DOCUMENT RESUME

ED 031 406 SE 007 157

By-Suppes, Patrick
On the Problems of Using Mathematics in the Development of the Social Sciences.
Stanford Univ., Calif. Inst. for Mathematical Studies in Social Science.
Pub Date May 69
Note-26p.; Technical Report No. 143 Psychology Series
EDRS Price MF-$0.25 HC-$1.40
Descriptors-*Curriculum Problems, *Educational Problems, *Mathematical Applications, *Mathematics Education, *Social Sciences

In the first part of this paper, several important trends of mathematics in the social sciences since the end of World War II are reviewed. Among these are (1) decision theory, (2) the development of macroeconomics as it relates to economic theory and the economics of growth, (3) the general theory of measurement, (4) structural linguistics, and (5) the role of computers. The second part of the paper focuses on problems encountered in attempting to extend the use of mathematics in the social sciences. Included among these problems are (1) the training of graduate students, (2) training in elementary set theory and logic, (3) identification and measurement of variables that will prove significant in social science theories of the future, (4) the problem of linearity, (5) the analytical difficulties of probability theory, (6) the issue of mathematical rigor versus mathematical power, (7) the assessment of the future role of computers in the social sciences and their impact on mathematical developments, and (8) real world predictions versus laboratory predictions in evaluation. (RP)

N-X

ED031406

ON THE PROBLEMS OF USING MATHEMATICS IN THE DEVELOPMENT OF THE SOCIAL SCIENCES

BY

PATRICK SUPPES

TECHNICAL REPORT NO. 143

May 12, 1969

PSYCHOLOGY SERIES

INSTITUTE FOR MATHEMATICAL STUDIES IN THE SOCIAL SCIENCES
STANFORD UNIVERSITY
STANFORD, CALIFORNIA

SE 007 157

TECHNICAL REPORTS

PSYCHOLOGY SERIES

INSTITUTE FOR MATHEMATICAL STUDIES IN THE SOCIAL SCIENCES

(Place of publication shown in parentheses; if published title is different from title of Technical Report, this is also shown in parentheses.)

(For reports no. 1 - 44, see Technical Report no. 125.)

50 R. C. Atkinson and R. C. Calfee. Mathematical learning theory. January 2, 1963. (In B. B. Wolman (Ed.), Scientific Psychology. New York: Basic Books, Inc., 1965. Pp. 254-275)

51 P. Suppes, E. Crothers, and R. Weir. Application of mathematical learning theory and linguistic analysis to vowel phoneme matching in Russian words. December 28, 1962.

52 R. C. Atkinson, R. Calfee, G. Sommer, W. Jeffrey and R. Shoemaker. A test of three models for stimulus compounding with children. January 29, 1963. (J. exp. Psychol., 1964, 67, 52-58)

53 E. Crothers. General Markov models for learning with inter-trial forgetting. April 8, 1963.

54 J. L. Myers and R. C. Atkinson. Choice behavior and reward structure. May 24, 1963. (Journal math. Psychol., 1964, 1, 170-203)

55 R. E. Robinson. A set-theoretical approach to empirical meaningfulness of measurement statements. June 10, 1963.

56 E. Crothers, R. Weir and P. Palmer. The role of transcription in the learning of the orthographic representations of Russian sounds. June 17,. 1963.

57 P. Suppes. Problems of optimization in learning a list of simple items. July 22, 1963. (In Maynard W. Shelly, II and Glenn L. Bryan (Eds.), Human Judgments and Optimality. New York: Wiley. 1964. Pp. 116-126)

58 R. C. Atkinson and E. J. Crothers. Theoretical note: all-or-none learning and intertrial forgetting. July 24, 1963.

59 R. C. Calfee. Long-term behavior of rats under probabilistic reinforcement schedules. October 1, 1963.

60 R. C. Atkinson and E. J. Crothers. Tests of acquisition and retention, axioms for paired-associate learning. October 25, 1963. (A comparison of paired-associate learning models having different acquisition and retention axioms, J. math. Psychol., 1964, 1, 285-315)

61 W. J. McGill and J. Gibbon. The general-gamma distribution and reaction times. November 20, 1963. (J. math. Psychol., 1965, 2, 1-18)

62 M. F. Norman. Incremental learning on random trials. December 9, 1963. (J. math. Psychol., 1964, 1, 336-351)

63 P. Suppes. The development of mathematical concepts in children. February 25, 1964. (On the behavioral foundations of mathematical concepts. Monographs of the Society for Research in Child Development, 1965, 30, 60-96)

64 P. Suppes. Mathematical concept formation in children. April 10, 1964. (Amer. Psychologist, 1966, 21, 139-150)

65 R. C. Calfee, R. C. Atkinson, and T. Shelton, Jr. Mathematical models for verbal learning. August 21, 1964. (In N. Wiener and J. P. Schoda (Eds.), Cybernetics of the Nervous System: Progress in Brain Research. Amsterdam, The Netherlands: Elsevier Publishing Co., 1965. Pp. 333-349)

66 L. Keller, M. Cole, C. J. Burke, and W. K. Estes. Paired associate learning with differential rewards. August 20, 1964. (Reward and information values of trial outcomes in paired associate learning. (Psychol. Monogr., 1965, 79, 1-21)

