$\text{where}\quad \begin{aligned} a' &= b,\ b' = a, \\ c' &= d,\ d' = c, \\ 0' &= 1,\ 1' = 0. \end{aligned}$$

Given the elementary difference between quantum-mechanical algebras and quantum-mechanical algebras of sets, it would also be interesting to have an axiomatization of the sentential logic whose valid sentences hold in every quantum-mechanical algebra. While more can be said about these problems, I shall move on to some closely related problems.

Another approach in the same qualitative fashion is characterization of the probability structure imposed on such algebras. For one thing, we can ask for a qualitative axiomatization of probability for quantum-mechanical algebras of sets corresponding to the classical de Finetti problem for ordinary algebras of sets. That most of the classical qualitative theory goes through is demonstrated in [7, Chapter 5], and so I shall not repeat those details here, but it does provide an elementary qualitative theory of quantum-mechanical probability, where *quantum-mechanical probability* means a probability measure not necessarily defined for union or intersection of arbitrary events in the algebra of events.

From a quantum-mechanical standpoint, however, the development of qualitative probability theory is merely a fragment, and to reach any genuinely quantum-mechanical questions, additional qualitative axioms are needed. It does not seem hopeless to proceed in the same fashion to obtain qualitative axioms, at least for simple cases, and it would seem to be a problem worth pursuing to see how far such a qualitative approach to the foundations of quantum mechanics can be followed. I may be mistaken, but I think one can go quite a way with such simple cases as linear harmonic oscillators in one dimension. Exactly how far such qualitative approaches can be pushed may provide some interesting insights into the structure of quantum mechanics.

Quite apart from the relatively simple substructures of classical quantum mechanics, there remains the open problem of constructing in proper set-theoretical form a fully adequate axiomatic formulation, which may initially be stated in terms of complicated mathematical notions, but which must be adequate to both the fundamental concepts and the standard applications. I believe as yet no such formulation exists in satisfactory form. Many partial aspects of the more complex theory, including abstract characterizations of the Hilbert-space theory of observables and operators, have been intensely studied, but what would appear on the surface to be the elementary matter of establishing a constructive theory for the relation between particular observables and particular operators, a necessary step in any theory adequate to applications, does not seem to have been investigated in an explicit axiomatic way. The standard axiom that there is a one-to-one correspondence between observables and self-adjoint operators of a separable, infinite-dimensional complex Hilbert space, for example, is far from providing anything like the needed constructive formulation.

**6. Stimulus-response theory of behavior.** I do not want to give the impression that the application of axiomatic methods in the empirical sciences is mainly restricted to the physical sciences. Many problems of interest in the behavioral and social sciences have also been treated from an axiomatic standpoint. Much of the contemporary work in mathematical economics satisfies a high standard of axiomatization, and when not explicitly so stated, it can easily be put within a standard set-theoretical framework without difficulty. On the other hand, with the exception of some of the problems of measurement I mentioned earlier, the impact of the theory of models as developed in logic and the kind of metamathematical questions characteristic of that theory have not been widely applied in the social sciences, and the relation of these sciences to fundamental questions of logic has not had the history of examination characteristic of problems of long standing in physics.

The kind of psychological theory I want to discuss in this section represents an area in which I have personally worked for a decade and a half. I apologize for the egocentric emphasis, but I believe there is a useful point to be made by considering this particular theory. Behavior theories of the kind mentioned are in their axiomatic formulation thoroughly embedded in probability theory. The explicit use of standard probability theory forbids at once any possibility of a natural first-order formulation. Even though in quantum mechanics it may be worthwhile to attempt an elementary qualitative formulation in order to investigate the foundations more thoroughly, in the present state of development of axiomatic behavior theories, it does not yet seem worthwhile to conduct such an investigation, because of the comparatively shallow theoretical developments so far obtained. In contrast, classical quantum mechanics offers something of great permanent value, in spite of the fact that it is known not to be empirically correct for significant domains of physical phenomena.

Once a probabilistic apparatus is assumed and a reasonably complex process is postulated, the axioms inevitably assume a character far removed from the elegant formulations characteristic of first-order theories that have received considerable investigation on the part of logicians and that have played a central part in the development of the theory of models.

