Patrick Suppes. The Desirability of Formalization in Science. Journal of Philosophy, 65, 1968, pp. 651-664.

In spite of this undramatic appearance of our quasi-idealistic conclusions, it seems to me that they are in a deep sense connected with what is true and important in the original thesis. Among other things, I believe that thinking of *Dinge an sich* as a separate, unknowable class of entities has always been a case of fallacious hypostatization. If this connection really exists, it is not surprising that as a by-product we can also partially vindicate the old Kantian doctrine of the synthetic a priori character of nontrivial mathematical (and, for us, logical) truths.

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THE DESIRABILITY OF FORMALIZATION IN SCIENCE *

IT would be amusing to put the question whether formalization in science is desirable to Archimedes in Sicily or some three hundred years later to Ptolemy in Alexandria. I can imagine Archimedes, in a characteristic turn of phrase, saying that no man of eminence in philosophy would ask such a question. It would be like asking if one wanted an actual demonstration in geometry as opposed to a suggestive but informal method. I think even Ptolemy, though much more deeply involved in empirical observations and the complicated problem of fitting theory to data, would answer in the same vein. In other words, I want to claim that the only branches of quantitative science seriously developed in ancient times were regarded as extensions of geometry and treated with the same degree of formality. I am thinking particularly of Archimedes' work in statics; his treatise on the equilibrium of planes is the first systematic treatise in mathematical physics. Also I am thinking of Ptolemy's *Almagest,* which is unquestionably the greatest scientific work of antiquity, if we mean by *science,* as opposed to *mathematics,* the development of theory and the confronting of theory with quantitative data. For this ancient tradition of scientific analysis, there was no proper way to think other than in terms of the formal methods of geometry. This pattern that originates with Archimedes has a long and continuous history through the Arabic and Latin tradition of the Middle Ages, leading without a break down to Newton's *Principia* in the seventeenth century. Newton's formal geometrical methods and his careful consideration of data are very much in the spirit of Ptolemy's *Almagest.* For Newton too, there is little question of

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the desirability of formalization in science. Even the most empirical of his systematic works, the *Opticks,* was organized in geometrical and theorem-like fashion.

It is possible to reply that formalization today means a great deal more than the explicit use of the geometrical method did in ancient Alexandria, at the University of Paris in the thirteenth century, or at Cambridge in the seventeenth century. But the point is not well taken, for surely the real conceptual point is that the standards of rigor and formality characteristic of the mathematics of the time are also to be found in the systematic science.

In the nearly three centuries since the publication of Newton's *Principia,* the divergence between the deductive method used in mathematics and in physics has become quite pronounced. The direction of mathematics has been to move toward ever more well-defined standards of rigor and formalization. This is not the case in mathematical physics, although even here a distinction has begun to appear between mathematical physics and theoretical physics. Mathematical physics is increasingly a subject done by mathematicians, and its concern for questions of rigor and formal clarity of assumptions is evident. Theoretical physics, on the other hand, is done in a fashion that is very far from satisfying modern mathematical standards. None of the historically important papers of this century in the theory of relativity or in quantum mechanics was written in clearly delineated, formalized mathematical style. (In referring to relativity theory, I have in mind the early papers of Einstein and Lorentz, not the later mathematical work of Minkowski, Veblen, Robb, and others.) Even von Neumann's book on quantum mechanics (1932) does not give an axiomatic, or what we might term Archimedean, development of quantum mechanics, but only of Hilbert space. It is difficult to predict the future of axiomatic and formalized methods in the empirical sciences. There are signs, at least, that the large gap presently separating the methods used in physics from those in mathematics is beginning to close, and will not widen in other empirical disciplines, such as mathematical economics and mathematical psychology. But it is not the purpose of this paper to attempt to give a closely argued projection of these future developments.

Putting aside the positive argument from the tradition that begins with Archimedes and that includes Newton, and also the negative argument that points out the low level of formalization current in much of contemporary science, it is still pertinent to ask what are the reasons for a philosophical concern with formalization in science. Is formalization desirable, or indeed in some cases necessary, for a proper philosophical analysis of concepts? One way of putting a philosophical argument for formalization is this. The role of philosophy in science is to clarify conceptual problems and to make explicit the foundational assumptions of each scientific discipline. The clarification of conceptual problems or the building of an explicit logical foundation are tasks that are neither intensely empirical nor mathematical in character. They may be regarded as proper philosophical tasks directly relevant to science.