67 M. F. Norman. A probabilistic model for free-responding. December 14, 1964.

68 W. K. Estes and H. A. Taylor. Visual detection in relation to display size and redundancy of critical elements. January 25, 1965, Revised 7-1-65. (Perception and Psychophysics, 1966, 1, 9-16)

69 P. Suppes and J. Donio. Foundations of stimulus-sampling theory for continuous-time processes. February 9, 1965. (J. math. Psychol., 1967, 4, 202-225)

70 R. C. Atkinson and R. A. Kinchla. A learning model for forced-choice detection experiments. February 10, 1965. (Br. J. math stat. Psychol., 1965, 18, 184-206)

71 E. J. Crothers. Presentation orders for items from different categories. March 10, 1965.

72 P. Suppes, G. Groen, and M. Schlag-Rey. Some models for response latency in paired-associates learning. May 5, 1965. (J. math. Psychol., 1966, 3, 99-128)

73 M. V. Levine. The generalization function in the probability learning experiment. June 3, 1965.

74 D. Hansen and T. S. Rodgers. An exploration of psycholinguistic units in initial reading. July 6, 1965.

75 B. C. Arnold. A correlated urn-scheme for a continuum of responses. July 20, 1965.

76 C. Izawa and W. K. Estes. Reinforcement-test sequences in paired-associate learning. August 1, 1965. (Psychol. Reports, 1966, 18, 879-919)

77 S. L. Blehart. Pattern discrimination learning with Rhesus monkeys. September 1, 1965. (Psychol. Reports, 1966, 19, 311-324)

78 J. L. Phillips and R. C. Atkinson. The effects of display size on short-term memory. August 31, 1965.

79 R. C. Atkinson and R. M. Shiffrin. Mathematical models for memory and learning. September 20, 1965.

80 P. Suppes. The psychological foundations of mathematics. October 25, 1965. (Colloques Internationaux du Centre National de la Recherche Scientifique. Editions du Centre National de la Recherche Scientifique. Paris: 1967. Pp. 213-242)

81 P. Suppes. Computer-assisted instruction in the schools: potentialities, problems, prospects. October 29, 1965.

82 R. A. Kinchla, J. Townsend, J. Yellott, Jr., and R. C. Atkinson. Influence of correlated visual cues on auditory signal detection. November 2, 1965. (Perception and Psychophysics, 1966, 1, 67-73)

83 P. Suppes, M. Jerman, and G. Groen. Arithmetic drills and review on a computer-based teletype. November 5, 1965. (Arithmetic Teacher, April 1966, 303-309.

84 P. Suppes and L. Hyman. Concept learning with non-verbal geometrical stimuli. November 15, 1968.

85 P. Holland. A variation on the minimum chi-square test. (J. math. Psychol., 1967, 3, 377-413).

86 P. Suppes. Accelerated program in elementary-school mathematics -- the second year. November 22, 1965. (Psychology in the Schools, 1966, 3, 294-307)

87 P. Lorenzen and F. Binford. Logic as a dialogical game. November 29, 1965.

88 L. Keller, W. J. Thomson, J. R. Tweedy, and R. C. Atkinson. The effects of reinforcement interval on the acquisition of paired-associate responses. December 10, 1965. (J. exp. Psychol., 1967, 73, 268-277)

89 J. I. Yellott, Jr. Some effects on noncontingent success in human probability learning. December 15, 1965.

90 P. Suppes and G. Groen. Some counting models for first-grade performance data on simple addition facts. January 14, 1966. (In J. M. Scandura (Ed.), Research in Mathematics Education. Washington, D. C.: NCTM, 1967. Pp. 35-43.

(Continued on inside back cover)

ON THE PROBLEMS OF USING MATHEMATICS IN THE DEVELOPMENT OF THE SOCIAL SCIENCES

by

Patrick Suppes

TECHNICAL REPORT NO. 143

May 12, 1969

PSYCHOLOGY SERIES

INSTITUTE FOR MATHEMATICAL STUDIES IN THE SOCIAL SCIENCES

STANFORD UNIVERSITY

STANFORD, CALIFORNIA

On the Problems of Using Mathematics in the Development of the Social Sciences[1]

Patrick Suppes

Institute for Mathematical Studies in the Social Sciences
Stanford University

To begin with I want to say that, although this is my first trip to Australia, for very special reasons I look upon Australia as a kind of paradise or promised land. During World War II, I spent two years in the Pacific--a year in the Solomon Islands and a year in Guam. The ambition of nearly every American serviceman in the South Pacific area was to have a leave in one of the main cities of Australia, and while a few friends of mine occasionally got to Australia, I was not so fortunate. I am pleased, however, to have realized that long-standing ambition years later. To be here is a pleasure thus enhanced by memories and hopes that go back for more than two decades.