Because of the complicated mathematical structures involved, it is not really feasible to look for metamathematical results about these theories of behavior. However, a tendency already in the theory of models within logic can be used to advantage in the present setting as well. For some time a definite effort has been made by Tarski and his collaborators to prove results established by metamathematical methods by purely mathematical arguments, and to study with some care the problems involved in making the transition. In the case of axiomatic behavior theories, there are few theorems of any depth. The real problems still center on proper formulation of the theories themselves. Here I think a lesson can be learned from the theory of models. The great explicitness and clarity of contemporary work in the theory of models can provide a guide to formulation in explicit mathematical terms of theories of behavior.

It might seem that if the theories were mainly going to be used to test fairly straightforward experimental results, there would be no need to formulate the theories in explicit axiomatic form, and consequently, there would be no real lesson to be learned from the methods of model theory. Whatever the situation should be, this is not the situation that obtains. A great deal of conceptual controversy exists about the intellectual power of various theories to explain different categories of behavior. A standard claim in psycholinguistics, for example, is that stimulus-response theories cannot possibly explain the complexities and subtleties of language learning and language behavior. The difficulty with these negative views of stimulus-response theory is that they have the status of negative dogmas rather than that of impossibility theorems. To convert these negative dogmas into genuine theoretical insights into the power or limitations of power of theories we must prove an impossibility theorem of the kind familiar in many domains of mathematics, but especially in the foundations of mathematics.

Particularly for behavior theories and language learning, there is a subtler and more intimate connection with contemporary work in mathematical logic, because of the connection between various types of languages, regular, context-free, context-sensitive, etc., and various types of abstract machines. In other words, axiomatic formulations of theories of behavior can themselves reflect back onto logic itself in order to give a new and different viewpoint of how logic and language can be learned, and how language behavior and mathematical thinking take place.

Stated at this level of generality I sound far too ambitious for theories of behavior and certainly unrealistic in terms of present accomplishments. My aim, however, is to give a perspective on what should be attempted, not on what has been accomplished.

I have taken some small steps in this direction myself. In [**19**], I formulated a version of stimulus-response theory and showed that given any connected finite automaton there is a stimulus-response model that at asymptote, i.e., as the number of learning trials goes to infinity, is isomorphic in the appropriate sense to the finite automaton. On simple, idealized assumptions about human memory, it is not difficult to extend these results to more powerful abstract machines. The conceptually more interesting problem is analyzing the mechanisms of reinforcement that produce the learning required for the isomorphism at asymptote. The schedule of reinforcement used in the theorem I proved is far too simple and too direct to account for much human learning, especially language learning. A considerably weaker schedule of reinforcement was also shown to be adequate to essentially the same result in Rottmayer's thesis [**14**]. The interesting aspect of Rottmayer's result and one that might be anticipated is that an isomorphism of the internal states no longer holds because of the weakness of the reinforcement scheme, which simply informs the learning mechanism whether it was correct or not after each externalized response. The internal states of the learning model do not become asymptotically isomorphic to the given automaton, but behavioral isomorphism is achievable.

For experimental and practical applications these asymptotic results need to be replaced by much more detailed analysis of the learning rate. At the present time, however, it seems forbiddingly difficult to compute the expected trial of last error or other related quantities. Also, rough estimates of such quantities indicate that learning is too slow, and further improvements in the theory are needed.

**7. Semantics of natural languages.** If we look at the abstract machine approach to language and its relation to the current definitions of formal languages, for example, context-free or context-sensitive languages, it is apparent that the most evident missing aspect is the serious account of the semantics of these languages. This is the final topic to which I turn.

Although this work is just beginning, it is my prediction that the numerous results that are an outgrowth of the theory of models, many of which stem from Tarski's work in the 1930's on the semantics of formalized languages, will in the future be of equal importance in the analysis of natural languages. Before his untimely death, Tarski's former student, the late Richard Montague, had already taken important steps in this direction [**10**].

I would like to sketch my own approach to these matters and to give enough details of some examples so that those familiar with the theory of models in logic will feel immediately at home with the concepts used for the analysis of natural languages. The definitions are formal and thereby apply to any context-free language; indeed they are easily generalized to richer languages, but I wish to emphasize that the apparatus as used by me and my collaborators is applied to the analysis of natural languages. In this respect our use of semantics is like the use of mathematics in physics. We are applying it to empirical data, and we are concerned with testing theoretical ideas within a standard scientific methodology for testing scientific theories, including, when the occasion is appropriate, a full statistical analysis of goodness of fit. I predict that model-theoretic semantics, so much associated with Tarski's work, will prove to be one of the most important tools in the scientific and empirical study of natural languages, once linguists and psychologists reach a deeper understanding and appreciation of the methods of model theory.

