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A COMPARISON OF THE MEANING AND USES OF MODELS IN MATHEMATICS AND THE EMPIRICAL SCIENCES

I. MEANING

Consider the following quotations:
'A possible realization in which all valid sentences of a theory T are satisfied is called a model of T.' (Tarski [1953, p. 11])
'In the fields of spectroscopy and atomic structure, similar departures from classical physics took place. There had been accumulated an overwhelming mass of evidence showing the atom to consist of a heavy, positively charged nucleus surrounded by negative, particle-like electrons. According to Coulomb's law of attraction between electric charges, such a system will collapse at once unless the electrons revolve about the nucleus. But a revolving charge will, by virtue of its acceleration, emit radiation. A mechanism for the emission of light is thereby at once provided.
'However, this mechanism is completely at odds with experimental data. The two major difficulties are easily seen. First, the atom in which the electrons revolve continually should emit light all the time. Experimentally, however, the atom radiates only when it is in a special, 'excited' condition. Second, it is impossible by means of this model to account for the occurrence of spectral lines of a single frequency (more correctly, of a narrow range of frequencies). The radiating electron of our model would lose energy; as a result it would no longer be able to maintain itself at the initial distance from the nucleus, but fall in toward the attracting center, changing its frequency of revolution as it falls. Its orbit would be a spiral ending in the nucleus. By electrodynamic theory, the frequency of the radiation emitted by a revolving charge is the same as the frequency of revolution, and since the latter changes, the former should also change. Thus our model is incapable of explaining the sharpness of spectral lines.' (Lindsay and Margenau [1936, pp. 390–91]).
'The author [Gibbs] considers his task not as one of establishing physical theories directly, but as one of constructing statistic-mechanical models

which have some analogies in thermodynamics and some other parts of physics; hence he does not hesitate to introduce some very special hypotheses of a statistical character.' (Khinchin [1949, p. 4])

'Thus, the model of rational choice as built up from pair-wise comparisons does not seem to suit well the case of rational behavior in the described game situation.' (Arrow [1951, p. 21])

'In constructing the model we shall assume that each variable is some kind of average or aggregate for members of the group. For example, D might be measured by locating the opinions of group members on a scale, attaching numbers to scale positions and calculating the standard deviation of the members' opinions in terms of these numbers. Even the intervening variables, although not directly measured, can be thought of as averages of the values for individual members.' (Simon [1957, p. 116])

'This work on mathematical models for learning has not attempted to formalize any particular theoretical system of behavior; yet the influences of Guthrie and Hull are most noticeable. Compared with the older attempts at mathematical theorizing, the recent work has been more concerned with detailed analyses of data relevant to the models and with the design of experiments for directly testing quantitative predictions of the models.' (Bush and Estes [1959, p. 3])

'I shall describe... various criteria used in adopting a mathematical model of an observed stochastic process... For example, consider the number of cars that have passed a given point by time t. The first hypothesis is a typical mathematical hypothesis, suggested by the facts and serving to simplify the mathematics. The hypothesis is that the stochastic process of the model has independent increments... The next hypothesis, that of stationary increments, states that, if $s < t$, the distribution of $x(t) - x(s)$ depends only on the time interval length $t - s$. This hypothesis means that we cannot let time run through both slack and rush hours. Traffic intensity must be constant.

'The next hypothesis is that events occur one at a time. This hypothesis is at least natural to a mathematician. Because of limited precision in measurements it means nothing to an observer... The next hypothesis is of a more quantitative kind, which also is natural to anyone who has seen Taylor's theorem. It is that the probability that at least one car should pass in a time interval of length h should be $ch + o(h)$.' (Doob [1960, p. 27])

The first of these quotations is taken from a book on mathematical logic, the next two from books on physics, the following three from works on the social sciences, and the last one from an article on mathematical statistics. Additional uses of the word 'model' could easily be collected in another batch of quotations. One of the more prominent senses of the word missing in the above quotations is the very common use in physics and engineering of 'model' to mean an actual physical model as, for example, in the phrases 'model airplane' and 'model ship'.