As I turn to the topic of this first lecture, the second thing I would like to emphasize is that I am going to try to say and talk about more than I should. I think we all respect the seriousness and importance of academic specialization. In this first general talk about problems of using mathematics in the social sciences, I shall talk about things that many of you know better than I do, so I ask your indulgence as I

[1] This paper is based on a lecture given at a UNESCO Seminar at the University of Sydney, Australia on May 20, 1968. The content of the paper reflects research that has been supported by the National Science Foundation.

attempt to say some general things about the relation between mathematics and the social sciences. Also, before turning to a consideration of problems, I would like briefly to review what I feel are important trends of mathematics in the social sciences since the end of World War II. Obviously, it is not possible to give a detailed survey or to cover with equal emphasis all disciplines. If, however, we look at the work that has taken place since 1945, five or six main lines of activity emerge as an impressive beginning. They represent developments that did not exist in substance prior to 1945.

I would first mention in this context the general topic of decision theory. There are at least three important aspects of decision theory, namely, game theory, statistical decision theory, and normative economics. Each of these three is closely related to the other two. In the case of normative economics, it is easy enough to cite important and major papers that appeared before World War II, but it still seems proper to claim that the mathematical development of normative economics has received a very great impetus since 1945. The early papers in game theory, particularly the early work of von Neumann and Borel, occurred even before 1930, but certainly the entry of game theory into the mainstream of thought in the social sciences dates from the appearance in the 1940's of the von Neumann and Morgenstern treatise. Above all, the work in statistical decision theory is almost entirely a product of research that has taken place since World War II, even though earlier papers of Ramsey, deFinetti, and others are of considerable conceptual and historical importance.

To move quickly to other topics of importance, macroeconomics has been a major development in terms of economic theory, and recently, especially the economics of growth. Within psychology there has been a continuing strengthening and deepening of work in psychometrics, but I would claim the area that has changed the most is learning theory. There are, of course, clear and definite mathematical antecedents in the application of mathematics to learning theory. Most of you will know at least something about the work of Clark Hull and know that he was very much concerned with the application of mathematics to behavioral psychology, particularly to learning theory. Yet I think it is fair to say that Hull's work was not really mathematically viable or effective. On the other hand, it does seem that Hull's work was almost a necessary stage leading up to the development of more realistic mathematical developments. What we now have is a generation of younger psychologists who are at home in mathematical ideas, and who can use these ideas both in constructing theory and in applying theory to experimental data. This is a development that simply did not exist prior to World War II.

Another area closely connected with psychometrics, but that goes beyond psychometrics or related problems of econometrics, is the general theory of measurement. It too has had an intensive mathematical development in the last two decades. Probably more papers have appeared on the foundations of measurement and the development of particular approaches to measurement theory in the last ten years than have appeared in all previous years put together. Again, of course, in the case of measurement theory, there is a distinguished and important history dating from the work of Helmholtz in the nineteenth century, of prominent mathematicians like Hölder at the turn of the century, of people like Norbert Weiner, and of Frisch, Ramsey, and others in the series of papers in economics on the measurement of utility.

Another area of important development since 1950 is the coming of age of structural linguistics. Linguistics, like economics, is a discipline that has a distinguished history, but we all sense that new and important things have happened in linguistics in the last decade, and the impact of linguistics on other parts of the social sciences has been felt increasingly. In my second lecture I deal in some detail with the relations between the psychology of learning and structural linguistics. Therefore, I shall not enter into the subject in greater detail here.

Finally, in terms of the widespread use of mathematical ideas in the social sciences, it is important to mention the ever more central role of computers. I am sure that most of you are familiar with that benchmark date of 1951, when the first commercial computer was sold. The widespread use of computers in the academic world has only been within the last ten years, and yet, already the impact of that development has been felt. Problems may be tackled and investigations attempted that simply would not have been entertained prior to the availability of large-scale computing facilities.

In summary, I would say that in talking about the place of mathematics in the social sciences we need not be too apologetic about the social sciences as a set of intellectual disciplines. What has happened during the period since World War II constitutes an impressive intellectual development. There has been measurable and real progress in the application of mathematics. This period will be regarded as historically important in marking a major turning point in the development of mathematical social science. At the same time, all of us realize I think, how much there is yet to be done and how incomplete these developments are.

I would like now to turn to some discussion of the problems we encounter, or will encounter, in attempting to deepen the use of mathematics in the social sciences. To begin with, I would like to take up the first problem, the one mentioned by Chancellor Williams; this is the matter of training. I would like to speak of the training of graduate students, however, and not of the training of undergraduates.

Several years ago, as part of the activities of the Committee on the Undergraduate Program in Mathematics of the Mathematical Association of America, I served as chairman of the Panel on Mathematics for the Biological, Management, and Social Sciences. We decided in this panel to do a hard-data study of the actual mathematical background of graduate students in various social sciences, the extent to which mathematics was used in social science courses at the graduate level, and the extent to which mathematics was used in the writing of dissertations in the social sciences. I summarize here the main data from the study (American Mathematical Monthly, 1962, 69, 515-522). The data are drawn from the

Insert Table 1 about here

records of ten of the most prominent universities in the United States and are primarily for the academic year 1960-61. The eight academic fields covered in the study are shown in Table 1, which classifies the level of mathematical training of first-year graduate students in 1960-61 in each of the eight fields. The data are somewhat conservative, because the transcripts did not include courses taken during the last half of the senior year.