In the context of such clarification and construction, a primary method of philosophical analysis is that of formalizing and axiomatizing the concepts and theories of fundamental importance in a given domain of science. To argue that such formalization is one important method of clarification is not in any sense to claim that it is the only method of philosophical analysis. Nearly everyone would agree that penetrating but informal criticisms of examples of formalization play a central role in assessing the intuitive correctness of a solution to a conceptual problem or the construction of an explicit foundation. Formalization will not answer all questions nor solve all problems, but there is a very instructive lesson to be found in the philosophy of mathematics. Over the past hundred years, methods of formalization have been applied extensively to the foundations of mathematics. It is fair to say that during that period there has been more discernible progress in our understanding of the foundations of mathematics and in the depth of the problems that are considered important than in any other branch of philosophy. This foundational work has had far-reaching consequences in mathematics itself. The set-theoretical foundations of mathematics that began with Cantor and Frege and that were perfected by Zermelo, Russell, and others are today a framework within which much of pure mathematics is written.

The sense of formalization I shall use in the subsequent discussion is just that of a standard set-theoretical formulation. I do not want to mean by formalization the stricter conception of a first-order theory that assumes only elementary logic. Such stricter formalization is appropriate for the intensive study of many elementary domains of mathematics, but in almost all areas of science a rich mathematical apparatus is needed. We can properly appeal to that apparatus within a set-theoretical framework. (A more elaborate defense of the exact sense of formalization intended here will not be given; I have defended a set-theoretical approach on numerous occasions in the past, and much of what I say here on more general issues will not depend in any sensitive way on the precise sense of formalization being used.)

There are other reasons why the formalization of a scientific theory is desirable. Something may be said, even if briefly, about them.

Explicitness. To formalize a connected family of concepts is one way of bringing out their meaning in an explicit fashion. A good example of what can be hoped for in this direction is provided by the concept of probability. Prior to the explicit analysis of the formal structure of probability by Kolmogorov (1933), there was much confusion about even the most elementary properties of probability, e.g., the domain of definition of a probability measure. The intuitive correctness of Kolmogorov's formalization was recognized almost at once, and is now universally adopted. On the other hand, the formalization did not end discussion and philosophical analysis of the concept of probability. Rather, it helped to raise the discussion to a new level. The difficulty with the purely set-theoretical characterization of Kolmogorov is that the concept of probability is not sufficiently categorical. Interpretations of the axioms are possible that correspond to no one's intuitions about probability. One way of interpreting the vast recent literature on the foundations of probability is as an attempt to supplement Kolmogorov's axioms to provide an acceptable and at the same time more nearly categorical axiom system, in order to make the meaning of probability still more explicit.

Standardization. One of the most desirable consequences of the use of a set-theoretical framework for the explicit formalization of scientific theories is the standardization of terminology and methods of conceptual analysis that would be brought to different branches of science. It is an empirical question, but one I think could be supported by systematic evidence, that students who know and can use set-theoretical methods find it easier to absorb different branches of science, all of which are formalized within a common language. One broad aim of formalization is to make communication easier across scientific disciplines. The unity of science, which has been so ably defended by many philosophers in this century, would be much more of a reality if a common theoretical language were used as widely as possible.

Generality. Another virtue of formalization is that it often provides a means of seeing the forest in spite of the trees. I mean by this that formalization eliminates provincial and inessential features of the way in which a scientific theory has been thought about. It is easy to give several examples of the virtue of formalization from this standpoint. Perhaps one of the nicest is the proof that the two classical versions of quantum mechanics: wave mechanics and matrix mechanics, are identical, in the sense that there is just one complex, separable Hilbert space, up to isomorphism, and both are realizations of such a space. In one clear sense, the controversy about whether a continuous mechanics of waves or a discrete mechanics of matrix representations should be used for the analysis of quantum-mechanical phenomena was rendered irrelevant by this proof of isomorphism.

