Book Reviews

The Prospects for Social Experimentation

Computers, System Science, and Evolving Society. The Challenge of Man-Machine Digital Systems. HAROLD SACKMAN. Wiley, New York, 1967. xviii + 638 pp., illus. $14.50.

Each culture seems to have to go through its own muddle concerning the planning of social systems. One age is concerned with the role of reason, another with the democratic base. What seems to characterize the muddle of 20th-century American culture is the use of science in the design of societies. Although the invention of pure science seems to have had little to do with how men should seek the good life, the invention of applied science is altogether concerned with this matter. Applied science tries to adapt the procedures of observation, reason, test, and verification—that is, "experiment"—to the real problems of human society.

The philosophy indigenous to America called pragmatism attempted to create a sound basis for applied experimentation. Charles S. Peirce, William James, John Dewey, and Edgar Singer are four outstanding philosophers who tried to clear up the muddle. For muddle it was. It is all very well to speak generally of the need to "experiment" with educational processes, but when we come down to the specifics, what do we mean? The pure scientist insists on experimental controls, a strict calibration of instruments, the exclusion of the interplay of unwanted variables, a well-designed manipulation of the wanted variables, a precise statistical test, and so on. But in a democratic social system we simply can't control the variables in the manner required by pure science, for reasons too obvious to mention. Two choices seem apparent. One is to say that the method of experiment is not appropriate in the planning of society because its demands are too rigorous, and the other is to say that the method of experiment is broader than a specific technique and that the method is applicable to social systems. Pragmatism took the second choice, and tried to provide a comprehensive view of the nature of experiment, a view which encompasses both the rigorous, precise technique of pure science and the less rigorous, more purpose-oriented technique of applied social science.

The advent of the large digital computer came decades after the fundamental philosophical work of pragmatism had been accomplished. But the computer, in principle at least, seems to provide an opportunity for social-system experiment that no earlier pragmatist could dream of. Before 1950 we could only watch a few variables in very circumscribed situations. Now it is possible, by means of this new extension of our brains, to watch millions of variables and to perform meaningful analyses on huge clusters of data.

To bring pragmatism up to date, so to speak, Harold Sackman has written a penetrating review of the use of man-machine digital systems in social experimentation. In relatively easy stages, he explains some of the more recent technical developments in computer sciences: time sharing, real-time data processing, regenerative memory, simulation, and the like. All of these technologies are illustrated in terms of existing man-machine digital systems: SAGE (the North American air defense information system), CLASS (an experimental educational system), system training, and many others.

Sackman closes his book by developing a theory and philosophy of the social prospects of man-machine digital systems, in which he draws heavily on the writings of the pragmatists. Perhaps I should say that he outlines such a theory and philosophy, rather than develops it, for this frustrating section of the book presents an imposing list of desiderata of social experimentation, rather than a detailed account of how the desired features are to be implemented. The list includes "evolutionary experimentalism," "real time science," something alarmingly called "humanistic automation," which turns out to be merely the "elevation" of human intelligence through computers, and ends with a "theory of evolving hypotheses." The spirit is open-endedness with a determination to learn to the maximum of our resources. Probably Sackman hoped that the earlier illustrative and technical material would fill in the details of his theoretical specifications, but without further explanation this hope is not realized.

However, there is one item of considerable importance that does not seem to fall in his list, and indeed did not greatly concern the older pragmatists either. It is interesting to note that *experiment* and *peril* come from the same root. *Peril* originally meant a trial, and later a risky trial. Few could deny that social experiment is also a risky trial. But who is imperiled? Why, the inhabitants of the system, of course. But what should their role be? In the case of SAGE, their role was nil, as it is in all the "secret" information systems. In educational experiments, the students are the subjects not the experimenters. Sackman wisely points out that in the new philosophy of social experiment the experimenters must think of themselves as subjects, but he does not mention that all the subjects might think of themselves as experimenters.

The missing item in the pragmatist's theory is who shall design the experiment, who shall run it, who shall draw the conclusions, who shall implement the results. Does the pragmatist dare say that the answer is "those who are qualified"? Then who decides on qualifications? Scientists? Why should the rest of society trust the scientist? Because he knows more? But knowledge can be turned to evil as well as good.

The muddle of our age is implementation. There are those who believe that people of quality should study social problems and implement their findings in order to provide the "best" environment for all to live in. They want the experts to educate the uneducated, develop underdeveloped countries, employ the unemployed. The ultimate aim of the experts is to bring the underprivileged into a society where eventually all persons are qualified, all will share in designing better systems. Unfortunately, none of these modern do-gooders can tell us why he is especially qualified to do the job he has set out to do. Surely the privileged people outside of Watts need "retraining" as much as the people inside, because they have a distorted view of social reality.