It may well be thought that it is impossible to put under one concept the several uses of the word 'model' exhibited by these quotations. It would, I think, be too much to claim that the word 'model' is being used in exactly the same sense in all of them. The quotation from Doob exhibits one very common tendency, namely, to confuse or to amalgamate what logicians would call the model and the theory of the model. It is very widespread practice in mathematical statistics and in the behavioral sciences to use the word 'model' to mean the set of quantitative assumptions of the theory, that is, the set of sentences which in a precise treatment would be taken as axioms, or, if they are themselves not adequately exact, would constitute the intuitive basis for formulating a set of axioms. In this usage a model is a linguistic entity and is to be contrasted with the usage characterized by the definition from Tarski, according to which a model is a non-linguistic entity in which a theory is satisfied.

There is also a certain technical usage in econometrics of the word 'model' that needs to be noted. In the sense of the econometricians a model is a class of models in the sense of logicians, and what logicians call a model is called by econometrians a *structure*.

It does not seem to me that these are serious difficulties. I claim that the concept of model in the sense of Tarski may be used without distortion and as a fundamental concept in all of the disciplines from which the above quotations are drawn. In this sense I would assert that the meaning of the concept of model is the same in mathematics and the empirical sciences. The difference to be found in these disciplines is to be found in their use of the concept. In drawing this comparison between constancy of meaning and difference of use, the sometimes difficult semantical question of how one is to explain the meaning of a concept without referring to its use does not actually arise. When I speak of the meaning of the concept of a model I shall always be speaking in well-defined technical contexts and what I shall be claiming is that, given this technical meaning of the concept of model, mathematicians ask a certain kind of question about models and empirical scientists tend to ask another kind of question.

Perhaps it will be useful to defend this thesis about the concept of model by analyzing uses of the word in the above quotations. As already indicated, the quotation from Tarski represents a standard definition of 'model' in mathematical logic. Our references to models in pure mathematics will, in fact, be taken to refer to mathematical logic, that branch of pure mathematics explicitly concerned with the theory of models. The technical notion of possible realization used in Tarski's definition need not be expounded here. Roughly speaking, a possible realization of a theory is a set-theoretical entity of the appropriate logical type. For example, a possible realization of the theory of groups is any ordered couple whose first member is a set and whose second member is a binary operation on this set. The intuitive idea of a model as a possible realization in which a theory is satisfied is too familiar in the literature of mathematical logic to need recasting. The important distinction that we shall need is that a theory is a linguistic entity consisting of a set of sentences and models are non-linguistic entities in which the theory is satisfied (an exact definition of theories is also not necessary for our uses here).

I would take it that the use of the notion of models in the quotation from Lindsay and Margenau could be recast in these terms in the following manner. The orbital theory of the atom is formulated as a theory. The question then arises, does a possible realization of this theory in terms of entities defined in close connection with experiments actually constitute a model of the theory, or, put another way which is perhaps simpler, do models of an orbital theory correspond well to data obtained from physical experiments with atomic phenomena? It is true that many physicists want to think of a model of the orbital theory of the atom as being more than a certain kind of set-theoretical entity. They envisage it as a very concrete physical thing built on the analogy of the solar system. I think it is important to point out that there is no real incompatibility in these two viewpoints. To define formally a model as a set-theoretical entity which is a certain kind of ordered tuple consisting of a set of objects and relations and operations on these objects is not to rule out the physical model of the kind which is appealing to physicists, for the physical model may be simply taken to define the set of objects in the set-theoretical model. Because of the importance of this point it may be well to illustrate it in somewhat greater detail. We may axiomatize classical particle mechanics in terms of the five primitive notions of a set P of particles, an interval T of real numbers corresponding to elapsed times, a position function s defined on the Cartesian product of the set of particles and the time interval, a mass function m defined on the set of particles, and a force function f defined on the Cartesian product of the set of particles, the time interval and the set of positive integers (the set of positive integers enters into the definition of the force function simply in order to provide a method of naming the forces). A possible realization of the axioms of classical particle mechanics, that is, of the theory of classical particle mechanics, is then an ordered quintuple $\mathscr{P} = \langle P, T, s, m, f \rangle$. A model of classical particle mechanics is such an ordered quintuple. It is simple enough to see how an actual physical model in the physicist's sense of classical particle mechanics is related to this set-theoretical sense of models. We simply can take the set of particles to be in the case of the solar system the set of planetary bodies. Another slightly more abstract possibility is to take the set of particles to be the set of centers of mass of the planetary bodies. This generally exemplifies the situation. The abstract set-theoretical model of a theory will have among its parts a basic set which will consist of the objects ordinarily thought to constitute the physical model (for a discussion of the axiomatic foundations of classical particle mechanics in greater detail along the lines just suggested see Suppes [1957, Chp. 12]).