Objectivity. Formalization provides a degree of objectivity that is impossible in theories that are not stated in such fashion. In areas of science where great controversy exists about even the most elementary concepts, the value of such formalization can be substantial. One example is the continuing controversy in psychology between behaviorally oriented stimulus-response theorists and cognitive theorists. For many of the experimental paradigms that have been intensively studied and for which extensive bodies of data exist, it is possible to show in a rigorous fashion that the models of precisely formulated versions of stimulus-response theory and the models of precisely formulated selection-of-strategy cognitive theories are isomorphic.[1]

Self-contained assumptions. Formalization is a way of setting off from the forest of implicit assumptions and the surrounding thickets of confusion, the ground that is required for the theory being considered. Physicists, for example, are fond of ruling out on "physical grounds" solutions to fundamental equations which they find unacceptable. But unless the assumptions required for the elimination of such solutions are stated in advance, the selection of solutions is left to untutored intuition. It is a proper question to ask why it is philosophically interesting to formulate a theory in terms of a self-contained set of assumptions, and I do not want to suggest that a definitive argument can be given about matters as complex as this. At bottom, however, I think there is an instinct for craftsmanship that is hard to resist once it is developed. Not to state systematically all the assumptions needed for a theory at the beginning of its development is much like building a house whose foundation is continually modified as the upper stories are constructed, because the structural load-bearing analysis was in error. To drive for assumptions that are self-contained is also a way of assuring scientific

[1] Some detailed results are to be found in Patrick Suppes and Richard C. Atkinson, *Markov Learning Models for Multiperson Situations* (Stanford, Calif.; University Press, 1960) and in my "Stimulus-response Theory of Finite Automata," to appear in the *Journal of Mathematical Psychology*.

objectivity. If new assumptions can be added informally as needed, there is always doubt whether a genuine explanation of empirical phenomena has been found. The *ad hoc* addition of new assumptions has been an especially prevalent practice in certain areas of psychology and sociology, but fortunately has received the criticism it justly deserves. Insistence on a standard level of set-theoretical formalization in the statement of theories is one safeguard against *ad hoc* and *post hoc* verbalizations.

Minimal assumptions. Formalization of a theory makes possible an objective analysis of what are the minimal assumptions necessary for statement of the theory. There is, I think, a direct aesthetic appeal in the task of finding a set of mutually independent assumptions that are self-contained as a formulation of a theory. It is philosophically interesting to ask why we make this search for a minimum set of independent assumptions, but for the present purposes it is more important to recognize that it is of almost universal concern. The meaningfulness of the question, "What are the minimal assumptions in terms of which a theory can be formulated?" is a rather direct test of the seriousness of our understanding of the theory, and a measure of the depth of its development. To ask that a branch of classical physics be formulated in terms of a minimum set of assumptions or axioms is a meaningful request, but the question what is a minimal but sufficient set of assumptions for psychoanalysis does not now have a serious response. In most areas of science we do not yet have a clear formalization of the discipline in terms of a minimum set of self-contained assumptions. One proper task for philosophers interested in the foundations of science is providing that analysis. I would defend the proposition that it is a task for which formally trained philosophers are more suited in most cases than scientists working in the discipline. The example of relativity mentioned below is to the point in this respect.

Although other general arguments for the desirability of formalization can be given, I would like now to consider three somewhat detailed examples of how methods of formalization can contribute in a useful way to the clarification of conceptual problems in different domains of science. The first analysis deals with the special theory of relativity, the second with the measurement of intensive properties, and the third with psycholinguistics.