Without giving a complete set of formal definitions, I shall try to state the intuitive character of my own ideas and to give some concrete examples. I start with the notion of a phrase-structure grammar developed by Chomsky and others. From a formal standpoint, a structure $G = \langle V, V_N, P, S \rangle$ is a *phrase-structure grammar* if and only if $V$ and $P$ are finite, nonempty sets, $V_N$ is a subset of $V$, $S$ is in $V_N$ and $P \subseteq V_N^* \times V^+$, where $V_N^*$ is the set of finite sequences of elements of $V_N$, and $V^+$ is the set of such sequences of elements of $V$, but excluding the empty sequence. Following the usual terminology, $V_N$ is the nonterminal vocabulary and $V_T = V - V_N$ the terminal vocabulary. The start symbol is $S$ or the single axiom from which we derive strings or words in the language generated by $G$. The set $P$ is the set of production or rewrite rules. If $\langle \alpha, \beta \rangle \in P$, we write $\alpha \rightarrow \beta$, which we read: From $\alpha$ we may produce or derive $\beta$ (immediately). Moreover, a phrase-structure grammar $G = \langle V, V_N, P, S \rangle$ is *context-free* if and only if $P \subseteq V_N \times V^+$, i.e., if $\alpha \rightarrow \beta$ is in $P$ then $\alpha \in V_N$ and $\beta \in V^+$.[2]

We next need to define derivation trees for context-free grammars. Each tree is ordered from left to right in the intuitive fashion to get a left to right reading of a terminal string, and each node has a label. The leaves have terminal labels and the other nodes have nonterminal labels. We go on to make this a *semantic tree*—this is the point at which model-theoretic semantics enters—by also requiring that each node of the tree have a denotation. We begin by requiring in the simple case that each terminal word have a denotation, and in the more complex and subtle cases that given terminal phrases denote with no subparts denoting. We get the denotation of the other nodes of the tree by recursion. The rules of the recursion are obtained by assigning to each production rule of the grammar a set-theoretical function that permits us to compute, so to speak, the denotation of the left-hand side of a production rule knowing the denotations of the members of the right-hand side of the rule. For example, if we have the rewrite rule

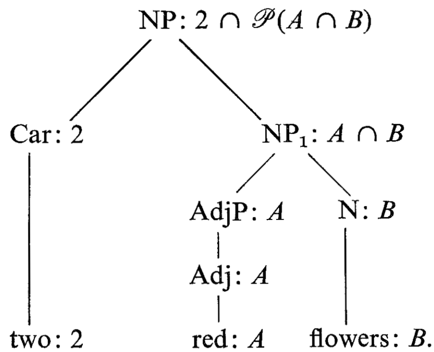
$$\mathrm{NP} \rightarrow \mathrm{Adj} + \mathrm{NP},$$

the set-theoretical function we assign to this rule in the simple case is that of intersection. These set-theoretical functions assigned to the production rules play the role of a recursive definition of truth in Tarski's characterization of truth for formal languages.

Finally, to get the ordinary model theory, the requirement is that the denotations all lie in a hierarchy of sets built up from a nonempty domain. For the uses of natural language a sufficiently rich hierarchy seems to be one that is closed under union of sets, the power set operation and the forming of subsets. Technical details are contained in **[18]**.

[2] To make context-free grammars a special case of phrase-structure grammars, as defined here, the first members of $P$ should be not elements of $V_N$, but one-place sequences whose terms are elements of $V_N$. This same problem arises in referring to elements of $V^*$, but treating elements of $V$ as belonging to $V^*$. Consequently, to avoid notational complexities, I treat elements, their unit sets and one-place sequences whose terms are the elements as identical.

I would like now to turn to some examples drawn from children's speech to illustrate how these ideas are applied empirically. I shall restrict myself to the simple example of the noun phrase *two red flowers* and its approximate equivalents in French and Chinese. For the semantics, I use the Frege-Russell concept of cardinal number: Two is just the set of all pair sets, and to avoid any paradoxes, we can consider only members of sets a certain distance up the hierarchy of sets. I use the standard notation for the power set, thus $\mathscr{P}(A)$ is the set of all subsets of $A$. In the tree for nonterminal symbols I have: NP for noun phrase, Car for cardinal number name, AdjP for adjective phrase, Adj for adjective, and N for noun. Also let $A$ be the set of red things and $B$ the set of flowers. The denotation of each node is shown after the colon that follows the label of the node. The tree then looks like this:

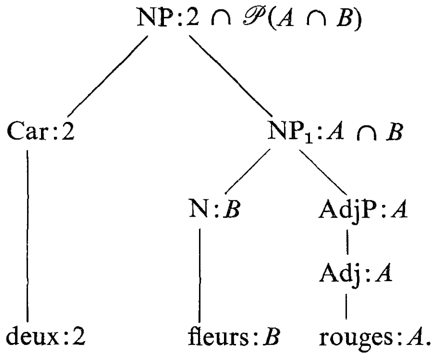



The partial grammar exhibited by this tree can be written in the following form:

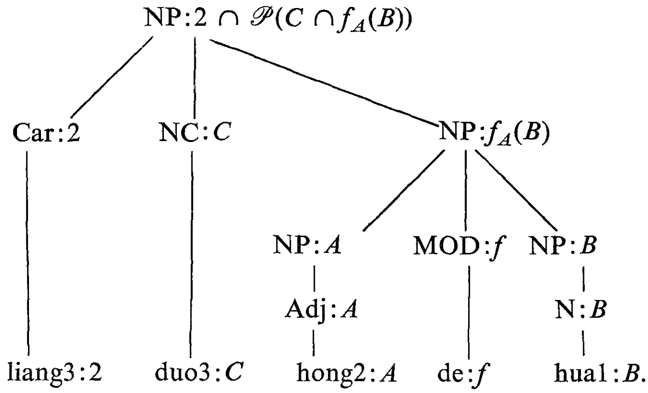
$$\begin{array}{ll} NP \rightarrow Car + NP_1, & NP_1 \rightarrow AdjP + N, \\ AdjP \rightarrow AdjP + Adj, & AdjP \rightarrow Adj. \end{array}$$

It is clear from the tree what the semantic function is for each rule. For example, for the third rule, intersection is the semantic function. In both the tree and the grammar I have used the subscript "1" on "NP" to impose a restriction that blocks the recursion of cardinal number names. At an elementary level we certainly do not want phrases such as *two four red flowers*.

The French phrase corresponding to *two red flowers* is *deux fleurs rouges*, even though it is more uncommon in French to omit the definite article than it is in English. The semantic tree is the same as the one above, except for the natural left-right reflection in the part of the tree that occurs in adjusting for the surface position of adjectives in French.




The corresponding Chinese semantic tree includes a noun classifier (NC) and the particle *de* (MOD) and is more complicated on the surface than the English or French trees, but the underlying semantics is similar. My current feeling is that the simple semantics of the noun classifiers is to let them denote the union of all the sets of objects denoted by the nouns they modify, but of course when an NC is used as the mechanism of pronominal reference something different has to be said. On the simple assumption just stated about union, the semantic tree for *liang3 duo3 hong2 de hua1* (two red flowers) is:




For the semantic interpretation of the particle *de*, $f$ is a choice function such that, for each $A, f_A(B) \subseteq B$, and the semantic function attached to the NP rule shown on the subtree on the right of the Chinese semantic tree is simply a semantic function of three arguments that corresponds to this choice function, i.e., $\psi(A, f, B) = f_A(B)$.

I am not suggesting that my approach to these simple phrases in English, French and Chinese is necessarily the best, or even one that will prove to be correct in all details, but I do think the basic approach is correct and it shows how natural it is to use model-theoretic semantics in the analysis of natural languages. The precise determination of the semantic functions associated with a given grammar of a natural language, or part of the language, is an empirical problem. The appropriate empirical methods of investigation are not yet fully developed, but I see no conceptual obstacles standing in the way.

Finally, I emphasize that the application of model-theoretic semantics is not in any sense restricted to context-free fragments of natural language. Extension of the methods to indexed grammars, which are context-sensitive, or to a variety of transformations is straightforward. On the other hand, it is equally apparent that major conceptual extensions of model-theoretic semantics will be required to give a complete scientific account of language learning and use, but this is as it should be and is expected in assessing the adequacy of any theory dealing with empirical phenomena at a fundamental level.

## REFERENCES

**1.** R. Abraham, *Foundations of mechanics. A mathematical exposition of classical mechanics with an introduction to the qualitative theory of dynamical systems and applications to the three-body problem*, Benjamin, New York, 1967. MR **36** #3527.