In the preceding paragraph we have used the phrases, 'set-theoretical model' and 'physical model.' There would seem to be no use in arguing about which use of the word 'model' is primary or more appropriate in the empirical sciences. My own contention in this paper is that the set-theoretical usage is the more fundamental. The highly physically minded or empirically minded scientists who may disagree with this thesis and believe that the notion of a physical model is the more important thing in a given branch of empirical science may still agree with the systematic remarks I am making.

An historical illustration of this point is Kelvin's and Maxwell's efforts to find a mechanical model of electromagnetic phenomena. Without doubt they both thought of possible models in a literal physical sense, but it is not difficult to recast their published memoirs on this topic into a search for set-theoretical models of the theory of continuum mechanics which will account for observed electromagnetic phenomena. Moreover, it is really the formal part of their memoirs which has had permanent value. Ultimately it is the mathematical theory of Maxwell which has proved important, not the physical image of an ether behaving like an elastic solid.

The third quotation is from Khinchin's book on statistical mechanics, and the phrase, 'the author,' refers to Gibbs whom Khinchin is discussing at this point. The use of the word 'model' in the quotation of Khinchin is particularly sympathetic to the set-theoretical viewpoint for Khinchin is claiming that in his work on the foundations of statistical mechanics Gibbs was not concerned to appeal directly to physical reality or to establish true physical theories but rather to construct models or theories having partial analogies to the complicated empirical facts of thermodynamics and other branches of physics. Again in this quotation we have as in the case of Doob, perhaps even more directly, the tendency toward a confusion of the logical type of theories and models, but again this does not create a difficulty. Anyone who has examined Gibb's work or Khinchin's will readily admit the ease and directness of formulating their work in such a fashion as to admit explicitly and exactly the distinction between theories and models made in mathematical logic. The abstractness of Gibb's work in statistical mechanics furnishes a particularly good example for applying the exact notion of model used by logicians, for there is not a direct and immediate tendency to think of Gibbs' statistical mechanical theories as being the theories of the one physical universe.

I think the following observation is empirically sound concerning the use of the word 'model' in physics. In old and established branches of physics which correspond well with the empirical phenomena they attempt to explain, there is only a slight tendency ever to use the word 'model.' The language of theory, experiment and common sense is blended into one realistic whole. Sentences of the theory are asserted as if they are the one way of describing the universe. Experimental results are described as if there were but one obvious language for describing them. Notions of common sense refined perhaps here and there are taken to be appropriately homogeneous with the physical theory. On the other hand, in those branches of physics which give as yet an inadequate account of the detailed physical phenomena with which they are concerned there is a much more frequent use of the word 'model.' Connotation of the use of the word is that the model is like a model of an airplane or ship. It simplifies drastically the true physical phenomena and only gives account of certain major or important aspects of it. Again, in such uses of the word 'model' it is to be emphasized that there is a constant interplay between the model as a physical or non-linguistic object and the model as a theory. The quotation from Arrow which follows the one from Khinchin exemplifies in the social sciences this latter tendency in physics. Arrow, I would say, refers to the *model* of rational choice because the theory he has in mind does not give a very adequate description of the phenomena with which it is concerned but only provides a highly simplified schema. The same remarks apply fairly well to the quotation from Simon. In Simon we have an additional phenomena exemplified which is very common in the social and behavioral sciences. A certain theory is stated in broad and general terms. Some qualitative experiments to test this theory are performed. Because of the success of these experiments scientists interested in more quantitative and exact theories then turn to what is called 'the construction of a model' for the original theory. In the language of logicians, it would be more appropriate to say that rather than constructing a model they are interested in constructing a quantitative theory to match the intuitive ideas of the original theory.

In the quotation from Bush and Estes and the one from Doob there is introduced an important line of thought which is, in fact, very closely connected with the concept of model as used by logicians. I am thinking here of the notion of model in mathematical statistics, the extensive literature on estimating parameters in models and testing hypotheses about them. In a statistical discussion of the estimation of the parameters of a model it is usually a trivial task to convert the discussion into one where the usage of terms is in complete agreement with that of logicians. The detailed consideration of statistical questions almost requires the consideration of models as mathematical or set-theoretical rather than simple physical entities. The question, 'How well does the model fit the data?' is a natural one for statisticians and behavioral scientists. Only recently has it begun to be so for physicists, and it is still true that much of the experimental literature in physics is reported in terms of a rather medieval brand of statistics.