TABLE 1

Undergraduate Training in Mathematics of First-year Graduate Students, 1960-61

	Anthropology	Biochemistry	Bus. Adm.	Economics	Poli. Sci.	Psychology	Sociology	Zoology
No mathematics	33	1	78	19	112	25	34	25
Precalculus	28	10	184	48	69	69	31	77
Calculus	15	25	138	45	28	49	11	37
Math. beyond calculus	4	21	117	32	16	33	7	20
Totals	80	57	517*	144	225**	176	83	159

* Random samples rather than entire populations used at five universities.

** Random sample rather than entire population used at one university.

In Table 2, each of the Ph.D. dissertations accepted between September 1, 1958 and March 1, 1961 has been classified into one of the same four categories used in Table 1. Simple computations and graphs were not considered to be mathematics. Routine uses of standard statistical tests were classified as precalculus, including standard analysis of variance.

Insert Table 2 about here

Information was also obtained on graduate courses using calculus and mathematics beyond calculus during the academic years 1959-60 and 1960-61. The data are briefly summarized in Table 3, which shows the number of universities of the total surveyed that offered such courses in each of the eight fields.

Insert Table 3 about here

A quick perusal of these tables shows that at the beginning of the 1960's the mathematical training of young social scientists just entering the profession was scattered, and in some fields, almost nonexistent. Over the last eight or nine years the situation has improved considerably, especially in anthropology and political science, but it is still true that a major problem in using mathematics in the social sciences is the shortage of scientists trained to do so.

My second remark concerns the extent to which training in elementary set theory, and to some extent logic, has been emphasized in the training of young social scientists in the United States. The clarity of thinking that can be taught by beginning with the elementary and easily understood

TABLE 2

Level of Mathematics Used in Ph.D. Dissertations Accepted by the

Department Between September 1, 1958 and March 1, 1961

	Anthro-pology	Biochem-istry	Bus. Adm.	Econom-ics	Poli. Sci.	Psychol-ogy	Sociol-ogy	Zool-ogy
No mathematics	74	43	75	114	178	75	94	102
Precalculus	10	33	42	66	12	142	63	52
Calculus	0	20	5	21	0	9	4	8
Math. beyond calculus	0	1	2	26	1	11	2	1
Totals	84	97	124	227	191	237	163	163

TABLE 3

Number of Universities in the Survey Offering Graduate Courses Using Calculus and Mathematics beyond the Calculus, 1959-60 and 1960-61

Anthropology	Biochemistry	Bus. Adm.	Economics	Poli. Sci.	Psychology	Sociology	Zoology
2/10	7/9	7/9	10/10	0/10	9/10	5/9	6/10

material in logic and beginning set theory can form an appropriate and proper place to begin the mathematical training of young social scientists. It is not, however, in any sense the place to end. We are not going to do much in the analysis of the real world with the mathematical devices available to us in logic and set theory taken simply as deductive disciplines. Obviously, I am speaking here of the direct application of logic to the construction of theories within the social sciences and not of the way in which logic enters into the design of computer hardware and software. More powerful mathematical methods and more complicated mathematical concepts than the fundamental concepts of logic and set theory are needed for the analysis of the real world in quantitative fashion. As the author of two textbooks on logic and one on axiomatic set theory, I certainly am making this point without prejudice. Another way of making the point is to say that the tradition of mathematical analysis prominent in the training of physical scientists for several hundred years also needs a place in the training of social scientists. It is not that social scientists will make exactly the same use of analysis that physical scientists have, but the concepts of analysis provide tools necessary for the building of complex theories. This is especially true of both theories and experiments that depend heavily on probabilistic and statistical concepts.

My third problem is whether we have yet identified and can measure variables that will prove significant in social science theories of the future. On Mondays, Wednesdays, and Fridays I suppose my own intuition is that we do have a good grip on significant variables, and our problem is to be clearer about the theoretical framework in which we should consider these variables. On Tuesdays, Thursdays, and Saturdays my intuition is that we are not thinking about the right variables, and I do not see any hope of getting a grip on the right variables until theoretical developments have proceeded much further than they yet have. Whichever view is correct, I think there are some important analogies in the history of the physical sciences that can be used to deepen our reflection on the problem of significant variables and their measurement. In the case of physics, its perfection in many ways certainly depended upon the prior clarification, identification, and quantification of significant variables. An example I find particularly striking and impressive in this respect is the development of ancient astronomy. Consider the period from the serious beginning of Babylonian astronomy in 700 B.C. to the writing of Ptolemy's Almagest in 150 A.D., some 850 years later. It is fairly clear that there was a long period of clarification of what variables to deal with, what variables should be recorded, measured, and attended to. The theory could not advance until the observations had begun to accumulate, and Ptolemy's deep mathematical synthesis of this long tradition certainly relied upon observations and measurements dating from the Babylonians in 700 B.C. It is perhaps too easy for us to forget how extensive in time and how tortuous was the clarification of variables in the physical sciences. It is now hard to believe, for example, that at one point in the development of physics Aristotle's distinction between violent and natural motion was taken seriously. There is a desire on the part of many people to believe that the twentieth century will play the role in the development of the social sciences that the seventeenth century played in the development of the physical sciences, but it is important to keep in mind that there was a great mathematical and quantitative tradition in the physical sciences running all the way back to Babylonian times, and that was essential to the most important results achieved in the seventeenth century. We are not operating in a similar deep tradition in the construction of social-science theories in the twentieth century. Without this tradition to fall back on in the social sciences, we do not know whether we should properly think about theory first and expect the appropriate variables to fall out from the conceptualization of the theory, or whether the story is the other way around. My own uncertainty about the true facts and what will turn out to be the real situation is my reason for emphasizing as serious this problem of identifying significant variables.