RELATIVITY

Anyone with some logical training who looks at the philosophical literature on relativity, ranging from Reichenbach to Grünbaum, will probably experience a certain uneasy feeling. The source of this uneasiness is the lack of clarity in the basic assumptions of the theory. For our purposes here, we may confine ourselves to the special theory of relativity. A possible response is that the special theory of relativity is many things to many physicists, and philosophers should not be asked to give one clear and fundamental set of assumptions from which will flow all desired results. However, I do not think that this view is correct. A responsibility of philosophers engaged in a logical analysis of relativity is to provide clear and self-sufficient assumptions from which the basic and fundamental results of the theory may be derived. There can indeed be some argument over exactly which results are to be considered fundamental, but there can scarcely be any disagreement about the essential importance of showing that any two inertial frames of reference are related by a Lorentz transformation. A formally self-contained set of axioms or assumptions from which this result may be derived is not to be found in the writings of either Reichenbach or Grünbaum. Indeed, it is surprising that the relevant references are not even given or mentioned in Grünbaum's long book on space and time.[2] Disappointed by the philosophical texts we consult, we may consult standard textbooks or treatises in physics. Here, however, we encounter the same problems. In fact, the discussion generally will not be as clear from a logical or mathematical standpoint as that to be found, for example, in Grünbaum. The negative results of our search might encourage us to think that the matter is too complicated or mathematically too difficult to have been completely solved. This is the irony of the situation. The mathematics is elementary. Several self-contained solutions exist in the literature, and at least one (that of Robb[3]) has existed for over thirty years; yet few physicists or philosophers of science seem aware of the results.

I showed a number of years ago that the single assumption needed for relativistic kinematics is that all observers at rest in inertial frames get identical measurements of relativistic distances along inertial paths when their measuring instruments have identical calibrations.[4] It is a consequence and *not* an assumption of this analysis that the observers in different frames of reference are moving with uniform velocity with respect to each other. Moreover, no assump-

[2] Adolf Grünbaum, *Philosophical Problems of Space and Time* (New York: Knopf, 1963).

[3] Alfred A. Robb, *Geometry of Time and Space* (New York: Cambridge, 1936).

[4] "Axioms for Relativistic Kinematics with or without Parity," in Leon Henkin, Patrick Suppes, and Alfred Tarski, eds., *The Axiomatic Method with Special Reference to Geometry and Physics* (Amsterdam: North-Holland, 1959), pp. 291–307.

tions of continuity or linearity are required. The special theory of relativity can be placed upon the narrow operational basis of measurements of relativistic distance along inertial paths. Moreover, the main results can be obtained over a denumerable field of numbers, and indeed over a field of numbers that is not even Archimedean. By "main" result I mean the derivation that any two frames of reference are related by a Lorentz transformation. From the standpoint of physics, it is important and interesting to note that no local differentiable properties are required and that in a strict mathematical sense the derivation of the Lorentz transformations is elementary. Recently, E. C. Zeeman has shown that invariance of measurement results is not required, but that assumptions of order may be used (although his proof does not hold for two dimensions).[5]

It should be apparent how these explicit analyses of the assumptions needed to set up relativistic kinematics exemplify the virtues of formalization described above in general terms. The single assumption used by me or the one used by Zeeman is explicit, is in standard mathematical language, is self-contained, and is, in a precise sense that may be demonstrated, minimal. This cannot be said for the majority of analyses found in textbooks of physics or in the writings of philosophers concerned with relativity theory. The generality gained by the formalization is particularly brought out by consideration of the fact that not even an Archimedean field of numbers is required for the derivation of Lorentz transformations. This makes very clear the elementary character and also the macroscopic character of special relativity. No deep properties of the real-number system are required in formulating the kinematical theory. Of greatest importance philosophically is the replacement of somewhat vague and occasionally mysterious talk about the basic assumptions of relativity by exactly formulated axioms. The question, "What is the conceptual basis of the special theory of relativity?" can be given any one of several equivalent answers that have no residue of conceptual confusion.

FUNDAMENTAL MEASUREMENT OF INTENSIVE PROPERTIES

One of the most conceptually interesting disputes in the theory and practice of measurement in science is whether it is possible to have fundamental measurement of intensive properties. In the first place, a claim of fundamental measurement involves the claim that a quantitative scale may be established on the basis of qualitative em-

[5] "Causality Implies the Lorentz Group," *Journal of Mathematical Physics*, v, 4 (April 1964): 490–493.

pirical observations that assume no prior measurements. The distinction between intensive and extensive properties is an old one in science and philosophy going back at least to Aristotle, and having an extensive literature even in the Middle Ages under the topic of intension and remission of forms. Extensive properties, or magnitudes, are properties that can be added; for example, mass and length are extensive. Intensive properties, in contrast, cannot be added, even though they can be measured. For instance, two volumes of gas with the same temperature do not combine to form one gas with twice the temperature. It has been repeatedly claimed by some measurement theorists, notably N. R. Campbell, that fundamental measurement of intensive properties is not possible. Thus in his discussion of the measurement of temperature, Campbell concludes that fundamental measurement of temperature is not possible since "there is no physical process of addition for temperature." [6] Campbell's insightful but confused analysis of measurement has had ramifications in many other areas of science. His position, for example, was essentially adopted in an influential article about measurement in psychology by Bergmann and Spence.[7] Here is what Spence and Bergmann say:

> In extensional dimensions and only in extensional dimensions, a factual meaning can be given to the equality of numerical differences without drawing upon any other empirical laws than the axioms of measurement themselves (111).