It may be felt by some readers that the main difficulty with the thesis being advanced in this paper is the lack of substantive examples in the empirical sciences. Such a reader would willingly admit that there are numerous examples of exactly formulated theories in pure mathematics and thereby an exact basis is laid for precisely defining the models in which these theories are satisfied. But it might be held the situation is far different in any branch of empirical sciences. The formulation of theory goes hand in hand with the development of new experiments and new experimental techniques. It is the practice of empirical scientists, so it might be claimed, not to formulate theories in exact fashion but only to give them sufficient conceptual definiteness to make their connections with current experiments sufficiently clear to other specialists in the field. He who seeks an exact characterization of the theory and thus of models in such branches of science as non-vertebrate anatomy, organic chemistry or nuclear physics is indeed barking up the wrong tree. In various papers and books I have attempted to provide some evidence against this view. In the final chapter of my *Introduction to Logic* I have formulated axiomatically a theory of measurement and a version of classical particle mechanics which satisfy, I believe, the standards of exactness and clarity customary in the axiomatic formulation of theories in pure mathematics. In Estes and Suppes [1960] such a formulation is attempted for a branch of mathematical learning theory. In Rubin and Suppes [1954] an exact formulation of relativistic mechanics is considered and in Suppes [1959] such a formulation of relativistic kinematics is given. These references are admittedly egocentric; it is also pertinent to refer to the work of Woodger [1937], Hermes [1938], Adams [1959], Debreu [1959], Noll [1959] and many others. Although it is not possible to pinpoint a reference to every branch of empirical science which will provide an exact formulation of the fundamental theory of the discipline, sufficient examples do now exist to make the point that there is no systematic difference between the axiomatic formulation of theories in well-developed branches of empirical science and in branches of pure mathematics.

By remarks made from a number of different directions I have tried to argue that the concept of model used by mathematical logicians is the basic and fundamental concept of model needed for an exact statement of any branch of empirical science. To agree with this thesis it is not necessary to rule out or to deplore variant uses or variant concepts of model now abroad in the empirical sciences. As has been indicated, I am myself prepared to admit the significance and practical importance of the notion of physical model current in much discussion in physics and engineering. What I have tried to claim is that in the exact statement of the theory or in the exact analysis of data the notion of model in the sense of logicians provides the appropriate intellectual tool for making the analysis both precise and clear.

II. USES

The uses of models in pure mathematics are too well-known to call for review here. The search in every branch of mathematics for representation theorems is most happily characterized in terms of models. To establish a representation theorem for a theory is to prove that there is a class of models of the theory such that every model of the theory is isomorphic to some member of this class. Examples now classical of such representation theorems are Cayley's theorem that every group is isomorphic to a group of transformations and Stone's theorem that every Boolean algebra is isomorphic to a field of sets. Many important problems in mathematical logic are formulated in terms of classes of models. For a statement of many interesting results and problems readers are referred to Tarski [1954].

When a branch of empirical science is stated in exact form, that is, when the theory is axiomatized within a standard set-theoretical framework, the familiar questions raised about models of the theory in pure mathematics may also be raised for models of the precisely formulated empirical theory. On occasion such applications have philosophical significance. Many of the discussions of reductionism in the philosophy of science may best be formulated as a series of problems using the notion of a representation theorem. For example, the thesis that biology may be reduced to physics would be in many people's minds appropriately established if one could show that for any model of a biological theory it was possible to construct an isomorphic model within physical theory. The diffuse character of much biological theory makes any present attempt to realize such a program rather hopeless. An exact result of this character can be established for one branch of physics in relation to another. An instance of this is Adams' [1959] result that for a suitable characterization of rigid body mechanics every model of rigid body mechanics is isomorphic to a model defined within simple particle mechanics. But I do not want to give the impression that the application of models in the empirical sciences is mainly restricted to problems which interest philosophers of science. The attempt to characterize exactly models of an empirical theory almost inevitably yields a more precise and clearer understanding of the exact character of the theory. The emptiness and shallowness of many classical theories in the social sciences is well brought out by the attempt to formulate in any exact fashion what constitutes a model of the theory. The kind of theory which mainly consists of insighful remarks and heuristic slogans will not be amenable to this treatment. The effort to make it exact will at the same time reveal the weakness of the theory.