Closely related to problems of measurement and quantification, and very deeply imbedded in almost all work in the social sciences, is the problem of linearity. In every direction of theoretical development, whether in economics, psychology, sociology, or political science, there are very strong reasons why we have to linearize our assumptions, why we have to deal with the mathematics that can be represented in terms that are essentially linear in nature. Occasionally, in a robust and ambitious moment, I try to look at nonlinear assumptions in learning theory and am discouraged after the first hour of work by the difficulties of making headway. We do have a powerful tool for escaping linearity restrictions in the increasingly widespread use of computers, and it could be that in terms of future development I am emphasizing a restriction that will shortly disappear. I am not convinced, however, that this is the case. It also may be that when the variables are properly identified and properly conceptualized, linearity will turn out to be natural to mathematical assumptions governing their relationships. As far as I am concerned, we are faced with a typical kind of trade-off in scientific research, a trade-off that has no clear resolution before the fact, and the guidelines for making a choice are vague indeed. Should we put a greater effort into pursuit of nonlinear assumptions in areas ranging from learning theory to macroeconomics, or is this a mistake and is it better to concentrate on rethinking the variables and formulating ideas that may be properly expressed in linear relationships? The absence of a clear-cut answer is my reason for restating the problem here.

A fifth problem area that I find depressing concerns the analytical difficulties of probability theory. The difficulties of obtaining solutions in closed form of even the simplest sorts of probabilistic processes are very discouraging to mathematically oriented social scientists. The deterministic differential equations that have dominated physics since the eighteenth century are much easier to handle and to apply in a constructive way. In generalizing, for example, the simplest models of learning from discrete trials to continuous time, analytical nightmares turn up dealing with situations that are conceptually and experimentally extraordinarily simple. The analytical difficulties I refer to is the problem of actually finding solutions in closed form of the appropriate conditional probabilities or asymptotic distributions. It is possible to prove that the quantities sought exist, but working them out in closed form in order to proceed with parameter estimation and to apply some version of statistical methods to fitting data to the theoretical results is extraordinarily time-consuming and difficult, even for the simplest cases.

Much of the work by mathematicians on the properties of stochastic processes does not yield results that are significant for those using mathematics in applied fashion. What we need for applications are powerful constructive methods of getting numerical answers, or parametric answers in closed form. This is not to criticize the high level of current mathematical work in probability theory. It is to reflect the judgment that we probably need a deeper contact between those working with the application of stochastic processes and those responsible for the mathematical developments within pure mathematics. I emphasize this problem also because of my own deep conviction that probabilistically formulated theories are here to stay in the social sciences. We shall not soon see a return to the deterministic sorts of theories that have been the glory of classical physics. I know that counterexamples to my statement can be found in some of the beautiful work on equilibrium questions in pure competition in mathematical economics in the past couple of decades, but most of that work has not yet made extensive contact with real data, and it would be my own conjecture that as it does, theories of a probabilistic sort will be needed.

Having mentioned problems about identification of variables, linearity of assumptions, and the analytical difficulties in studying probabilistic processes, I want to generalize these difficulties to a query about the present theories in the social sciences. The query is this. Are the theories we have in front of us essentially and fundamentally too simple in character, or is it rather that they are not too simple, but that we do not have the mathematical prowess and experimental insight to develop the theories in appropriate ways? It is now very fashionable in several areas of the social sciences to say that the theories that have been current and dominant for several decades could not possibly explain the actual complex patterns of behavior which form the subject of investigation. This has been a constant theme, for example, of cognitive psychologists who maintain that the fundamental concepts and assumptions of behavioral psychology are far too simple to explain any complex behavior, especially the complex verbal behavior of humans. The apparent complexity of learning to speak a first language, of learning to interact in subtle social fashion, or of learning a complex body of cognitive concepts, as in the case of mathematics, seems to cognitive psychologists clearly an impossible task to account for within the framework of behavioral psychology, or more especially, stimulus-response theory.

On the other hand, much of the work in cognitive psychology is oriented toward earlier work in social psychology, and the past several decades of social psychology provide a very good example of scientists rushing about for continually new sets of concepts. No single set of concepts receives a very deep or very articulated form of systematic development. To put the matter another way, the response of behavioral psychologists can be that cognitive psychologists have been effective only in their negative criticisms. Their own contributions in terms of the development of systematic theory are very thin indeed.

A two-sided argument of the same sort can easily be developed in economics regarding the status of classical theories of the market and of pure competition. I do not want to suggest for a moment that it is clear to me in any of these instances which particular view is correct. There is one conceptual point, however, that I do wish to emphasize. From a casual commonsense viewpoint, it would seem that the negative arguments are surely the sound ones. From a perusal of the basic ideas of stimulus-response theory, for example, it would scarcely seem possible on a commonsense basis to develop a set of concepts and ideas rich enough to account for complex behavior. I suspect that this commonsense viewpoint is deeply a part of the view of those cognitive psychologists who are so skeptical about the possibility of an adequate development of stimulus-response theory. It is also characteristic of most cognitive psychologists who express this viewpoint that they themselves do not have any extensive mathematical experience with a complex deductive elaboration of a few simple concepts. My reservation about the correctness of the commonsense viewpoint emanates exactly from this way of looking at the problem. There exists in mathematics itself a beautiful counterexample to the commonsense viewpoint, and I would like to expand upon this example.