**2.** Z. Domotor, *Causal models and space-time geometries*, Synthese **24** (1972), 5–57.

**3.** H. G. Forder, *The foundations of Euclidean geometry*, Cambridge Univ. Press, Cambridge, 1927; reprint, Dover, New York, 1958. MR **20** #6671.

**4.** R. H. Hudgin, *Coordinate-free relativity*, Synthese **24** (1972), 281–297.

**5.** S. Kochen and E. P. Specker, *Logical structures arising in quantum theory*, Theory of Models (Proc. 1963 Internat. Sympos. Berkeley), North-Holland, Amsterdam, 1965, pp. 177–189. MR **34** #5409.

**6.** D. H. Krantz, *Fundamental measurement of force and Newton's first and second laws of motion* (to appear).

**7.** D. H. Krantz, R. D. Luce, P. Suppes and A. Tversky, *Foundations of measurement*. Vol. 1, Academic Press, New York, 1971.

**8.** R. W. Latzer, *Nondirected light signals and the structure of time*, Synthese **24** (1972), 236–280.

**9.** J. C. C. McKinsey, A. C. Sugar and P. Suppes, *Axiomatic foundations of classical particle mechanics*, J. Rational Mech. Anal. **2** (1953), 253–272. MR **14**, 1023.

**10.** R. Montague, "English as a formal language," in *Linguaggi Nella Società e Nella Tecnica*, edited by B. Visentini et al., Milan, 1970, pp. 189–224.

**11.** W. Noll, *The foundations of classical mechanics in the light of recent advances in continuum mechanics*, Proc. Internat. Sympos. (Univ. of Calif., Berkeley, 1958), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1959, pp. 266–281. MR **21** #6757.

**12.** H. Reichenbach, *Philosophic foundations of quantum mechanics*, Univ. of California Press, Berkeley, Calif., 1944.

**13.** A. A. Robb, *Geometry of space and time*, Cambridge Univ. Press, Cambridge, 1936.

**14.** W. A. Rottmayer, *A formal theory of perception*, Technical Report #161, Institute for Mathematical Studies in the Social Sciences, Stanford University, Stanford, Calif., 1970.

**15.** D. Scott, *Measurement models and linear inequalities*, J. Mathematical Psychology **1** (1964), 233–247.

**16.** D. Scott and P. Suppes, *Foundational aspects of theories of measurement*, J. Symbolic Logic **23** (1958), 113–128. MR **28** #6716.

**17.** P. Suppes, *New foundations of objective probability: Axioms for propensities*, Logic, Methodology and Philosophy of Science, *IV*, Proc. Fourth Internat. Congress for Logic, Methodology and Philosophy of Science (Bucharest, 1971), edited by P. Suppes, L. Henkin, A. Joja and Gr. C. Moisil, North-Holland, Amsterdam, 1973.

**18.** ———, *Semantics of context-free fragments of natural languages*, Technical Report #171, Institute for Mathematical Studies in the Social Sciences, Stanford University, Stanford, Calif., 1971; *Approaches to natural language*, edited by K. J. Hintikka, J. Moravcsik and P. Suppes, Reidel, Dordrecht, 1973, pp. 370–394.

**19.** ———, *Stimulus-response theory of finite automata*, J. Mathematical Psychology **6** (1969), 327–355.

**20.** ———, *The probabilistic argument for a non-classical logic of quantum mechanics*, Philos. Sci. **33** (1966), 14–21. MR **35** #6415.

**21.** A. Tarski, *What is elementary geometry?* Proc. Internat. Sympos. (Univ. of Calif., Berkeley, 1957), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1959, pp. 16–29. MR **21** #4919.

**22.** A. Tarski, *Über die Beschränktheit der Ausdrucksmittel deduktiver Theoriem*, Ergebnisse eines mathematischen Kolloq., Univ. Vienna **7** (1934/35), 15–22; English transl. in A. Tarski, *Logic, semantics, metamathematics. Papers from* 1923–1938, Clarendon Press, Oxford, 1956. MR **17**, 1171.

**23.** V. S. Varadarajan, *Geometry of quantum theory*. Vol. 1, Van Nostrand, Princeton, N.J., 1968.

**24.** ———, *Probability in physics and a theorem on simultaneous observability*, Comm. Pure Appl. Math. **15** (1962), 189–217. MR **29** #917.

**25.** E. C. Zeeman, *Causality implies the Lorentz group*, J. Mathematical Phys. **5** (1964) 490–493. MR **28** #5785.

STANFORD UNIVERSITY