An important use of models in the empirical sciences is in the construction of Gedanken experiments. A Gedanken experiment is given precision and clarity by characterizing a model of the theory which realizes it. A standard and important method for arguing against the general plausibility of a theory consists of extending it to a new domain by constructing a model of the theory in that domain. This aspect of the use of models need not however be restricted to Gedanken experiments. A large number of experiments in psychology are designed with precisely this purpose in mind, that is, with the extension of some theory to a new domain, and the experimenter's expectation is that the results in this domain will not be those predicted by the theory.

It is my own opinion that a more exact use of the theory of models in the discussion of Gedanken experiments would often be of value in various branches of empirical science. A typical example would be the many discussions centering around Mach's proposed definition of the mass of bodies in terms of their mutually induced accelerations. Because of its presumed simplicity and beauty this definition is frequently cited. Yet from a methematical standpoint and any exact theory of models of the theory of mechanics it is not a proper definition at all. For a very wide class of axiomatizations of classical particle mechanics it may be proved by Padoa's principle that a proper definition of mass is not possible. Moreover, if the number of interacting bodies is greater than seven a knowledge of the mutually induced acceleration of the particles is not sufficient for unique determination of the ratios of the masses of the particles. The fundamental weakness of Mach's proposal is that he did not seem to realize a definition in the theory cannot be given for a single model, but must be appropriate for every model of the theory in order to be acceptable in the standard sense.

Another significant use of models, perhaps peculiar to the empirical sciences, is in the analysis of the relation between theory and experimental data. The importance of models in mathematical statistics has already been mentioned. The homogeneity of the concept of model used in that discipline with that adopted by logicians has been remarked upon. The striking thing about the statistical analysis of data is that it is shot through and through with the kind of comparison of models that does not ordinarily arise in pure mathematics. Generally speaking, in some particular branch of pure mathematics the comparison of models involves comparison of two models of the same logical type. The representation theorems mentioned earlier provide good examples. Even in the case of embedding theorems, which establish that models of one sort may be extended in a definite manner to models of another sort, the logical type of the two models is very similar. The situation is often radically different in the comparison of theory and experiment. On the one hand, we may have a rather elaborate set-theoretical model of the theory which contains continuous functions or infinite sequences, and, on the other hand, we have highly finitistic set-theoretical models of the data. It is perhaps necessary to explain what I mean by 'models of the data.' The maddeningly diverse and complex experience which constitutes an experiment is not the entity which is directly compared with a model of a theory. Drastic assumptions of all sorts are made in reducing the experimental experience, as I shall term it, to a simple entity ready for comparison with a model of the theory.

Perhaps it would be well to conclude with an example illustrating these general remarks about models of the data. I shall consider the theory of linear response models set forth in Estes and Suppes [1959]. For simplicity, let us assume that on every trial the organism can make exactly one of two responses, A_1 or A_2, and after each response it receives a reinforcement, E_1 or E_2, of one of the two possible responses. A learning parameter Θ, which is a real number such that $0 < \Theta \leq 1$, describes the rate of learning, in a manner to be made definite in a moment. A possible realization of the theory is an ordered triple $\mathscr{X} = < X, P, \Theta >$ of the following sort. X is the set of all sequences of ordered pairs such that the first member of each pair is an element of some set A and the second member an element of some set B, where A and B each have two elements. Intuitively, the set A represents the two possible responses and the set B the two possible reinforcements. P is a probability measure on the Borel field of cylinder sets of X, and Θ is a real number as already described. (Actually there is a certain arbitrariness in the characterization of possible realizations of theories whose models have a rather complicated set-theoretical structure, but this is a technical matter into which we shall not enter here.) To define the models of the theory, we need a certain amount of notation. Let $A_{i,n}$ be the event of response A_i on trial n, $E_{j,n}$ the event of reinforcement E_j on trial n, where $i, j = 1, 2$, and for x in X let x_n be the equivalence class of all sequences in X which are identical with x through trial n. A possible realization of the linear response theory is then a model of the theory if the following two axioms are satisfied in the realization:

Axiom 1. *If* $P(E_{i,n}A_{i',n}x_{n-1}) > 0$ then

$$P(A_{i,n+1} \mid E_{i,n}A_{i',n}x_{n-1}) = (1 - \Theta)P(A_{i,n} \mid x_{n+1}) + \Theta.$$