Until relatively recently the views expressed by Campbell, by Bergmann and Spence, and by others, that intensive properties cannot be fundamentally measured were widespread, and they still have currency in some quarters. Yet it is elementary and simple to demonstrate that these views, so categorically and dogmatically expressed, rest upon no serious formal analysis and can in fact be refuted by simple counterexamples that are themselves directly applicable to the measurement of intensive properties. One of the most important virtues of formalization can be brought out in this context. Within a discipline that is formalized, claims like those of Campbell, or Bergmann and Spence, are simply not made without formal proof. One of the most important formal developments in the history of mathematics and logic has been the development of techniques for proving that certain things are not possible. Classical examples are

[6] *Physics: The Elements* (New York: Cambridge, 1920), p. 396.

[7] Gustav Bergmann and Kenneth W. Spence, "The Logic of Psychophysical Measurement," *Psychological Review,* LI, 1944. Reprinted in Herbert Feigl and May Brodbeck, eds., *Readings in the Philosophy of Science* (New York: Appleton-Century-Crofts, 1953), pp. 103–119.

the proofs that arbitrary angles cannot be trisected by straightedge and compass, that the square root of 2 cannot be represented by the ratio of two integers, and that Euclid's parallel postulate cannot be derived from the other postulates. In logic and mathematics general claims of impossibility must be supported by sharply defined proofs in order to be taken seriously. Exactly the same standard can be applied in science when it is organized within a formal framework. In the case of intensive properties, for example, it is trivial to show that a quaternary relation expressing equality of numerical differences is invariant under a change of unit and origin and thus does not represent a relation expressing additivity, which is invariant only under change of unit. Such scales are now widely used in the measurement of utility and in the measurement of other psychological characteristics. It is also simple and elementary to give axioms for such a quaternary relation in purely qualitative terms. An example is to be found in my logic text [8] and a more extended discussion in the article by Zinnes and me.[9]

Unless the discussion of measurement of intensive properties is placed within a formal framework, there is little hope of an exact settlement of the issues. A very large psychological literature can be cited to show how confused matters can get when no serious attempt at formal analysis is made. Of course, the problem with Campbell is that he was an experimental physicist and did not really understand any sort of mathematical argument. He would have been astounded at the request that he give a proof of his claims that only extensive properties can be measured fundamentally. A case can be made for the view that Campbell did not get much beyond the discussion of the qualitative and quantitative intension and remission of forms by Thomas Bradwardine at Merton in the fourteenth century. Certainly Archimedes or Ptolemy, if confronted with Campbell's *Physics: The Elements,* would find almost unbelievable the degradation in standards of analysis and conceptual clarity that had taken place over a period of two thousand years.

PSYCHOLINGUISTICS

Strong winds of divergent opinion are currently more to be felt in psycholinguistics than in almost any other field of science. The conflict between linguists using mentalistic concepts and psychologists using a restricted set of behavioral concepts has been intense, at least since the publication of Chomsky's review (1959) of Skinner's

[8] *Introduction to Logic* (New York: Van Nostrand, 1957).

[9] Suppes and Joseph L. Zinnes, "Basic Measurement Theory," in R. D. Luce, R. R. Bush, and E. H. Galanter, eds., *Handbook of Mathematical Psychology,* vol. I, (New York: Wiley, 1963).