If one had approached Euler or Lagrange, or any of the other prominent mathematicians in the eighteenth century, and had said that all mathematical notions could be defined just in terms of the simple notion of set membership, just in terms of an object being a member of a set, I am sure the response would have been that an idea as simple as that of set membership could not possibly generate the rich complexity of mathematical analysis and geometry characteristic of the best in eighteen-century mathematics. The commonsense response among mathematicians surely would have been that nothing of the sort was possible. Yet one of the great triumphs from a conceptual standpoint in mathematics, especially the foundations of mathematics over the last hundred years, has been to show precisely how such a reduction could be made in completely explicit and rigorous terms. It is, it seems to me, one of the great intellectual surprises about the structure of mathematics that all of the standard mathematical notions can be reduced by explicit definition just to the simple concept of set membership. The elaborate and complicated chain of reduction, far from obvious to naive intuition, is a warning to social scientists who want readily to dispense with theories because of their presumed lack of adequate complexity.

It is apparent, of course, that my own sentiments lie for pushing as far as possible the reduction of complex ideas to simple ones. I shall have more to say about this in my second lecture, but there is another critical remark of a purely mathematical sort that bears upon this argument within the social sciences. One of the great things we have learned in mathematics during the nineteenth and twentieth centuries is that negative arguments can be made in as powerful and precise a way as positive arguments. From a general historical standpoint, the understanding of how such negative arguments can be put together is one of the most important accomplishments of modern mathematics. So, if we want to be very tough-minded with cognitive psychologists, for example, who maintain that certain complex ideas <u>cannot</u> be explained by the simple ideas of stimulus, response, and reinforcement, for instance, we can ask them for a well-defined negative proof that such an explanation is not possible. A classic example in mathematics of such a negative argument is the proof that the three famous problems of the Greeks can indeed not be solved by straightedge and compass. The most familiar of the three is the famous problem of trisecting an angle. However, even in the case of this familiar example, the proof is by no means trivial, and I would say the same of any of the claims of cognitive psychologists. The proofs that would be needed to convert negative dogmas about stimulus-response theory into negative proofs are not obvious. I hope to show on Wednesday that the problem is difficult, and it is far from clear that it has a definite negative solution.

Nevertheless, I do not want to end this discussion of the adequacy of the complexity of theories on the note that I do think the simpler theories that have been current necessarily are going to be sufficient to the task. I do wish to emphasize the openness of the problem. What is most discouraging in my own view is the lack of well-defined alternatives. It is not easy to create substantial theories, and the number of well worked out alternatives is small. Finally, let me mention that there have already been one or two very satisfactory negative proofs in the social sciences. One good example is Arrow's famous proof of the impossibility of having a social-decision procedure satisfying certain intuitively attractive postulates. Another example is Milnor's negative proof that there is no strategy in games against nature satisfying all of the intuitive postulates that have been advanced by people as desirable.

As a seventh problem, I mention the issue of mathematical rigor versus mathematical power. It is probably the case that contemporary mathematical economics is written with a considerably higher degree of mathematical rigor than contemporary mathematical physics. There is no doubt that among the social sciences the mathematical rigor and sophistication of mathematical economics are greater than those of any other discipline. On the other hand, rigor in mathematical economics seems to be a substitute for confrontation with data. There is great concern for proving general theorems that everyone will agree are valid, and yet these theorems do not fall together into a body of theory that may be put to the acid test of confrontation with the empirical world. It is easy to show that much of the work that has been judged most important and most profound in mathematical economics has had little if any detailed contact with systematic empirical data. Nevertheless, I do sense a change in attitude and an increasing concern for questions of data among the younger mathematical economists of my acquaintance. There is of course the long and distinguished tradition of econometrics in economics, and much distinguished work in the statistical problems of data analysis. In mathematical psychology at present there is little concern for rigor and much greater concern for finding constructive answers, but the bag of mathematical tools used by the current generation of mathematical psychologists is relatively simple. Because there is a very strong experimental tradition in psychology, I would predict that psychology will develop like physics with an emphasis on constructive mathematical methods for obtaining answers with minimal concern for rigor. Yet it is all too easy to fall into a slogan of rigor versus power. It was not really until this century that there was a strong divergence between mathematics and physics. In the long term I would consider it a mistake to encourage a continued separation and drifting apart of mathematics and the mathematically oriented sciences. With the advent of computers, which I discuss in more detail below, it is very possible that many of the previous questions of rigor and power in mathematics will no longer seem relevant and will be replaced by a new set of problems that have not yet been explicitly formulated.