Axiom 2. *If* $P(E_{j,n}A_{i',n}x_{n-1}) > 0$ *and* $i \neq j$ *then*

$$P(A_{i,n+1} \mid E_{j,n}A_{i',n}x_{n-1}) = (1 - \Theta)P(A_{i,n} \mid x_{n-1}).$$

As is clear from the two axioms, this linear response theory is intuitively very simple. The first axiom just says that when a response is reinforced the probability of making that response on the next trial is increased by a simple linear transformation. And the second axiom says that if some other response is reinforced, the probability of making the response is decreased by a second linear transformation. In spite of the simplicity of this theory it gives a reasonably good account of a number of experiments, and from a mathematical standpoint it is by no means trivial to characterize asymptotic properties of its models.

The point of concern here, however, is to relate models of this theory to models of the data. Again for simplicity, let us consider the case of simple noncontingent reinforcement. On every trial the probability of an E_1 reinforcement, independent of any preceding events, is π. The experimenter decides on an experiment of, say, 400 trials for each subject, and he uses a table of random numbers to construct for each subject a finite reinforcement sequence of 400 trials. The experimental apparatus might be described as follows.

The subject sat at a table of standard height. Mounted vertically on the table top was a 125 cm. wide by 75 cm. high black panel placed 50 cm. from the end of the table. The experimenter sat behind the panel, out of view of the subject. The apparatus, as viewed by the subject, consisted of two silent operating keys mounted 20 cm. apart on the table top and 30 cm. from the end of the table; upon the panel, three milk-glass panel lights were mounted. One of these lights, which served as the signal for the subject to respond, was centered between the keys at a height of 42 cm. from the table top. Each of the two remaining lights, the reinforcing signals, was at a height of 28 cm. directly above one of the keys. On all trials the signal light was lighted for 3.5 sec.; the time between successive signal exposures was 10 sec. The reinforcing light followed the cessation of the signal light by 1.5 sec. and remained on for 2 sec.

The model of the data incorporates very little of this description. On each trial the experimenter records the response made and the reinforcement given. Expressions on the subject's face, the movement of his limbs, and in the present experiment even how long he takes to make the choice of which key to punch are ignored and not recorded. Even though it is clear exactly what the experimenter records, the notion of a possible realization of the data is not unambiguously clear. As part of the realization it is clear we must have a finite set D consisting of all possible finite sequences of length 400 where, as previously, the terms of the sequences are ordered couples, the first member of each couple being drawn from some pair set A and the second member from some pair set B. If a possible realization consisted of just such a set D, then any realization would also be a model of the data. But it seems natural to include in the realization a probability measure P on the set of all subsets of D, for by this means we may impose upon models of the data the experimental schedule of reinforcement. In these terms, a possible realization of the data is an ordered couple $\mathscr{D} = \langle D, P \rangle$ and, for the case of noncontingent reinforcement a realization is a model if and only if the probability measure P has the property of being a Bernoulli distribution with parameter π on the second members of the terms of the finite sequences in D, i.e., if and only if for every n from 1 to 400, $P(E_{1,n} \mid x_{n-1}) = \pi$ when $P(x_n) > 0$.

Unfortunately, there are several respects in which this characterization of models of the data may be regarded as unsatisfactory. The main point is that the models are still too far removed even from a highly schematized version of the experiment. No account has been taken of the standard practice of randomization of response A_1 as the left key for one subject and the right key for another. Secondly, a model of the data, as defined above, contains 2^{400} possible response sequences. An experiment that uses 30 or 40 subjects yields but a small sample of these possibilities. A formal description of this sample is easily given, and it is easily argued that the 'true' model of the data is this actual sample, not the much larger model just defined. Involved here is the formal relation between the three entities labeled by statisticians the 'sample,' the 'population,' and the 'sample space.' A third difficulty is connected with the probability measure that I have included as part of the model of the data. It is certainly correct to point out that a model of the data is hardly appropriately experimental if there is no indication given of how the probability distribution on reinforcements is produced.

It is not possible in this paper to enter into a discussion of these criticisms or the possible formal modifications in models of the data which might be made to meet them. My own conviction is that the set-theoretical concept of model is a useful tool for bringing formal order into the theory of experimental design and analysis of data. The central point for me is the much greater possibility than is ordinarily realized of developing an adequately detailed formal theory of these matters.

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