Verbal Behavior.[10] Among the welter of claims and counterclaims made by both linguists and psychologists, I have selected one issue upon which to comment here. Linguists of Chomskian persuasion have on repeated occasions asserted, and on a few occasions have even tried to prove, that no standard stimulus-response theory of conditioning can account for any central aspects of language behavior. For example, Katz and Postal declare in their book, "A conditioning theory of language acquisition must be rejected as being, in principle, incapable of explaining how language is learned."[11] Although some arguments are given to support their claim, they give no serious proof of the sort discussed above, which is standard in mathematical claims of a negative nature. It is even doubtful if they fully recognize the need for such a proof in order to give their assertion about conditioning theory a definite status. Above all, they do not give a clear formal interpretation of the phrase 'in principle'; yet such an interpretation is certainly of overriding importance in any serious analysis of this kind. For example, they do not distinguish between the claim that no conservative extension of the theory of conditioning can account for major aspects of language learning, and the stronger claim that even with the addition of other behavioral concepts the theory will not suffice. I mean here by a *conservative* extension of the theory one which would employ the same fundamental concepts as the original theory.[12]

A more ambitious attempt of the same sort as Katz and Postal's has recently been made by Bever, Fodor, and Garrett.[13] They attempt a formal proof, or at least what they consider a formal proof, of the limitations of associationism, or of what comes to the same thing, the stimulus-response theory of conditioning. However, their supposed proof is not a formal proof in any sense, and what they have to say about the informally stated postulate from which they work is conceptually confused. Their fundamental postulate is that "associative principles are rules defined over the 'terminal' vocabulary of a theory, i.e., over the vocabulary in which behavior is described" (583). Not many lines later, they assert that "a corollary of

[10] Noam Chomsky, review of B. F. Skinner, *Verbal Behavior* (New York: Appleton-Century-Crofts, 1957), *Language*, xxxv (1959): 26–58 (a).

[11] Jerrold J. Katz and Paul M. Postal, *An Integrated Theory of Linguistic Descriptions* (Cambridge, Mass.: MIT Press, 1964), p. 174.

[12] For a more detailed discussion of this point, see Edward Crothers and Suppes, *Experiments in Second-language Learning* (New York: Academic Press, 1967), pp. 1–25.

[13] Thomas G. Bever, Jerry A. Fodor, and Merrill Garrett, "A Formal Limitation of Associationism," in Theodore R. Dixon and David L. Horton, eds., *Verbal Behavior and General Behavior Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1968).

the terminal postulate is that, since behavior is organized in time, every associative relation is a relation between left and right elements of a sequence" (*loc. cit.*). No proof of this corollary is attempted, and it is patently obvious that in no serious mathematical sense is it a logical consequence of their fundamental postulate. They go on to discuss the simple case of a mirror-image language made up from a vocabulary of two letters. The grammatical strings of this language are then just the strings of the form *aa*, *abba*, *aabbaa*, etc. They make the correct point that such a language cannot be generated by a finite automaton, but they take this fact to be evidence of a formal limitation of associationism. The relevance of this example to any serious account of language is not discussed. In fact, much of what is said by linguists about languages that cannot be generated by finite automata seems as irrelevant to any theory of language learning and actual language behavior as the proof that no actual computer can generate the set of nonrecursive functions. Further evidence of confusion on this point on the part of Bever, Fodor, and Garrett is clear from the closing lines of their article. They say there that even if a large repertoire of behavior can be generated by associative principles, more powerful theories can explain more behavior, and therefore behavior can be more satisfactorily accounted for by these more powerful theories. They close with the remark, "Anything you can do with one hand tied behind your back, you can do with both hands free." It is hard to think of a remark that is mathematically less apposite to the task of giving a realistic account of language learning and behavior. If we are to take seriously this remark and the earlier remarks about powerful theories, there is no point in searching for models of the appropriate degree of complexity and power. We might just as well postulate from the beginning that we have a universal Turing machine, or, if the recursive character of such a device is too great a limitation, then postulate that we have available an abstract device that can generate all functions, recursive or not. The mélange of conceptual confusions to be found in this article is to be expected when relatively subtle formal points are discussed without serious attention to matters of exact formulation or proof.

Contrary to what these two examples suggest, I do not mean to argue that the venal sin of rash claims is committed solely by linguists. Chomsky's patient and detailed criticisms of the exaggerated claims made by Skinner in *Verbal Behavior* have had a salutary effect in psychology, and the tendency of psychologists to claim too much for the theories has been properly checked by the bold counterclaims of linguists.