In fact, this mention of computers takes me to the eighth problem area, the assessment of the future role of computers in the social sciences and their impact on mathematical developments. A number of people have expressed the view that just as classical mathematical analysis was the natural tool of classical physics, so mathematical logic would be the natural mathematical tool of computers, and secondarily, of the analysis of intellectual processing in human beings. Nevertheless, certainly to date, the deep application of mathematical logic to computers or to the cognitive processing of humans has been disappointing. We do not have a body of mathematical ideas applicable to actual computers in a fashion that is at all comparable to the depth of classical analysis as applied to physics in the seventeenth, eighteenth, and nineteenth centuries. On the other hand, the discipline is extremely young and the theoretical developments scarcely are underway. From an entirely different standpoint, it is true that because of large-scale computers, problems are being investigated in terms of large bodies of data which simply would not have been explored a decade ago.

In talking about the impact of computers in the social sciences, it is important to distinguish between the two kinds of impact already mentioned. One is the use of computers for large-scale numerical analysis to study bodies of data that would have been impossible in the past. I am skeptical about some of the endless correlation matrices that have been looked at in the more purely empirical parts of psychology and sociology. This use will not have a significant impact unless there are accompanying theoretical developments to extend the analysis beyond rather superficial descriptive statistics. But I am not pessimistic about this direction of development. The existence of the capacity, even the relatively low-level mathematical sophistication required for straight statistical and numerical work, has already had an impact in the more mathematically naive parts of the social sciences.

The other kind of impact of computers seems to be more fundamental and more significant for the future. This is the use of computers to provide models of information processing, to investigate by simulation or numerical analysis the effects of variations in parameters of well formulated theories, and to realize optimization processes, whether in teaching and learning or in the organization of the economy. The range of these latter applications all require a deep use of mathematical ideas, some of which are just in the process of development in many parts of the social sciences. The current generation of graduate students is coming to feel as much at home with the computer as their peers in the physical sciences. I would hazard a guess that because of the enormous impact computers will have in the social sciences, in another couple of decades there will be little, if any, difference between the mathematical training and outlook of social scientists and physical scientists.

Finally, as the ninth problem area I mention real-world predictions versus laboratory predictions. Which criterion is used for evaluation makes an enormous difference in judging the success of a scientific discipline. It seems to me that a central problem for most of us working in the social sciences is that the criterion applied is that of predicting actual behavior. We are asked to analyze not simple paradigms of behavior, but actual behavior. We are asked to deal not with simple paradigms of language behavior, but with the actual first-language acquisition of a child. We are asked to deal not with simple paradigms of economic behavior, but with the actual development of economic aspects of our society. We are asked to deal not with simple paradigms of social behavior, but with the actual course of development of tensions and conflicts in the inner city. And so it goes throughout the other disciplines. This has not been the history of the physical sciences. Even today, for example, physicists would be hard pressed to satisfy a tough-minded criterion for predicting the actual weather or other secular physical processes like those of geology and geophysics. A recent much discussed example is the total inadequacy of geophysics to predict with any success the occurrence of earthquakes. But, because physicists cannot predict the weather or earthquakes, we do not judge that their science is unsuccessful. It seems important for us in the social sciences to recognize that we are working for a sterner master than are our colleagues in the physical sciences.

I think we want to satisfy this sterner criterion. We want to be able to meet the challenge of dealing with the course of developments in our actual society and in the behavior of its individual members. But it is important for us to keep in mind that a complete account of this actual behavior, whether by social groups or by the individual, will as in the case of the weather escape us. Yet we should not be disgruntled with our science because we are not able thoroughly to master all aspects of the behavior we intend to study.

(Continued from inside front cover)