It is not altogether ridiculous to say that social experimentation requires that there be no experts—or, if you wish, that everyone is an expert on some relevant aspect of planning. Indeed, one measure of performance of social planning might very well be the extent of contribution of all members of society. One of the most frustrating aspects of society today is the very little that most of us can contribute to the planning of social change; at best we have an occasional vote (often on undesirable alternatives) or an occasional letter to a representative. It would be a tragedy if all the good work of the earlier pragmatists produced a society ruled by "scientific" experts, no matter how elegant their experiments might be.

I'd feel a lot happier about the coming age of man-machine digital systems if I could more clearly understand a theory of implementation of the results of social experiment.

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Philosophical Inquiries

Delaware Seminar in the Foundations of Physics. MARIO BUNGE, Ed. Springer-Verlag, New York, 1967. xii + 193 pp. $9.50. Studies in the Foundations, Methodology and Philosophy of Science, vol. 1.

The most outstanding feature of this collection of essays is the diversity of their subject matter and approach. Three of the contributions (Bergmann, Bernays, Bunge) treat general questions about the nature of physical theories; two (Noll, Truesdell) present closely connected, and rather technical, discussions of the foundations of continuum mechanics; two (Grad, Jaynes) offer divergent accounts of the nature of statistical physics; two (Schiller, Margenau and Park) treat quantum mechanics, from quite different points of view; one (Post) offers a critical discussion of the physical content of the covariance principle; and another (Havas) presents a general discussion of problems arising in formulating general relativity theory.

Of the general discussions, the most substantial is Bunge's. He presents the outlines of a general account of the nature of "contemporary theoretical physics" by referring to a specific example of a physical theory—the two-component theory of the neutrino. He analyzes this theory into four components; its background (the other theories it presupposes); its form (the mathematical formalism of the theory); its content (the physical interpretations of this formalism); and finally, its evidential support. The general picture that emerges is one of a theory whose "essence" is a certain mathematical formalism which may be given various interpretations, or applied to various physical situations. The enterprise of giving such interpretations is discussed and an attempt made to distinguish this from the enterprise of producing empirical tests for the theory. A suggestion is made that the understanding of this enterprise—the semantical side of physical theorizing—may be advanced by employing the concepts and results of model theory. This appears to be a fruitful suggestion. It is regrettable that Bunge does not pursue it further in his article.

Two of the more specialized contributions are unified by a feature worth noting. Both the contribution of Jaynes and that of Margenau and Park deal extensively with the concept of subjective probability and its role in physics. It is astonishing that neither refers to *any* of the vast body of literature on subjective probability published in the last 30 years. The work of Ramsey, de Finetti, Savage, and Jeffrey has produced a theory of subjective probability quite distinct from, and superior to, the rudimentary efforts of Laplace, Keynes, and Jeffreys. In particular, the work of de Finetti has provided an account of how there can be wide intersubjective agreement about the values of certain subjective probabilities—one of the commonly mentioned difficulties in viewing physical probabilities as subjective.

This lacuna is particularly surprising and regrettable in the work of Jaynes—surprising because Jaynes is an advocate of a subjective interpretation of probabilities in physics, regrettable because it masks a very interesting problem in Jaynes's maximum-entropy principle. This is essentially a principle for choosing a prior probability distribution on the basis of data about average values of certain quantities. It is introduced by Jaynes as a postulate. From the point of view of modern subjective probability theories, an interesting question is this: Can this distribution be shown by the one on which *all* prior distributions converge in the limit of large numbers of certain kinds of experiments—the experiments which give us the data on "average values"?

The lacuna is not so surprising, but equally regrettable, in the contribution of Margenau and Park. The most glaring manifestation of it is the absence in their discussion of a distinction between the a priori or logical interpretation of probability (whose most outstanding recent proponent is Carnap) and the subjective interpretation in general. What the relation between these interpretations is may not be a closed question, but it is generally recognized not to be identity. The question of whether or not a subjective interpretation of probability in quantum mechanics is possible or fruitful is hardly illuminated by mounting objections to a quite different interpretation of probability. It is also regrettable that an alternative to the authors' account of the "seemingly miraculous" numerical agreement between subjective and objective probabilities—that of de Finetti—is not even mentioned.

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The Theory of Automata

Computation: Finite and Infinite Machines. MARVIN L. MINSKY. Prentice-Hall, Englewood Cliffs, N.J., 1967. xviii + 317 pp., illus. $12. Prentice-Hall Series in Automatic Computation.

A mathematical theory of computing machines (sometimes called automaton theory) was first presented by Alan Turing in 1936 (hence such machines are also often called Turing machines). Since that time there has been an extensive development of the field, although the models proposed are still very far from the current digital computers we find in so many places these days.

Minsky's *Computation: Finite and Infinite Machines* has finally brought order and sense to the mass of material that has appeared in the literature since Turing's first publication. The book has the character of simplicity that so often marks a great step forward; it uses only elementary mathematics (although in somewhat sophisti-