It is generally recognized in mathematics that negative arguments must be formulated in a more formal and explicit fashion than positive arguments. Yet anyone who has looked at the myriad of solutions suggested over the past several hundred years to the classical problem of trisecting angles with straightedge and compass would recognize that the standards of formality adopted by Bever, Fodor, and Garrett are below that of many of these false positive solutions. It is particularly incumbent upon those who wish to make a negative claim about any theory, whether it be the theory of conditioning or the theory of geometric constructions, to make their mathematical argument in a formal and indeed painfully explicit way.

A deep conceptual controversy in a given domain of science can be one of the most stimulating influences on the growth of knowledge in that domain. The challenges and responses passing back and forth between linguists and psychologists is probably one of the healthiest and most important controversies in psychology in this century. The constructive role of formalization in such controversies should be apparent. The ultimate reason for formalization is that it provides the best objective way we know to convince an opponent of a conceptual claim. As I have already remarked, the widespread use of formalization in the foundations of mathematics is undoubtedly responsible for the great progress we have seen in that discipline during the past hundred years. Linguists like Chomsky have introduced a new formal machinery and a new kind of formalization in psychological analysis of behavior, especially language behavior. The overzealous and formally unsupportable claims that have been made in the wake of this introduction are natural consequences of a new and important scientific development. For the long run, what is much more important and especially relevant to the topic of this paper is the introduction of hierarchies of grammars and automata that generate these grammars. Even if psychologists are unwilling to accept any grammar that requires an automaton with an unbounded number of states, like, for example, the push-down automata required for arbitrary context-free grammars, finitary models proposed to mirror the actual performance of human beings must still reflect many of the properties of these potentially infinite automata. The linguists' insistence that they will accept nothing less than a complete and detailed account will probably turn out to be the most important conceptual demand on psychology of this century. Given the entrenchment of various positions in the controversy, I can see explicit formalization of relevant theory in a standard set-theoretical fashion as the only way of settling the theoretical issues, and the correspondingly formal analysis of data as the only way of satisfactorily settling the empirical issues.

Extending what I have said to other areas of science, let me conclude with a transcendental argument for formalization in science. Formalization is necessary in order to achieve objective resolution of conflict. There is no other general means of resolving conceptual conflict in science. Moreover, in a wide variety of experimental situations, there is no way to resolve disputes about the interpretation of data objectively except by careful and explicit use of the set-theoretical methods of contemporary mathematical statistics. But what is necessary is necessarily desirable, and so it is with formalization in science.

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NOTES AND NEWS

In September, a group of philosophers attending the International Congress of Philosophy in Vienna came in contact with eight University students from Prague who had left Czechoslovakia after the Soviet invasion, convinced that the freedom necessary for the continuation of their education was now obtainable only outside Czechoslovakia. An ad hoc committee (consisting of Max Black, Gertrude Ezorsky, Samuel Gorovitz, Ruth B. Marcus, Bernham Terrell, and Julius Weinberg) met with the students and ultimately arranged visas and air tickets to the United States. But bringing these students here is only the beginning. Economically they have nothing. The American philosophers who brought these students to the United States to complete their education did so in confidence that our colleagues at home, for whom freedom of inquiry is a professional necessity, would respond to the very real and immediate need of these courageous young people. Contributions of any amount should be sent to Samuel Gorovitz, Case Western Reserve University, Cleveland, Ohio. Checks should be made payable to Czech. Student Relief Fund, or CSRF. Further information may be obtained at the same address.

The Philosophy Department of Southern Illinois University announces with pleasure a new semi-annual philosophy journal, KINESIS, which will be published by the graduate students at Southern Illinois and which will consist entirely of work solicited from graduate students there and elsewhere. The first issue of KINESIS will appear in the late fall.

The University of Notre Dame announces a symposium in celebration of the birth of Giambattista Vico (1668–1744), on November 2, 1968. Participants will include: Thomas C. Bergin (Italian literature, Yale), A. Robert Camponigri (Notre Dame), Frederick J. Crosson (Notre Dame), Max H. Fisch (Illinois), Matthew A. Fitzsimons (history, Notre Dame), Angelo de Gennaro (Romance languages, Loyola), and Elio Gianturco (Romance languages, Hunter).