91 P. Suppes. Information processing and choice behavior. January 31, 1966.
92 G. Groen and R. C. Atkinson. Models for optimizing the learning process. February 11, 1966. (Psychol. Bulletin, 1966, 66, 309-320)
93 R. C. Atkinson and D. Hansen. Computer-assisted instruction in initial reading: Stanford project. March 17, 1966. (Reading Research Quarterly, 1966, 2, 5-25)
94 P. Suppes. Probabilistic inference and the concept of total evidence. March 23, 1966. (In J. Hintikka and P. Suppes (Eds.), Aspects of Inductive Logic. Amsterdam: North-Holland Publishing Co., 1966. Pp. 49-65.
95 P. Suppes. The axiomatic method in high-school mathematics. April 12, 1966. (The Role of Axiomatics and Problem Solving in Mathematics. The Conference Board of the Mathematical Sciences, Washington, D. C. Ginn and Co., 1966. Pp. 69-76.
96 R. C. Atkinson, J. W. Breisford, and R. M. Shiffrin. Multi-process models for memory with applications to a continuous presentation task. April 13, 1966. (J. math. Psychol., 1967, 4, 277-300).
97 P. Suppes and E. Crothers. Some remarks on stimulus-response theories of language learning. June 12, 1966.
98 R. Bjork. All-or-none subprocesses in the learning of complex sequences. (J. math. Psychol., 1968, 1, 182-195).
99 E. Gammon. The statistical determination of linguistic units. July 1, 1966.
100 P. Suppes, L. Hyman, and M. Jerman. Linear structural models for response and latency performance in arithmetic. (In J. P. Hill (ed.), Minnesota Symposia on Child Psychology. Minneapolis, Minn.: 1967. Pp. 160-200).
101 J. L. Young. Effects of intervals between reinforcements and test trials in paired-associate learning. August 1, 1966.
102 H. A. Wilson. An investigation of linguistic unit size in memory processes. August 3, 1966.
103 J. T. Townsend. Choice behavior in a cued-recognition task. August 8, 1966.
104 W. H. Batchelder. A mathematical analysis of multi-level verbal learning. August 9, 1966.
105 H. A. Taylor. The observing response in a cued psychophysical task. August 10, 1966.
106 R. A. Bjork. Learning and short-term retention of paired associates in relation to specific sequences of interpresentation intervals. August 11, 1966.
107 R. C. Atkinson and R. M. Shiffrin. Some Two-process models for memory. September 30, 1966.
108 P. Suppes and C. Ihrke. Accelerated program in elementary-school mathematics--the third year. January 30, 1967.
109 P. Suppes and I. Rosenthal-Hill. Concept formation by kindergarten children in a card-sorting task. February 27, 1967.
110 R. C. Atkinson and R. M. Shiffrin. Human memory: a proposed system and its control processes. March 21, 1967.
111 Theodore S. Rodgers. Linguistic considerations in the design of the Stanford computer-based curriculum in initial reading. June 1, 1967.
112 Jack M. Knutson. Spelling drills using a computer-assisted instructional system. June 30, 1967.
113 R. C. Atkinson. Instruction in initial reading under computer control: the Stanford Project. July 14, 1967.
114 J. W. Brelsford, Jr. and R. C. Atkinson. Recall of paired-associates as a function of overt and covert rehearsal procedures. July 21, 1967.
115 J. H. Stelzer. Some results concerning subjective probability structures with semiorders. August 1, 1967
116 D. E. Rumelhart. The effects of interpresentation intervals on performance in a continuous paired-associate task. August 11, 1967.
117 E. J. Fishman, L. Keller, and R. E. Atkinson. Massed vs. distributed practice in computerized spelling drills. August 18, 1967.
118 G. J. Groen. An investigation of some counting algorithms for simple addition problems. August 21, 1967.
119 H. A. Wilson and R. C. Atkinson. Computer-based instruction in initial reading: a progress report on the Stanford Project. August 25, 1967.
120 F. S. Roberts and P. Suppes. Some problems in the geometry of visual perception. August 31, 1967. (Synthese, 1967, 17, 173-201)
121 D. Jamison. Bayesian decisions under total and partial ignorance. D. Jamison and J. Kozielecki. Subjective probabilities under total uncertainty. September 4, 1967.
122 R. C. Atkinson. Computerized instruction and the learning process. September 15, 1967.
123 W. K. Estes. Outline of a theory of punishment. October 1, 1967.
124 T. S. Rodgers. Measuring vocabulary difficulty: An analysis of item variables in learning Russian-English and Japanese-English vocabulary parts. December 18, 1967.
125 W. K. Estes. Reinforcement in human learning. December 20, 1967.
126 G. L. Wolford, D. L. Wessel, W. K. Estes. Further evidence concerning scanning and sampling assumptions of visual detection models. January 31, 1968.
127 R. C. Atkinson and R. M. Shiffrin. Some speculations on storage and retrieval processes in long-term memory. February 2, 1968.
128 John Holmgren. Visual detection with imperfect recognition. March 29, 1968.
129 Lucille B. Mlodnosky. The Frostig and the Bender Gestalt as predictors of reading achievement. April 12, 1968.
130 P. Suppes. Some theoretical models for mathematics learning. April 15, 1968. (Journal of Research and Development in Education, 1967, 1, 5-22)
131 G. M. Olson. Learning and retention in a continuous recognition task. May 15, 1968.
132 Ruth Norene Hartley. An investigation of list types and cues to facilitate initial reading vocabulary acquisition. May 29, 1968.
133 P. Suppes. Stimulus-response theory of finite automata. June 19, 1968.
134 N. Moler and P. Suppes. Quantifier-free axioms for constructive plane geometry. June 20, 1968. (In J. C. H. Gerretsen and F. Oort (Eds.), Compositio Mathematica. Vol. 20. Groningen, The Netherlands: Wolters-Noordhoff, 1968. Pp. 143-152.)
135 W. K. Estes and D. P. Horst. Latency as a function of number or response alternatives in paired-associate learning. July 1, 1968.
136 M. Schlag-Rey and P. Suppes. High-order dimensions in concept identification. July 2, 1968. (Psychom. Sci., 1968, 11, 141-142)
137 R. M. Shiffrin. Search and retrieval processes in long-term memory. August 15, 1968.
138 R. D. Freund, G. R. Loftus, and R.C. Atkinson. Applications of multiprocess models for memory to continuous recognition tasks. December 18, 1968.
139 R. C. Atkinson. Information delay in human learning. December 18, 1968.
140 R. C. Atkinson, J. E. Holmgren, and J. F. Juola. Processing time as influenced by the number of elements in the visual display. March 14, 1969.
141 P. Suppes, E. F. Loftus, M. Jerman. Problem-solving on a computer-based teletype. March 25, 1969.
142 P. Suppes and Mona Morningstar. Evaluation of three computer-assisted instruction programs. May 2, 1969.
143 P. Suppes. On the problems of using mathematics in the development of the social sciences. May 12, 1969.